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PHILOSOPHICAL
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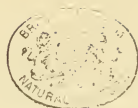
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PART I.

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MDCCXCVII.



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 enlarged, 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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APPENDIX.

Meteorological Journal kept at the Apartments of the Royal Society by Order of the President and Council.

THE PRESIDENT and COUNCIL of the ROYAL SOCIETY adjudged, for the year 1796, the Medal on Sir GODFREY COPLEY's Donation, to GEORGE ATWOOD, Esq. F. R. S. for his paper on the Construction and Analysis of geometrical Propositions, determining the Positions assumed by homogeneal Bodies which float freely, and at rest, on a fluid Surface ; and also determining the Stability of Ships, and other floating Bodies.

PHILOSOPHICAL
TRANSACTIONS.

- I. *The Croonian Lecture. In which some of the morbid Actions of the straight Muscles and Cornea of the Eye are explained, and their Treatment considered. By Everard Home, Esq. F. R. S.*

Read November 17, 1796.

IN two former Lectures, which I have had the honour of communicating to this learned Society, upon the subject of vision, I confined myself to the adjustment of the eye for seeing objects at different distances.

From the attention which in that investigation I necessarily paid to the natural actions of the muscles, and the structure of the cornea, I have been led to consider the effects which a diseased state of these parts will produce on the phænomena of vision. The observations I have made upon this subject, I now lay before this learned Society.

That I may be understood in giving an account of the diseases that arise from morbid actions of the straight muscles of the eye, it will be necessary to explain the effects which their natural actions are intended to produce; for these are

not confined to the separate, or combined actions of the muscles, but also vary according to the degrees of their contraction.

The first and most simple of these effects is that of moving the eyeballs in different directions.

The second is that of making the motions of the two eyes correspond with such a degree of accuracy, that when an object is viewed with both eyes, the impressions from the object shall be made on corresponding parts of the retina of each eye.

The third is that of compressing the eyeballs laterally, which renders the cornea more convex, and pushes forwards the crystalline lens, to adjust the eye to near distances.

Distinct vision with two eyes depends upon these different actions of the straight muscles; an imperfection in any one of them, as it renders the organ unfit to perform its functions, must be considered as a disease.

Three different diseases occur in practice, which appear to arise from morbid actions of the straight muscles. These are, an inability to see near objects distinctly; double vision; and squinting.

I shall consider each of these separately.

Of the inability to see near Objects distinctly.

As that action of the muscles which produces the adjustment of the eye to near objects, consists of the greatest degree of contraction usually exerted by them, it puts the fibres into a very uneasy state; which while in health they support with the utmost difficulty, and when affected by disease are unable to

sustain: under these last circumstances near objects cannot be seen at all without considerable pain, and never distinctly, the eye not remaining a sufficient time adjusted for that purpose. I cannot better explain the nature of this disease, than by giving an account of the symptoms which occurred in the following case.

A gentleman forty years of age, naturally short-sighted, of a delicate irritable habit from his infancy, never able to bear much bodily fatigue, being always soon tired by walking, or other exercises that required muscular exertion, had the following affection of his eyes.

His sight had been very perfect till he was nineteen years of age; at that time he resided in a part of the country where the ground consisted principally of white chalk, which produced an unpleasant glare; and his constant amusement both by day-light and candle-light was drawing, which he frequently pursued so far as to fatigue his eyes. While thus employed his complaints had their origin. The first symptoms were that of being unable to look long at any object without pain, and feeling uneasiness when exposed to strong light. The eyes to all appearance were free from disease, having no unusual redness, nor any purulent, or watery discharge. The plan that was first adopted for his relief consisted in lowering the system, both constitutionally and locally; but this treatment rendered him more irritable, and made his eyes rather worse than before; he therefore, after a trial of eight years, in different means of this kind, gave them entirely up. For the next five years, in which nothing was done to the eyes, the symptoms appeared to have been stationary; but at the end of that period, his mind suffering from an uncommon degree of anxiety, the

complaints in his eyes were evidently rendered worse; this effect, however, depended solely on the state of mind, for as soon as ever he recovered from his distress, the eyes also returned to their former state. In this condition I first saw him in the year 1795; and, at that time, his eyes had no external mark of disease, and were moved by the muscles in every direction without the smallest uneasiness. He could look at any thing that was at some distance, as the furniture in the room, the passing objects, &c. with perfect ease; but whenever he attempted to adjust the eyes to near objects, the effort gave so much pain, that although he succeeded in seeing them, he was almost immediately obliged to desist. Every attempt to write or read gave so much pain, that he became unable to do either; but as soon as the strain produced by such an effort was taken off, he was at ease. His disease therefore consisted in a want of power to adjust the eyes to near objects for a sufficient length of time to render them distinct, which of course incapacitated him from reading or writing. The cause of this disease appears to me to be a morbid affection of the straight muscles of the eyes, which allows them to perform all their intermediate contractions as usual, but not the extreme degrees of contraction without considerable pain.

As these symptoms have not, I believe, been before accounted for in this way, it may appear to many who have not seen similar affections of other muscles, that the present opinion is rather theoretical than practical; it will therefore be satisfactory to illustrate this disease in the muscles of the eye, by examples of the same kind of morbid action in other muscles, more within the reach of common observation. The following instances all refer to the muscles of the fore-arm and hand, employed in

actions with which every one is familiar, and show that these muscles are liable to be affected in the same manner as the muscles of the eye.

A gentleman, forty-six years of age, naturally of an irritable habit, which had been much increased by a long residence in the East Indies, was, about eight years ago, in a situation of great responsibility in that country. He was much engaged in writing, and previous to the sailing of a vessel for England, had, with a view to finish some dispatches of importance, written incessantly for a great many hours; the immediate effects of this exertion were simply fatigue, and stiffness in the muscles; but when he again attempted to employ the muscles in that action, he felt a nervous pain in the fore-arm, which was so severe as to oblige him to desist. This pain gave him considerable alarm, from the notion of its being of a paralytic nature, and many attempts were made to remove it. Recourse was had to electricity, and several other stimulating applications; but these always aggravated the symptoms, and they still continue. The circumstance in this case which is peculiarly applicable to my present purpose is, that the pain is only felt in the act of writing, the common motions of the fingers and thumb not giving the smallest uneasiness.

A gentleman about forty-six years of age, of a very irritable constitution, who had been in the habit of dealing cards for whole evenings together, was engaged in this employment one night for six hours; the weather was very warm, and he walked home in a state of perspiration, and went to bed. The window of his apartment, which faced the north, and was directly opposite to the foot of the bed, had been left open; the bed curtains were also undrawn. In the course of the night there was

a sudden change in the weather from hot to cold, and the wind having shifted to the north, blew directly upon his right arm, which was accidentally exposed. In the morning when he awoke his arm was in a very uneasy state. This however went off; but there was a pain in the muscles situated between the thumb and fore finger, and those of the fore-arm, which continued, and gave him great uneasiness. As it was supposed to be paralytic, blisters were applied to the origin of the nerves at the shoulder, and a visit to Bath was agreed upon as a necessary measure. The effects of the blister rather increased the complaint, which raised a doubt about its nature; and I found, upon a careful investigation, that particular muscles only were affected, which suggested an inquiry into the use that had been made of them. This inquiry led to a discovery of the real nature of the complaint, as only those muscles used in dealing cards were particularly affected. They were not in pain while at rest, but were unable to bear the least action without considerable uneasiness. This was greater at some times, than others; and although a year has now elapsed since the complaint came on, it is not entirely removed.

One of the principal tavern keepers in London was rendered very uneasy by a pain in the fore-arm, close to the elbow, which at times was very severe. Upon examining the parts, the pain was evidently not in the joint, but appeared to arise from an affection of the supinator brevis muscle, as the motion of that muscle gave pain. This I stated to him, but told him I was at a loss to find out in what way that part could have been injured; this was readily cleared up, when he informed me that the greatest pain he felt was in drawing claret corks, which he did with a jerk or sudden motion of the arm, and it was immediately after an

exertion of this kind that he had first felt the complaint. It was clear from this account that this particular muscle had been strained, and was rendered unfit to bear any violent action.

These cases will be sufficient to explain that a muscle, or set of muscles, may be unable to perform those actions which require the greatest exertion, although capable of performing all the others.

If then we consider the disease which causes the inability to see near objects as a strain upon the muscles, and compare it with the same disease in other muscles, there will be no difficulty in accounting for the bad effects produced by every thing that irritates, or weakens the parts themselves, or the general habit: it will follow, that such a mode of practice should be laid aside, and those means adopted by which the parts can be soothed in their sensations, and quieted and strengthened in their actions, since in that way only the muscular fibres can possibly recover their tone.

Of double Vision.

Many opinions have been advanced to account for the single appearance of objects when seen by both eyes.

Dr. REID of Glasgow, who has taken much pains on this subject, has treated it with ingenuity and a great deal of knowledge; and the opinion he has advanced, of objects appearing single when the impressions from the object are made upon parts of the retina of the two eyes which correspond with each other, and double whenever that is not the case, is very strongly confirmed by the following observations upon double vision.

There are two circumstances under which double vision

takes place; one where the muscles of the eye do not correspond in their action, and therefore the two eyes do not bear equally upon the object; the other, where some change has taken place in the refracting media of one eye which prevents the pencils of light from impressing the corresponding parts of the retina of both eyes. Instances of double vision produced by these two modes have fallen under my notice.

It has been long ascertained by experiments, that when the eyes are not turned equally towards an object, it appears double, and the disease in the muscles which produces this effect is the subject which I now mean to consider. It will, at the same time, be proper to distinguish this kind of double vision from that which is produced by a change in the refracting media of the eye; and this will be best done by explaining the nature of those changes in consequence of which it occurs.

When one eye has had the crystalline lens extracted, the other remaining perfect, objects seen by both eyes will appear double.

This is a fact which was noticed in a former lecture, in treating of the adjustment of the eye. At first it appeared difficult to account for the double vision, particularly as the two images were entirely separate from each other. It could not arise from the absence of the lens, as that would not alter the situation of the images on the retina; and the two images being of different dimensions on similar parts of the retina, would appear to be one before the other. As the operation of extracting the lens in no respect affects the muscles of the eye, the action of the muscles would be the same as before, and therefore could not contribute to produce this effect.

The double vision in this instance appears to arise from the

cornea of the eye which had undergone the operation being rendered flatter than the other, and giving a different direction to the rays of light, so as to form an image on a part of the retina not corresponding with the part impressed in the other eye.

If the crystalline lens be extracted from both eyes, and the person applies a convex glass to one eye only, and looks at an object, it will appear double; but if the convex glass is moved in different directions before the cornea, there will be found one situation in which it makes the object single. In this instance the corneas and muscles of the two eyes are under exactly the same circumstances; and when the centre of the convex glass is directly in the axis of vision, the image on the retina of that eye is formed on parts that correspond with those impressed in the other; but whenever the centre of the convex glass is out of the axis of vision this does not take place, and the object appears double.

The experiments of which these observations are the result, were made upon the eyes of a lady who had lost the sight of both, by opacities in the crystalline lenses; but by submitting to have the lenses extracted recovered her sight, and had afterwards an uncommon degree of distinct vision; which made her a very favourable subject for experiments of this kind.

Having explained the two different modes by which double vision may take place in consequence of operations that render the refracting media of the eye imperfect, I shall now consider it when produced by a morbid action of the muscles.

Several cases of this kind have come within my own knowledge, and I am induced to dwell upon the subject, because some of them had been considered as arising from a defect in

the organ, and erroneously treated. The fact has been long established by philosophers that a defect in the muscles may produce such a disease, but as other causes may likewise do the same, I believe that such a defect has not been practically considered, as one of the diseases of the eye; certainly not as a very common one, which undoubtedly it will be found.

The first case of this kind which led me to pay attention to the subject, was that of a friend, a lieutenant colonel of engineers, who was in perfect health, shooting moor-game upon his own estate in Scotland. He was very much surprised towards the evening of a fatiguing day's sport, to find all at once that every thing appeared double; his gun, his horse, and the road, were all double. This appearance distressed him exceedingly, and he became alarmed lest he should not find his way home; in this, however, he succeeded by giving the reins to his horse.

After a night's rest the double vision was very much gone off; and in two or three days he went again to the moors, when his complaint returned in a more violent degree. He went to Edinburgh for the benefit of medical advice. The disease was referred to the eye itself, and treated accordingly; the head was shaved, blistered, and bled with leeches. He was put under a course of mercury, and kept upon a very spare diet. This plan was found to aggravate the symptoms; he therefore, after giving it a sufficient trial, returned home in despair, and shut himself up in his own house. He gradually left off all medicine, and lived as usual. His sight was during the whole time perfectly clear, and at the same time near objects appeared single; at three yards they became double, and by increasing the distance they separated further from each

other. When he looked at an object, it was perceived by a by-stander, that the two eyes were not equally directed to it. The complaint was most violent in the morning, and became better after dinner, when he had drank a few glasses of wine. It continued for nearly a twelvemonth, and gradually went off.

The above account of the disease was given to me by the patient himself, who is an intelligent man, very soon after his recovery. It was considered as a curious disease, and I had several conversations with Mr. RAMSDEN respecting it. The more we considered it, the more we were convinced that the disease had been entirely in the muscles; and this I explained to the patient at the time as my opinion.

It is now about eight years ago, and the gentleman has had no return of the disease; but for two or three years past has lost in a great measure the use of his lower extremities, being unable to walk alone.

Some time after the recovery of this gentleman, a house-painter, who had worked a good deal in white lead, was admitted a patient in St. George's Hospital, on account of a fever, attended with a violent headach. Upon recovering from the fever, he was very much distressed at seeing every thing double; and as the fever was entirely gone, he was put under my care for this affection of his eyes. Upon an inquiry into his complaints, I found them exactly to correspond with the case I have just described, and therefore treated them as arising entirely from an affection of the muscles. I bound up one eye, and left the other open; he now saw objects single, and very distinctly, but looking at them gave him pain in the eye, and brought on headach. This led me to believe that I had erro-

neously tied up the sound eye; the bandage was therefore removed to the other eye, and that which had been bound up was left open. He now saw objects without pain, or the smallest uneasiness. He was thus kept with one eye confined for a week, after which the bandage was laid aside; the disease proved to be entirely gone, nor did it return in the smallest degree while he remained in the hospital. Rest alone had been sufficient to allow the muscles to recover their strength, and thus produced a cure.

A repetition of cases, I am very sensible, is not the most pleasing mode of conveying information, except to medical men; I have therefore selected those only, which are absolutely necessary to explain the different phænomena of the diseased states of the eye at present under consideration. The cases brought forward with this view, are rather to be looked upon as the detail of so many experiments made in the investigation of the diseases, than as histories of particular patients.

When muscles are strained or over fatigued, to put them in an easy state, and confine them from motion, is the first object of attention; and this practice is no less applicable to the muscles of the eye, than to those of other parts.

Of Squinting.

Whenever the motions of the two eyes differ from one another, whether in a less degree, so as to produce double vision, or in a greater, turning one eye entirely from the object, the disease has been called squinting. What I mean at present to consider under this head is, where the deviation of one of the eyes from the axis of vision is greater than that by which ob-

jects are made to appear double; so that in this view, double vision is an intermediate state between single vision with both eyes, and squinting. Squinting has been very generally believed to arise entirely from an inability in the muscles to direct the eye properly to the object. There is, however, probably no original defect in the muscles; certainly none sufficient to sanction such an opinion; since the muscles of a squinting eye have the power of giving it any direction, but cannot do it without some degree of effort. The defect, therefore, appears to be principally in the eye itself, which is too imperfect to assist the other in producing distinct vision. From this imperfection, the muscles have not the same guide to direct them as those of the other eye; and, therefore, although perfectly formed, cannot make their actions exactly correspond with them.

In a squinting person, both eyes certainly do not see the object looked at. This is evident to a by-stander, who is able to determine, that the direction of one of the eyes differs so much from that of the other, that it is impossible for the rays of light from any object to fall upon the retinas of both; and, therefore, that one eye does not see the object.

The same thing may be proved in another way; for since a small deviation in the direction of either eye from the axis of vision, produces double vision, any greater deviation must have the same effect, only increasing the distance between the two images, till it becomes so great that one eye only is directed to the object. In squinting there is evidently a greater deviation from the axis of vision than in double vision, and the object does not appear double; it is therefore not seen by both eyes.

The circumstance of those who squint having an imperfect eye, is corroborated by all the well authenticated observations

which have been made upon persons who have a confirmed squint, which all agree in stating, that one of the eyes is too imperfect to see distinctly.

From these observations, it would be natural to suppose that the loss of sight in one eye, should produce the appearance of squinting, which is by no means the case; for when that happens, the motions of the two eyes continue to correspond, although not exactly; but the deviation is not equal to that which is met with in squinting; it is nearer to that which occurs in double vision.

The reason why the imperfect eye of a squinting person is directed from the object, while a blind one in its motions follows the other, is, probably, that the indistinct vision of the imperfect eye prevents the muscles from directing it to the object with the same accuracy as those of the other do; this small deviation from the axis of vision renders the object double, and interferes with the vision of the perfect eye; and it is in the effort to get rid of the confused image that the muscles acquire a habit of neglecting to use the imperfect eye. It may also happen, when the eye is so imperfect as not to receive a correct image of any object, that it may have been neglected from the beginning. Distinct vision being at once obtained by the perfect eye, the end is answered, and the mind is never afterwards led to employ the other.

The direction the eye takes under either of these circumstances is inwards, towards the nose, the adductor muscle being stronger, shorter, and its course more in a straight line, than any of the other muscles of the eye.

That the eye, when not accurately directed to the object, produces confused vision, and is for that reason turned away,

appears to be confirmed by the case of a patient, from whom I had extracted the crystalline lens. This man, at first, saw objects double, in a manner which extremely distressed him; but, after some months, acquired the habit of neglecting to employ the imperfect eye, and no longer found any inconvenience.

The different degrees of squinting appear to be in proportion to the imperfection in the vision of the eye, and, in some instances, the person is capable of seeing distant objects with both eyes, and only squints when looking at near ones. The following case is of this kind.

A young lady, twenty-three years of age, has been observed to squint from her infancy; this has not been considered by her friends as the consequence of any defect in her eyes, but as arising from the cradle in which she lay having been so situated, with respect to the light, as to attract her notice in one particular direction, so much as to occasion a cast in one eye. Her eyes are apparently both perfect; when she looks with attention at an object some yards distant she has no squint, but if her eyes are not engaged by any object, or a very near one, she squints to a considerable degree.

Upon being asked if she saw objects distinctly with both eyes, she said certainly, but that one was stronger than the other. To ascertain the truth of this, I covered the strong eye and gave her a book to read; to her astonishment, she found she could not distinguish a letter, or any other near object. More distant objects she could see, but not distinctly. When she looked at a bunch of small keys in the door of a bookcase, about twelve feet from her, she could see the bunch of keys, but could not tell how many there were.

To see how far the two eyes had the same focus, she was desired to look at an object in the field of a microscope, and it was found that she saw most distinctly with both eyes at the same focal distance, although the object was considerably more distinct to the perfect eye than to the other; so that the focuses of the two eyes were the same.

I desired her to cover the perfect eye, and endeavour to acquire an adjustment of the other to near objects, by practising the use of that alone. At first she was unable to see at all with the imperfect eye, but in some weeks she has improved so much as to be able to work at her needle with it; this she cannot do long at any one time, the eye being soon fatigued and requiring rest, though without giving pain. She is unable to read with the imperfect eye. These trials have only been made in the course of two months, for a few hours in the day, and her friends think that she squints less frequently than she did.

In this case it is probable that the imperfect eye never had acquired the power of adjustment to near objects; for as distinct vision seems necessary to direct the muscles in their actions, the perfect eye would require less practice to adjust itself than the other; and as soon as the near object became distinct to one eye, no information being conveyed to the mind of the failure in the other, all efforts to render its adjustment perfect would be at an end, and it would ever after be neglected, while the perfect eye was in use.

Squinting, according to these observations, appears to arise from the vision in one eye being obscure. It may, however, be acquired in degree by children who have the lenses of their eyes of different focuses; or have one eye less perfect in its

vision than the other, living constantly with those who do squint, and, by imitation, acquiring a habit of neglecting to use one eye.

The power of squinting voluntarily may also be acquired at any age. This we find to be true in persons who look much through telescopes; they are led to apply the mind entirely to one eye, not seeing at all with the other. In this case the neglected eye will at first, from habit, follow the other; but in time, if frequently neglected, may lose this restraint, and be moved in another direction. Some astronomers, whose eyes have been much used in this way, are said to be able to squint at pleasure.

From this view of squinting, it takes place under the three following circumstances: where one eye has only an indistinct vision; where both eyes are capable of seeing objects, but the one less perfect in itself than the other; and where the muscles of one eye have acquired from practice a power of moving it independently of the other.

Where squinting arises from an absolute imperfection in the eye there can be no cure.

Where it arises from weakness only in the sight of one eye, it may, in some instances, be got the better of; but to effect the cure there is only one mode, which is that of confining the person to the use of the weak eye by covering the other; in this way the muscles, from constant use, will become perfect in the habit of directing the eye upon the object, gain strength in that action, and acquire a power of adjusting the eye; when these are established in a sufficient degree, the other eye may be set at liberty. The time that will be necessary for the cure must depend upon the degree of weakness of the sight, and

the length of time the muscles have been left to themselves; for it is with difficulty they acquire an increased degree of action after having been long habituated to a more limited contraction.

Of the Nature of the Cornea, some of its Diseases, and Mode of Treatment.

The cornea of the eye, as the name implies, has been considered of a cuticular nature. Baron HALLER compares it to the nails in a soft state, and believes that in its regeneration it resembles the epidermis.

This opinion is founded upon its want of sensibility, and having no vessels which carry red blood; the appearance it puts on when preserved in spirits, which is exactly similar to the nails at their roots, probably confirmed this supposition.

As the cuticle is devoid of life, it is only under the influence of disease during its growth; once formed, it continues unchanged. The cornea, were it of the same nature, would be equally incapable of taking on new actions from disease, or any other cause; but we find, on the contrary, that it undergoes many changes, which exactly correspond with those which the living parts of an animal body go through when under the influence of disease, from which I am induced to consider it alive; and I find that many of the present teachers of anatomy are of the same opinion.

To prove that the cornea has life it is necessary, as a previous step, to shew, that being supplied with vessels which carry red blood, and having sensibility, are not essential to the possession of the living principle; for this purpose all that is required is to

demonstrate that there are living parts which have neither the one nor the other. Tendons and ligaments in a natural state are instances of this kind. That these parts are not supplied with red blood is obvious to the eye of a common observer; no illustration will therefore be required to substantiate that proof. That they are not endowed with sensibility was, I believe, first taught by the late Dr. WILLIAM HUNTER,* who published the following account of it.†

In a case where the last joint of the ring-finger had been torn off, half an inch of the tendon of the flexor muscle projected beyond the stump; this it was thought right to remove; and to ascertain whether it was possessed of sensibility, the following experiment was made: a piece of cord the thickness of the tendon was passed round the wrist and along the side of the finger, so as to project even with the end of the tendon; the man was then told to turn away his head, and tell which of the two were cut through; the tendon was divided, and the man declared it was the string, not having felt the smallest degree of pain.

This proof is satisfactory; but that the cornea is possessed of life, by no means rests upon any negative proofs; which I shall now endeavour to explain.

The cornea in its structure is made up of membranous laminae. One of these appears to be a portion of the tunica conjunctiva, but it is either so extremely thin, or so intimately connected with the lamina next to it, as not to admit of more than a very partial separation from it; another lamina, as I

* This doctrine was first taught by Dr. HUNTER, in the year 1746. HALLER made experiments proving the same thing in 1750.

† Medical Observ. and Inquir. Vol. IV. page 343.

have shewn in a former lecture, is a continuation of the tendons of the four straight muscles; but as both these laminæ have the same properties as the other parts of the cornea, and are not to be distinguished from them, they must be considered in every respect as a part of it.

The tunica conjunctiva and tendons, a continuation of which forms these anterior laminæ of the cornea, are allowed to be living parts, and the portions that make part of the cornea are not to be distinguished by their structure from the rest; we must therefore suppose them to be also composed of living parts.

When the cornea is wounded it unites, like other living parts, by the first intention. If the wound is made by a clean cutting instrument the cicatrix is small; but if by a blunt instrument it is larger, extending further into the neighbouring parts of the cornea, and a greater quantity of the coagulating lymph of the blood being required to procure the union.

Although the cornea, when divided in the operation for extracting the crystalline lens, commonly unites by the first intention, this union is in some cases attended with inflammation, which produces an opacity of the cornea; in other cases the inflammation exceeds the limits of adhesion, and the whole internal cavity of the eye proceeds to a state of suppuration. These stages of inflammation are only met with in parts possessed of life.

It is true, that an injury may be committed to the cornea, such as a small piece of metal sticking in it, which from the indolent nature of its substance, shall remain there for months without producing inflammation; but an irritation of a less violent kind upon the edge of the cornea, by which the tunica conjunctiva is also affected, will produce inflammation upon

that vascular membrane, which may extend itself upon the cornea; for it is impossible that the vessels of the cornea, which naturally carry only lymph or serum, can be made to carry red blood, unless the irritation extends to some neighbouring part supplied with red blood.

That vessels carrying red blood have been met with upon the cornea in a diseased state, is doubted by HALLER; he does not altogether deny it, but the assertion, he says, requires proof, as he is not satisfied with the authorities of PETIT and others whom he quotes upon that subject.

It is so common a thing in inflammations of the eye to have the branches of the arteries of the tunica conjunctiva continued upon the cornea, that every practical surgeon must have met with it. In some instances of this kind, which have come immediately under my own care, I have examined these vessels with a magnifying glass, and have seen distinctly small arteries from the tunica conjunctiva, uniting upon the cornea into a common trunk larger than any of the branches that supplied it, and this trunk has sent off other branches distributed over the cornea.

These vessels may, by some physiologists, be supposed to be continued upon the lamina of the tunica conjunctiva, which is spread over the cornea; this, however, is not the case, as they pass behind it, and therefore belong as much to the lamina under them as that which is over them; and, in many instances of disease, vessels carrying red blood are met with in the substance of the cornea still deeper seated. This has been seen by Professor RICHTER,* who says, he has divided a

* RICHTER *Med. Doctor. et Professor publicus Ordinarius Soc. Reg. Scient. Gotting. et Acad. Reg. Scient. Sueciæ Mem. in Novis Comment. Soc. Reg. Gotting. T. vi. ad annum 1775.*

thickened cornea, and the vessels in its substance have poured out red blood.

The cornea is not only capable of uniting by the first intention, inflaming, and suppurating, but when the inflammation is carried to a great height, a portion of its substance is sometimes removed by ulceration, and the ulcer so formed is filled up by coagulating lymph, which afterwards becomes cornea, acquiring the necessary property of transparency. This new formed part is weaker than the rest of the cornea, and commonly projects beyond it, forming one species of staphyloma; in the substance of the cornea, round the basis of the staphyloma, I have frequently seen vessels carrying red blood.

From the opinion of the cornea being devoid of life, the opacities which are found to take place on it have been considered apart from common surgery, and entrusted to the care of men who are supposed to have made the diseases of the eye their particular study.

According to this theory, the opacity was supposed to arise from a film of inanimate matter laid over the cornea, and upon that idea very acrid and irritating applications were employed with the view of scraping it off, or destroying it, as powdered glass, powdered sugar, &c. and such applications being of service, confirmed the opinion which gave rise to the practice.

Having shown that the cornea is possessed of life, I shall now point out the parts of the body it resembles in structure, and to which it bears the greatest analogy, both in its healthy actions, and those arising from disease; and endeavour, by comparing them, to establish some general principle which will explain the beneficial effects of irritating applications in cases of inflammation and opacity of the cornea.

The cornea, from some experiments and observations mentioned in a former lecture, appears to be similar in structure and use to the elastic ligaments. It has all the common properties of ligaments, those of elasticity and transparency being superadded.

Like other ligaments it can be divided into laminæ, in an healthy state has no vessels carrying red blood, and is devoid of sensibility; when divided it readily admits of union, when inflamed acquires a great degree of sensibility, is slow in its powers of resolution, and when the inflammation subsides, the coagulating lymph deposited in the adhesive stage of the inflammation remains, producing an opacity which it is afterwards found difficult to remove.

All ligamentous parts, of which I consider the cornea to be one, are weak in their vital powers; this arises from their having no vessels carrying red blood; when they inflame, which is a state of increased action, they therefore require a different mode of treatment from the other parts of the body, whose vital powers are strong, in consequence of being largely supplied with red blood.

The truly healthy inflammation requires an increased action in the parts affected; and if this, either from weakness or indolence, is not kept up, the inflammation does not go rapidly through its stages, but remains in a state between resolution and suppuration. In ligamentous structures the actions must therefore be roused and supported when under inflammation, to promote resolution, and prevent the parts from falling into an indolent diseased state. This is, however, attended with difficulty, and they too often become considerably thickened.

by a deposition of coagulating lymph during the adhesive state of inflammation, which in the cornea renders it opaque. The thickening of the parts remains after the inflammation is gone, and can only be removed by absorption, which is best effected by the application of very stimulating medicines.

Upon these principles all ligamentous structures require a treatment peculiar to themselves, which may be illustrated both in inflammations of joints and of the cornea of the eye; the applications made use of with the greatest advantage in both cases being of a very stimulating kind.

The advantages attending this mode of treating the cornea were, probably, discovered by accident; and when they were ascertained, it established itself as a very general practice. It must, however, in the hands of those who had no general principle to direct their practice, have been sometimes applied without benefit, and must sometimes have been injurious.

It is an extremely curious circumstance, and probably the most so that can be met with in the history of medicine, that a local application should have been discovered to be of service in a particular disease 2513 years ago, that the same application, or those of a similar kind, should have been in very general use ever since, and in all that time no rational principle on which such medicines produced their beneficial effects should have been ascertained. This appears, from the following account, to have been the case with respect to stimulating applications to the cornea in a diseased state, and can only be accounted for by a want of knowledge of the structure of the parts, which is an argument of uncommon weight in favour of the study of anatomy.

In the Apocrypha we find, in the book of Tobit*, a very circumstantial account of an opacity of the cornea successfully treated by stimulating applications. It is there stated as a miracle, but we have the authority of JEROME, a father of the church, who wrote in the fourth century, to say, “ the church reads the books of Tobit, &c. for examples of life and instruction of manners, but doth not establish any doctrine by them.” We shall therefore consider the account which is given in extracts from the book of Tobit in that view.

Tob. chap. vi. ver. 2.

“ When TOBIAS went down to wash himself in the river Tigris, a fish leaped out of the river and would have devoured him.

“ Ver. 4. The angel of the Lord told him to take out the gall, and put it up in safety.

“ Ver. 6. TOBIAS asked the angel what was the use of the gall.

“ Ver. 8. As for the gall (said the angel) it is good to anoint a man who hath whiteness in his eyes, and he shall be healed.”

Chap. xi. ver. 11.

“ TOBIAS took hold of his father, and strake of the gall in his father’s eyes, saying, be of good hope, my father.

“ Ver. 12. And when his eyes began to smart he rubbed them.

“ Ver. 13. And the whiteness pilled away from the corners

* TOBIT was of the tribe of Naphtali, in the city of Thisbe, in Upper Galilee; he was carried captive to Nineveh, after the extinction of the kingdom of Israel, by Enemassar, or Salmanassar, about the year of the world 3283.

GRAY’S Key to the Old Testament and Apocrypha, page 554.

“ of his eyes, and when he saw his son he fell upon his
“ neck.”*

In conversing with my friend Dr. RUSSELL on the manner in which the Arabians treat inflammations and opacities of the cornea, he very kindly favoured me with the following account.

“ Respecting the practice of the Arabians in disorders of the
“ eyes, I find nothing of consequence in my papers. An ocu-
“ list among them is a distinct profession; and the collyria they
“ apply are secret compositions, which pass hereditarily from
“ father to son. The Arabian writers give a number of recipes,
“ most of which are taken from GALEN and the Greek physi-
“ cians. One composition in AVICENNA contains the gall of a
“ crow, crane, partridge, goat, &c. At Aleppo, the gall of
“ the sheet fish, *Silurus Glanis* of LINN. was in particular re-
“ quest; but it should be remarked, that they always add to the
“ gall other ingredients, it being a material circumstance in
“ that country, that a recipe should consist of a multitude of
“ ingredients. What often struck me in their practice was the
“ successful application of sharp or acrid remedies, at a time
“ I should have been induced to make use of the mildest emol-
“ lient applications.”

* Since this paper was read before the Royal Society, my friend Dr. WELLS acquainted me with the following case, published in the Annual Register for the year 1768.

“ One of the Paris newspapers gives an account of an extraordinary cure effected by
“ the gall of a barbel, in a case of blindness, in substance as follows: A journeyman
“ watchmaker, named CENSIER, having heard that the gall of a barbel was the remedy
“ which TOBIAS employed to cure his father's blindness, resolved to try its effects on
“ the widow GERMAIN, his mother-in-law, whose eyes had for six months been af-
“ flicted with ulcers, and covered with a film, which rendered them totally blind:

From this account given by Dr. RUSSELL there can be no doubt of gall having continued in use, as an application to the eye among the eastern nations, from the time of TOBIT down to the present day.

I have in the course of the last three years made many trials of the effects of gall, as an application to the cornea in a diseased state. I have used it pure, and diluted; and compared its effects with those of the unguentum hydrargyri nitrati, and the solution of the argentum nitratum; and find in old cases of opacity it is, in some instances, the best application. The gall of quadrupeds, in these trials, gave more pain than the gall of fish. The painful sensation was very severe for an hour or two, and then went off. It is proper to observe, that the beneficial effects it produces appear to be in proportion to the local violence at the time of its application.

To enter further into the practical part of the treatment for removing opacities from the cornea, would be foreign to the pursuits of this learned Society, which I consider to be confined

“ CENSIER having obtained the gall of that fish, squeezed the liquor out of it into a phial, and in the evening he rubbed it with the end of a feather into his mother’s eyes.
 “ It gave her great pain for about half an hour, which abated by degrees, and her eyes watered very much: next morning she could not open them, the water as it were gluing her eyes up: he bathed them with pure water, and she began to see with the eye which had received the most liquor. He used the gall again in the evening; the inflammation dispersed, the white of her eyes became red, their colour returned by degrees, and her sight became strong. He repeated it a third time, with all the desired success. In short, she recovered her sight without any other remedy.
 “ The widow GERMAIN is in her fifty-third year. She had been pronounced blind by the surgeons of the Hôtel-Dieu: and her blindness and cure have been attested by order of the lieutenant general of police. She sees stronger and clearer now than before the accident.”

Annual Register, Vol. xi. page 143.

to the general principles of the different branches of science, and to collecting facts out of which new principles may be formed, or those already known better established.

The practice of applying very stimulating applications to the cornea has stood the test of twenty-five centuries, it can therefore require no support. The object of the present observations has been to explain the principle upon which the beneficial effects depend, a knowledge of which may serve as a guide to regulate our practice. It will guard us against using such medicines while the inflammatory action is increasing, it will lead us to adopt them the moment the inflammation appears to be at a stand, and not postpone this practice till an indolent unhealthy state takes place, which too often terminates in opacities no applications can afterwards remove.

II. *Observations on horizontal Refractions which affect the Appearance of terrestrial Objects, and the Dip, or Depression of the Horizon of the Sea.* By Joseph Huddart, Esq. F. R. S.

Read November 24, 1796.

THE variation and uncertainty of the dip, in different states of the air, taken at the same altitude above the level of the sea, was the occasion of my turning my thoughts to this subject; as it renders the latitude observed incorrect, by giving an erroneous zenith distance of a celestial object.

I have often observed that low lands and the extremity of head lands or points, forming an acute angle with the horizon of the sea, and viewed from a distance beyond it, appear elevated above it, with an open space between the land and the sea. The most remarkable instance of this appearance of the land I observed at Macao, for several days previous to a typhoon, in which the *Locko* lost her topmasts in Macao roads; the points of the islands and low lands appearing the highest, and the spaces between them and the sea the largest, I ever saw. I believe it arises, and is proportional to the evaporation going on from the sea; and in reflecting upon this phenomenon, I am convinced that those appearances must arise from refraction, and that instead of the density of the atmosphere increasing to the surface of the sea, it must decrease from some space above it; and that evaporation is the

principal cause which prevents the uniformity of density and refraction being continued, by the general law, down to the surface of the earth: and I am inclined to believe, though I mention it here as a conjecture, that the difference of specific gravity in the particles of the atmosphere may be a principal agent in evaporation; for the corpuscles of air, from their affinity with water, being combined at the surface of the fluid from expansion, form air specifically lighter than the drier atmosphere; and therefore float, or rise, from that principle, as steam from water; and in their rising (the surrounding corpuscles from the same cause imbibing a part of the moisture), become continually drier as they ascend, yet continue ascending until they become equally dense with the air.* However, these conjectures I shall leave, and proceed to the following observations upon refractions.

In the year 1793, when at Allonby, in Cumberland, I made some remarks on the appearance of the Abbey Head, in Gallo-way, which in distance from Allonby is about seven leagues; and from my window, at fifty feet above the level of the sea at that time of tide, I observed the appearance of the land about the Head as represented in Tab. I. fig. 1. There was a dry sand, $x y$, called Robin Rigg, between me and the Head, at the distance from my house of between three and four miles, over which I saw the horizon of the sea, $H O$; the sand at this time was about three or four feet above the level of the sea.

* Mr. HAMILTON, in his very curious Essay on the Ascent of Vapours, does not allow of this principle, even as an assistant; though by a remark (page 15) he takes notice of those appearances in the horizon of the sea, and says they arise from a strong or unusual degree of refraction; the contrary of which I hope to illustrate in the course of this paper.

The hummock *d* is a part of the head land, but appeared insulated or detached from the rest, and considerably elevated above the sea, with an open space between. I then came down about twenty-five feet, when I had the dry sand of Robin Rigg, *x y*, in the apparent horizon, and lost all that floating appearance seen from above, and the Abbey Head appeared every where distinct to the surface of the sand; this being in the afternoon, the wet or moisture on the sand would in a great measure be dried up. I have reason, therefore, to conclude that evaporation is the cause of a less refraction near the surface of the sea; and when so much so as to make an object appear elevated wholly above the horizon, (as at *d* in fig. 1.) there will from every point of this object issue two pencils of rays of light, which enter the eye of the observer; and that below the dotted line *AB* (parallel to the horizon of the sea *HO*), the objects on the land will appear inverted.

To explain this phænomenon, I shall propose the following theory, and compare it with the observations which I have made. Suppose *HO*, fig. 2. to represent the horizontal surface of the sea, and the parallel lines above it, the lamina or strata of corpuscles, which next the fluid are most expanded, or the rarest; and every lamina upwards increasing in density till it arrive at a maximum (and which I shall in future call the maximum of density) at the line *DC*, above which it again decreases in density *ad infinitum*.

Though this in reality may be the case, I do not wish to extend the meaning of the word density farther, than to be taken for the refractive power of the atmosphere; that is, a ray of light entering obliquely a denser lamina to be refracted towards a perpendicular to its surface; and in entering a rarer lamina,

the contrary; which laminæ being taken at infinitely small distances, the ray of light will form a curve, agreeable to the laws of dioptrics.

In order to establish this principle in horizontal refractions, I traced over various parts of this shore at different times, when those appearances seemed favourable, with a good telescope, and found objects sufficient to confirm it; though it be difficult at that distance of the land to get terrestrial objects well defined so near the horizon, as will afterwards appear.

One day observing the land elevated, and seeing a small vessel at about eight miles distance, I from my window directed my telescope to her, and thought her a fitter object than any other I had seen for the purpose of explaining the phænomena of these refractions. The telescope was forty feet above the level of the sea. The boat's mast about thirty-five feet, she being about twenty to thirty tons burthen. The barometer at 29,7 inches, and FAHRENHEIT'S thermometer at 54° .

The appearance of the vessel, as magnified in the telescope, was as represented in fig. 3, and from the mast head to the boom was well defined. I pretty distinctly saw the head and shoulders of the man at the helm; but the hull of the vessel was contracted, confused, and ill defined: the inverted image began to be well defined at the boom (for I could not clearly perceive the man at the helm inverted), and from the boom to the horizon of the sea the sails were well defined, and I could see a small opening above the horizon of the sea, in the angle made by the gaff and mast; and had the mast been shorter by ten feet (to the height of y), the whole would have been elevated above the horizon of the sea, and from y to d an open space. This drawing was taken from a sketch I took at the

time, and represents the proportion of the inverted to the erect object, as near as I could take it by the eye, the former being about two-thirds of the latter in height, and the same breadth respectively; though at one time during my observation, which I continued for about an hour, I thought the inverted nearly as tall as the erect object. The day was fine and clear, with a very light air of wind, and I found very little tremor or oscillation in viewing her through the telescope.

I have laid down fig. 4. for the explanation of the above phenomena, in which A represents the window I viewed B the vessel from; H O, the curved surface of the sea; C D parallel to H O, the height of the maximum of density of the atmosphere; the lines marked with the small letters *a a*, *b b*, *c c*, *d d*, the pencils of rays under their various refractions from the vessel to the eye, or object glass of the telescope.

The pencil of rays *a a*, from a point near the head of the mainsail, is wholly refracted in a curve convex upwards, being every where above the maximum of density; and the pencil of rays *d d*, which issues from the same point in the sail, and passes near the horizon of the sea at *x*, is convex upwards from the sail to W, where it passes the line of maximum of density, which is the point of inflection; there it becomes convex downwards, passing near the horizon at *x* to *y*, where it is again inflected, and becomes convex upwards from thence to the eye. The pencil of rays *b b*, from the end of the boom, passing nearly parallel to the horizon, and near the maximum of density, suffers very little deviation from a right line in the first part; but in ascending (from the curvature of the sea) will be convex upwards to the eye. The pencil of rays *c c*, from the same point in the boom, may have the small part to *c* convex upwards,

from c to z it will be convex downwards, and from z to the eye convex upwards.

From this investigation it appears, that two pencils of rays cannot pass from the same point, and enter the eye, from the law of refraction, except one pencil pass through a medium which the other has not entered; and therefore the maximum of density was below the boom, and could not exceed ten feet of height above the surface of the sea at the time these observations were made.

Respecting the hull of the vessel being confused, and ill defined in the telescope, as by fig. 3, it arises from the blending of the rays, from the different parts of the object, refracted through the two mediums; some parts of the hull appearing erect, and some inverted. Suppose the dotted line ii , fig. 4, an indefinite pencil of rays, passing from between the inverted and erect parts of the object, or the upper part of the hull of the vessel, to the eye, (for the lower part of the hull could not be observed): the objects cannot appear inverted, except the angles at the eye aAc and aAd , exceed the angle aAi ; for the intermediate space could only be contracted by the secondary pencils of rays. The lengths of the inverted, compared with the erect image of the sail, is as the sines of the angles at the eye aAi to iAd ; and the angle at the eye aAd , made by the two pencils of rays from the same point near the head of the sail, must be double the angle aAi , when the inverted image is as tall as the erect. In this case, the sines of the angles aAb , aAc , aAd , fig. 4, are proportional to the altitudes ab , ac , ad , in the magnified view of the vessel, fig. 3.

Under this consideration no inverted image of the sail will be formed, until the angle at the eye, made by the two refracted

pencils of rays aa and dd , exceed the angle made by aa , and bb , the apparent height of the sail of the vessel; for were those angles equal, the inverted sail would only be contracted into the parallel of altitude of the boom b , and render the appearance confused, as in the hull of the vessel.

Respecting the existence of two pencils of rays entering the eye from every point of an object not more elevated than a , or less than i , fig. 3, in this state of the atmosphere, I cannot bring a stronger proof than that of the strength of a light when the rays pass near the horizon of the sea, proved by the following observations.

Going down Channel about five years ago in the Trinity yacht, with several of the elder brethren, to inspect the light-houses, &c. I was told by some of the gentlemen, who had been on a former survey, that the lower light of Portland was not so strong as the upper light, at near distances, but that at greater distances it was much stronger. I suspected that this difference arose from the lower light being at or near the horizon of the sea, and mentioned it at the time; but afterwards had a good opportunity of making the observation. We passed the Bill of Portland in the evening, steering towards the Start, a fresh breeze from the northward and clear night; when we had run about five leagues from the lights, during which time the upper light was universally allowed to be the stronger, several gentlemen keeping watch to make observations thereon, the lower light, drawing near the horizon, suddenly shone with double lustre. Mr. STRACHAN, whose sight is weak, had for some time before lost sight of both lights, but could then clearly perceive the lower light. I then went aloft, (as well as others,) but before I got half mast up, the lower

light was weaker than the upper one; on coming down upon deck, I found it again as strong as before. We proceeded on, and soon lost the lower light from the deck; and upon drawing the upper light near the horizon, it like the former shone exceeding bright. I again went aloft, when it diminished in brightness; but from the mast head I could then see the lower light near the horizon as strong as before. This is in consequence of the double quantity of light entering the eye by the two pencils of rays from every point. To illustrate which, we compare the vessel, fig. 4, to a lighthouse built upon the shore, and A the place of the observer; and having brought down the light so low as to view it in the direction *aa*, another light would appear in the horizon at *x* from the pencil *dd*; and had the vessel been still enough to have observed it at this time with a good glass, I doubt not but the two images might have been distinctly seen: as the light dropped, (by increasing the distance) the two images would appear continually to approach each other, till blended with double light in one, and disappear at the altitude *i*, above the apparent horizon of the sea. But, as explained before, if the strength of evaporation did not separate by refraction the pencils *aa* and *dd* to a greater angle than double the angle that the lamps and reflectors appear under, the two images would be blended, and the strong appearance of light would be of shorter duration. The distance run from the lights, during the time each of the lights shone bright, would have been useful, but this did not occur at the time, nor have I had the like opportunity since. However, I recommend to the mariner to station people at different heights in looking out for a light, in order to get sight of it near the horizon, when it is always strongest.

Respecting the appearance of the Abbey Head before mentioned, fig. 1, the dotted line AB represents the limit, or the lowest points of the land that can be seen over the sea; for, as above stated, all the objects appearing below this line, are the land above it inverted; and where the land is low, as at d and m , it must appear elevated above the horizon of the sea.

In fig. 5. let HO represent the curve of the ocean, and d the extreme top of the mount visible at A by the help of refraction; the dotted pencil of rays cc passing from d to the eye in some part a little below the maximum of density, where inversion begins; therefore no land lower than this can be seen; for any pencil from a point in the land lower than this, must in the refraction have a contrary flexure in the curve, and therefore pass above the observer. Let AD be a tangent to the curve at A , then the object d will appear to be elevated by refraction to D ; also let Av be a tangent to the pencil Ax at A , then the angle DAx will appear to be an open space, or between D and the horizon of the sea. Suppose a star should appear very near and over the mount d , as at $*$, two pencils would issue from every point of it, and form a star below as well as above the hummock d . There are always confused or ill defined images of the objects at the height of the dotted line, fig. 1, above the level of the sea, as before mentioned; and instead of the points of d ending sharp in that line, they appear blunted, and the Abbey Head is frequently insulated at the neck m .

I have viewed, from an elevated situation, a point or head land at a distance beyond the horizon of the sea, forming, as in fig. 6. a straight line AB , making an acute angle BAO with the horizon of the sea. Seeing the extreme point blunted and elevated, I descended; and though in descending the horizon

cut the land higher, as at $H O$, $H O$, yet the point had always the same appearance as a , a , a , fig. 6, though the land is known to continue in the direction of the straight line $A B$ to beneath the horizon, or nearly so, as viewed from the height above.

If then from a low situation we view this head land through a telescope, the inclination of the surface $A B$ to the horizon being known to be a straight line, it will appear as in fig. 7. the dotted line (at the height of the point where a perpendicular $x y$ would touch the extreme of the land) being at the limit or lowest point of erect vision. And if a tangent to the curved appearance of the land $a b$, is drawn parallel to the inclined surface of the land $A B$, fig. 6, touching it at C , the point C will shew the height of the maximum of density, where the pencil of the rays of light, from thence to the eye, approach nearest the sea; for pencils of rays from this land, taken at small distances from C , will form parallel curves, nearly, through the refracting mediums, and C will be the point of greatest refraction; for above C as at B the refraction somewhat decreasing, will appear below the line $a b$, or the parallel to the surface of the land, and the refractions decrease below the point C ; for had they increased uniformly down to the surface of the sea, it would render the apparent angle of the point of land z more acute than the angle $C a O$, contrary to all observations.

Thus I have endeavoured to explain the phænomena of the distorted appearance of the land near the horizon of the sea, when the evaporation is great; and when at the least, I never found the land quite free from it when I used a telescope; and from thence infer, that we cannot have any expectation to find a true correction for the effect of terrestrial refraction, by tak-

ing any certain part of the contained arc; for the points z C B, fig. 7, will have various refractions, though they are at nearly the same distance from the observer. And if the observations are made wholly over land, if the ground rises to within a small distance of the rays of light in their passage from the object to the eye, as well as at the situation of the object and observer, the refractions will be subject to be influenced by the evaporation of rains, dews, &c. which is sufficiently proved by the observations of Colonel WILLIAMS, Captain MUDGE, and Mr. DALBY, Phil. Trans. 1795, p. 583.

The appearances mentioned by Colonel WILLIAMS, Captain MUDGE, and Mr. DALBY, (Phil. Trans. 1795, p. 586, 587,) cannot be demonstrated upon general principles, as they arise from evaporation producing partial refractions. In those general principles, it is supposed that the same lamina of density is every where at an equal distance from the surface of the sea, at least as far as the eye can reach a terrestrial object; but in the partial refractions, the lamina of the expanded or rarefied medium may be of various figures according to circumstances, which will refract according to the incidence of the rays, and affect the appearance of the land accordingly, which I have often seen to a surprising degree. But my principal view is to shew the uncertainty of the dip of the sea, and that the effect of evaporation tends to depress the apparent horizon at x , when the eye is not above the maximum of density; and from hence the difficulty of laying down any correct formula for these refractions, whilst the law of evaporation is so little understood, which indeed seems a task not easy to surmount. The effect indicated by the barometer and thermometer is insufficient: and should the hygrometer be improved to fix a

standard for moisture in the atmosphere, and shew the variations near the surface of the ocean, which certainly must be taken into the account, (evaporation going on quicker in a dry than a moist atmosphere,) the theory might still be incomplete for correcting the tables of the dip. I shall therefore conclude this paper, by shewing a method I used in practice, in order to obviate this error, in low latitudes.

When I was desirous to attain more accurately the latitude of any head land, &c. in sight, I frequently observed the angular distances of the sun's nearest limb from the horizons, upon the meridian both north and south, beginning a few minutes before noon, and taking alternately the observations each way, from the poop, or some convenient part of the ship, where the sun and the horizon both north and south were not intercepted; and having found the greatest and least distances from the respective horizons, which was at the sun's passing the meridian, and corrected both for refraction, by subtracting from the least, and adding to the greatest altitude, the quantity given by the table; and also having corrected for the error of the instrument, and the sun's semidiameter; the sum of these two angular distances, reduced as above, — 180° , is equal to double the dip, as by the following

EXAMPLE.

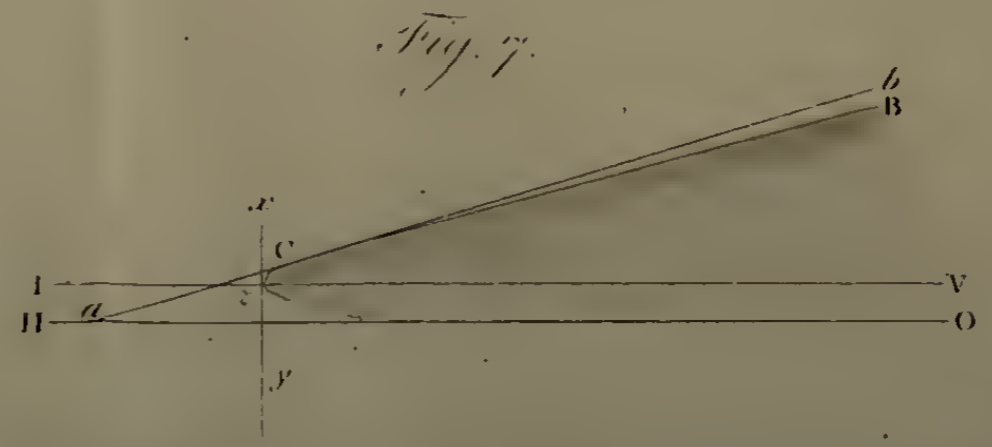
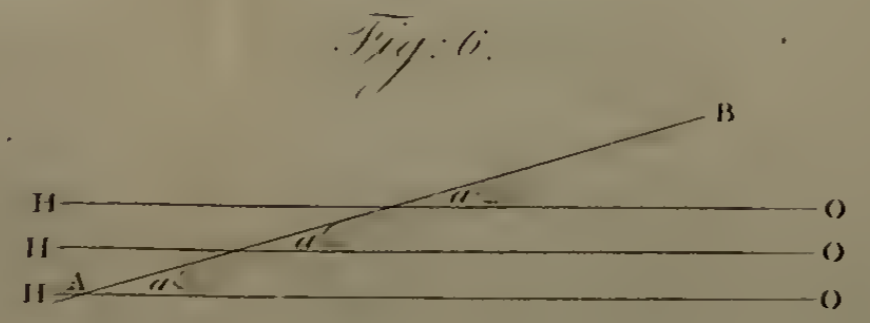
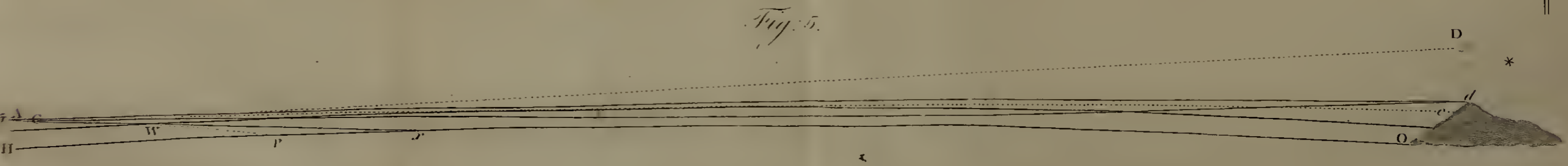
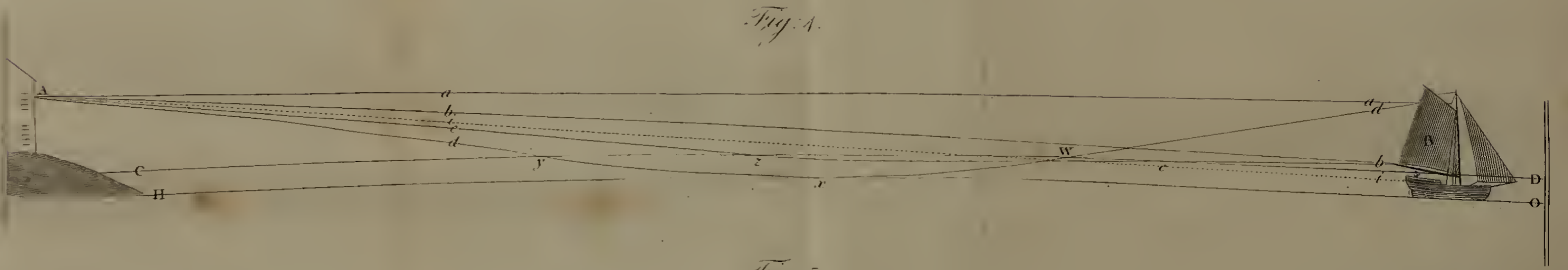
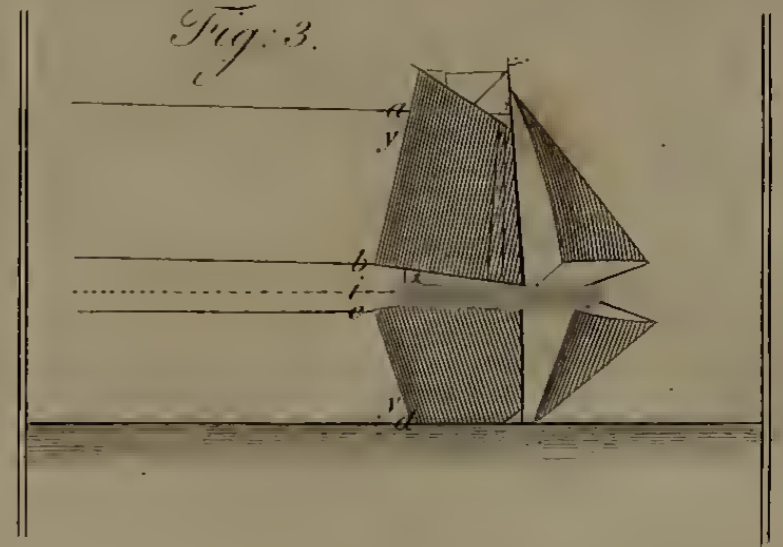
The sun's declination $4^{\circ} 32' 30''$ north, and its semidiameter $15' 58''$ took the following observation :

The meridian distance of the			
sun's nearest limb from the		South.	North.
horizon of the sea	-	$78^{\circ} 36' 30''$	$= 101^{\circ} 1' 20''$
Refraction <i>per</i> table	- -	$- 0 11$	$= + 0 11$
Distances corr. for refraction		<u>$= 78 36 19$</u>	<u>$= 101 1 31$</u>
Error of the sextant	- -	$+ 1 32$	$+ 1 32$
Sun's semidiameter	- -	$+ 15 58$	$+ 15 58$
		<u>$78 53 49$</u>	<u>$101 19 1$</u>
$\frac{1}{2}$ diff. or the dip found	- -	$- 6 25$	$78 53 49$
Altitude reduced		<u>$= 78 47 24$</u>	<u>$180 12 50$</u>
Zenith distance		<u>$= 11 12 36$</u>	<u>180</u>
			<u>Diff. $12 50$</u>
The sun's declination	N. =	<u>$4 32 30$</u>	$\frac{1}{2} = 6 25$
Latitude of the ship	N. =	<u>$15 45 06$</u>	Dip.

I regret that I cannot in this paper insert the dip which I have found in my observations; for I only retained the latitude of the ship determined thereby, as is usual at sea; I generally rejected the error of the instrument, the dip, and semidiameter, as they affect both observations with the same signs, and reduced the observation by the following method :

	South:	North.	
Sun's dist. as before	78° 36' 30"	101° 1' 20"	
Refraction - -	- 0 11	+ 0 11	
	<hr/>	<hr/>	
Dis. corr. for refraction	78 36 19	101 1 31	101° 1' 31"
		+ 78 36 19	
		<hr/>	
Sum of S. diam. dip, and		Sum 179 37 50	
refraction = $\frac{1}{2}$ diff.	+ 11 5	180	+ 11 5
	<hr/>		
	78 47 24	Diff. 22 10	
		$\frac{1}{2}$ 11 5	<hr/>
			101 12 36
	<hr/>		
	90		90
			<hr/>
The $\frac{1}{2}$ dist. as before =	11 12 36	$\frac{1}{2}$ D. =	11 12 36

It may be observed, that neither the dip, semidiameter, or index error, can affect the zenith distance of the sun's centre; and the refraction being small near the zenith, the result must be true if the angles are accurately taken; and it is only necessary to observe, that when the sum of the distances is less than 180° , the half difference must be added to the distances, as by the last reduction. There is a difficulty in making this observation when the sun passes the meridian very near the zenith, as the change in azimuth from east to west is too quick to allow sufficient time; nor can it be obtained by the sextant when the sun passes the meridian more than 30 degrees from the zenith; for I never could adjust the back observation of the HADLEY'S quadrant with sufficient accuracy to be depended upon.





III. *Recherches sur les principaux Problèmes de l'Astronomie Nautique. Par Don Josef de Mendoza y Rios, F. R. S. Communicated by Sir Joseph Banks, Bart. K. B. P. R. S.*

Read December 22, 1796.

DANS les Recherches suivantes, je me suis proposé de considérer les principaux problèmes de l'Astronomie Nautique d'une manière générale, pour établir des formules qui embrassent tous les cas, et dont on puisse déduire les différentes méthodes propres à les résoudre avec plus ou moins d'avantages. Elles sont divisées en deux Parties.

Dans la Première Partie j'ai compris ce qui regarde la détermination de la latitude du lieu du vaisseau par deux hauteurs du soleil ; ainsi que le calcul de l'angle horaire d'un astre par la hauteur observée, et celui de la hauteur par l'angle horaire.

Le sujet de la Seconde Partie est la réduction des distances de la lune au soleil, ou à une étoile, observées à la mer, pour déterminer la longitude. J'ai considéré séparément les solutions directes, et les méthodes d'approximation. Quant aux dernières, j'ai tâché aussi de donner des formules propres pour examiner et porter un jugement définitif sur tous les procédés de cette espèce dont on voudra prouver la fausseté ou la justesse, ou bien les degrés d'exactitude qu'ils comportent.

Dans ces Recherches, ainsi que dans un ouvrage* que j'ai composé, avec un grand nombre de tables pour faciliter les calculs de l'Astronomie Nautique, j'ai employé les sinus-verses en

* L'impression de cet ouvrage est déjà très avancée.

les envisageant sous certaines relations réciproques qui me paroissent susceptibles de plusieurs applications utiles. Avant d'entrer en matière, il est donc à-propos de les expliquer, et de faire connoître les expressions dont je me suis servi pour les désigner. Les voici, (en supposant, comme nous le ferons par la suite, le sinus total = 1)

$$\text{sinus-verse } A = 1 - \cos. A = 2 \sin. \frac{1}{2} A$$

$$\text{susinus-verse } A = 1 + \cos. A = \sin. v. (180^\circ - A) = 2 \cos. \frac{1}{2} A$$

$$\text{cosinus-verse } A = 1 - \sin. A = \sin. v. (90^\circ \sim A) =$$

$$\text{susin. v. } (90^\circ + A) = 2 \sin. \frac{1}{2} (90^\circ \sim A) =$$

$$2 \cos. \frac{1}{2} (90^\circ + A)$$

$$\text{sucosinus-verse } A = 1 + \sin. A = \sin. v. (90^\circ + A) =$$

$$\text{susin. v. } (90^\circ \sim A) = 2 \sin. \frac{1}{2} (90^\circ + A) =$$

$$2 \cos. \frac{1}{2} (90^\circ \sim A)$$

PREMIÈRE PARTIE.

Trouver la Latitude du Vaisseau par deux Hauteurs du Soleil, et le Tems écoulé entre les Observations.

La latitude est l'élément le plus précieux de la Navigation. La facilité et l'exactitude avec lesquelles on peut la déduire de la hauteur méridienne du soleil, sont cause que les Pilotes se fient principalement à cette donnée pour la direction de leurs routes. Mais cela même fait, que, quand on manque l'observation du midi, l'incertitude qui y résulte est plus grande; et le danger devient imminent dans des circonstances critiques. Ainsi, depuis que les voyages longs et fréquens de la Navigation moderne donnèrent lieu à des recherches exactes pour traverser l'Océan avec sûreté, on a tâché de trouver des

règles propres pour déterminer la latitude par des observations prises hors du méridien; et le public possède à ce sujet un grand nombre de méthodes,* plus ou moins ingénieuses dans la théorie, mais dont la plupart sont restées tout à fait inutiles dans

* Le célèbre PIERRE NUNNEZ (ou NONIUS) s'occupa beaucoup des moyens de déterminer la latitude, et après avoir démontré la fausseté des règles publiées par PIERRE APPIAN (*Cosmographia*) et JACOB ZIEGLER (*Commentarium in secundum librum Naturalis Historiæ Plinii*) il donna différens problèmes de son invention, et entre eux celui qu'on résout par deux hauteurs, et l'arc d'horizon compris par les verticales de l'astre (*De Arte atque Ratione Navigandi*, 1573; *De Observ. Regul. et Instrum. Geometr. &c.*). Je n'ai pas pu éclaircir celui qui le premier substitua au lieu du dernier élément, l'arc de l'équateur compris entre les horaires, ou bien l'intervalle de tems entre les observations; mais on trouve cette solution énoncée comme une chose connue quoique peu utile, dans le traité *De Globis et eorum Usu*, par ROBERT HUES. (Je n'ai jamais vu la première édition de ce livre; celles que je connois, outre les traductions en Anglois et en François, sont une *cum Annot.* J. ISAACCI PONTANI, *Amst.* 1617; et une autre, *Oxon*, 1663.) Le procédé mentionné par HUES exige l'usage des globes. M. FACIO DUILIER (*Navigation improved*, 1728,) expliqua avec assez de détail la même méthode par le calcul trigonométrique; et cependant M. PITOT la publia ensuite (*Mém. de l'Académie des Sciences de Paris*, 1736,) comme quelque chose d'important et de nouveau. Mr. R. GRAHAM imagine pour le même objet un appareil mécanique (*Philosoph. Transact.* 1734,) et M. DE MAUPERTUIS donna aussi une solution tirée des formules établies dans son *Astronomie Nautique* (*Probl. XII.*). Dans les ouvrages postérieurs on ne rencontre, pour la plupart, que les idées des auteurs que nous venons de citer. Au reste, voyez sur la détermination de la latitude par deux hauteurs et par d'autres procédés; *Comm. Acad. Imp. Sc. Petropolit.* 1729, Mémoires de DAN. BERNOULLI, HERMAN, EULER, FR. CHRIST. MAYER, et W. KRAFFT. Id. 1779, Mémoire de M. LEXELL; *Nautical Almanack*, 1778, Appendice par M. LYONS; *L'Astronomie des Marins*, par le P. PEZENAS; *L'Astronomie de M. de LA LANDE*; RÖSLERS *Handbuch der Practic. Astronomie*; *La Trigonométrie rectiligne et sphérique*, par M. CAGNOLI; *Berlin. Astronom. Jahrbücher*, 1787, 1789, 1790, Mémoires de M. M. HENERT, GRAF PLAATEN, et SCHUBERT; *Allgemeine Wörterbuch der Marine*, par RÖDING; *Sammlung Astronom. Abhandlungen*, par KÄSTNER; *Elements of Navigation*, by ROBERTSON; *Traité de Navigation de BOUGUER*, par LA CAILLE; *Opusculs Mathématiques de M. D'ALEMBERT*, IV. p. 357; *Cours de Mathématiques*,

la pratique. La seule qui ait été adoptée par les navigateurs assez généralement est celle de M. Douwes, * qui mérita pour sa solution une récompense du Bureau des Longitudes de la Grande Bretagne. Cette méthode, pourtant, est sujette à quelques inconvéniens; entre autres celui d'exiger dans les opérations l'usage combiné des nombres naturels et artificiels. Je me suis proposé de trouver des moyens plus simples et plus généraux pour calculer la latitude; ce qui m'a engagé dans des recherches, dont je me contenterai de donner ici celles qui me paroissent remplir quelque but utile.

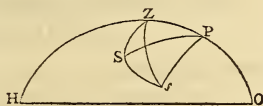
par M. BEZOUT, VI. *Navigation; Voyage de la Flore*, par M. M. DE VERDUN, DE BORDA, et PINGRÉ, I.; *Description et Usage du Cercle de Réflexion*, par M. DE BORDA; *Dictionnaire Encyclopédique des Mathématiques*, II.; *Traité Analytique des Mouvements apparens des Corps Célestes*, par M. DU SEJOUR, &c.

* M. CORNELIUS DOUWES expliqua sa méthode avec beaucoup de détails théoriques et pratiques dans les *Actes de l'Académie de Haarlem*, I. 1754. Ce Mémoire est très intéressant, mais il est resté presque tout à fait inconnu au reste de l'Europe, à cause de la langue du pays où il fut écrit. Je me propose de publier la traduction en François. Les tables de M. DOUWES pour faciliter sa méthode suivirent de très près le précédent ouvrage; et c'est d'après un exemplaire de cette édition que HARRISON fit la sienne en 1759, à Londres. Le Dr. PEMBERTON, à la vue de ces tables, dont il paroît avoir ignoré l'auteur, trouva la théorie et l'inséra dans les *Transactions Philosophiques*, 1760. La connoissance qu'on a des principes du professeur Hollandois est pour la plupart dérivée de ce Mémoire. Sur cette méthode, et sur quelques changements qu'on y a proposé ou fait, ainsi que sur les tables plus étendues qu'on a calculé pour en faciliter l'usage, voyez d'ailleurs—*The British Mariner's Guide*, by Mr. MASKELYNE; *Nautical Almanack*, 1771, Appendice par l'Amiral CAMPBELL; *Nautical Almanack*, 1781, Appendice par Mr. EDWARDS; *Requisite Tables*, 1781; *Le Guide du Navigateur*, par M. LEVÊQUE; *Sammlung Astronomischer Abhandlungen*, 1793, par M. BODE, Mémoire de Mr. NIEUWELAND; *Verhandeling over het bepaalen der Lengte op Zee*, Amst. 1789, par M. M. VAN SWINDEN, NIEUWELAND, et VAN KEULEN; *L'Astronomie de M. DE LA LANDE*; *Tratado de Navegacion*, por DON JOSEF DE MENDOZA RIOS, 1787; *Connoissance des Temps*, 1793, Mémoire de M. DE MENDOZA; *Nautical Almanacks*, 1797—1800, Appendice par Mr. BRINKLEY, &c.

Nous supposons, pour la Première Partie de ces Recherches, la plus grande hauteur du soleil = a , l'angle horaire correspondant = b , l'azimuth correspondant = e , la déclinaison correspondante = d , ou la distance au pôle élevé = D , et l la latitude du lieu où l'on a observé cette hauteur; la petite hauteur du soleil = a' , et les autres éléments relatifs à cette observation = b' , e' , d' , D' , l' . Nous représenterons aussi l'angle horaire, moyen entre b et b' , par m , et la différence entre b et b' par t .

Méthode directe.

Soit HO l'horizon, $HZPO$ le méridien, Z le zénith, P le pôle élevé, et S, s les lieux du soleil aux instants des observations, que nous supposons faites dans le même lieu.



Voici le procédé qu'on prescrit ordinairement pour faire le calcul par la Trigonométrie Sphérique.

Dans le triangle SPs on connoit l'angle SPs qu'on déduit de l'intervalle, et les deux cotés SP , sP qui sont les distances du soleil au pôle élevé; dont on pourra conclure Ss , et SsP , ou sSP . Avec Ss et les compléments des hauteurs ZS , Zs on calculera ZsS ou ZSs . La comparaison entre SsP et ZsS , ou entre sSP et ZSs donnera ZsP ou ZSP . Le premier de ces deux angles, et les cotés Zs , Pz suffisent pour résoudre le triangle ZsP ; ou bien, on pourra résoudre le triangle ZSP à l'aide de l'autre angle ZSP et de ZS , PS ; en concluant ainsi le complément de la latitude ZP .

Tâchons d'établir des formules pour abrégér et simplifier ce calcul.

Dans le triangle SPs que je considérerai comme isoscèle, en supposant la déclinaison constante,

on a $\cos. S s = \cos. t \sin.^2 D + \cos.^2 D$.
d'où l'on déduit

$$\begin{aligned} 1 - \sin. v. S s &= \cos. t \sin.^2 D + \cos.^2 D. \\ \sin. v. S s &= \sin.^2 D - \cos. t \sin.^2 D. \\ \sin. v. S s &= \sin.^2 D \sin. v. t. \end{aligned}$$

Formule propre pour le calcul de $S s$ par les sinus-verses.

En substituant $2 \sin. \frac{1}{2} S s = \sin. v. S s$, et $2 \sin. \frac{1}{2} t = \sin. v. t$, on déduit, pour le calcul par les sinus

$$\sin. \frac{1}{2} S s = \sin. D \sin. \frac{1}{2} t.$$

Dans le même triangle on a

$$\cos. S s P = \frac{\cos. D - \cos. S s \cos. D}{\sin. S s \sin. D}$$

par conséquent

$$\text{susin. } v. S s P - 1 = \frac{\cos. D - \cos. S s \cos. D}{\sin. S s \sin. D}$$

$$\text{susin. } v. S s P = \frac{\cos. D - \cos. S s \cos. D + \sin. S s \sin. D}{\sin. S s \sin. D}$$

$$\text{susin. } v. S s P = \frac{\cos. D - \cos. (S s + D)}{\sin. S s \sin. D}$$

$$\text{susin. } v. S s P = \frac{2 \sin. (\frac{1}{2} S s + D) \sin. \frac{1}{2} S s}{\sin. S s \sin. D}$$

et en substituant $\sin. \frac{1}{2} S s = \sin. D \sin. \frac{1}{2} t$
il résultera

$$\text{susin. } v. S s P = \frac{2 \sin. (\frac{1}{2} S s + D) \sin. \frac{1}{2} t}{\sin. S s}$$

Formule pour calculer $S s P$ par les sinus-verses, et les double-sinus.

Pour le calcul par les sinus on déduit

$$\cos. \frac{1}{2} S s P = \sqrt{\frac{\sin. (\frac{1}{2} S s + D) \sin. \frac{1}{2} t}{\sin. S s}}$$

On pourroit trouver aussi

$$\sin. v. S s P = \frac{2 \sin. (\frac{1}{2} S s \sim D) \sin. \frac{1}{2} t}{\sin. S s}$$

$$\text{et } \sin. \frac{1}{2} S s P = \sqrt{\frac{\sin. (\frac{1}{2} S s \sim D) \sin. \frac{1}{2} t}{\sin. S s}}$$

Dans le triangle $Z S s$ on a

$$\cos. S s Z = \frac{\sin. a - \cos. S s \sin. a'}{\sin. S s \cos. a'}$$

$$1 - \sin. v. S s Z = \frac{\sin. a - \cos. S s \sin. a'}{\sin. S s \cos. a'}$$

$$\sin. v. S s Z = \frac{\sin. (S s + a') - \sin. a}{\sin. S s \cos. a'}$$

$$\sin. v. S s Z = \frac{2 \cos. \frac{1}{2} (S s + a' + a) \sin. \frac{1}{2} (S s + a' - a)}{\sin. S s \cos. a'}$$

Formule pour calculer $S s Z$ par les sinus-verses.

Pour le calcul par les sinus on déduit

$$\sin. \frac{1}{2} S s Z = \sqrt{\frac{\cos. \frac{1}{2} (S s + a' + a) \sin. \frac{1}{2} (S s + a' - a)}{\sin. S s \cos. a'}}$$

On pourroit trouver aussi

$$\text{susin. } v. S s Z = \frac{2 \sin. \frac{1}{2} (S s + a - a') \cos. \frac{1}{2} ((S s - a') \sim a)}{\sin. S s \cos. a'}$$

$$\text{et } \cos. \frac{1}{2} S s Z = \sqrt{\frac{\sin. \frac{1}{2} (S s + a - a') \cos. \frac{1}{2} ((S s - a') \sim a)}{\sin. S s \cos. a'}}$$

Plusieurs auteurs de Trigonométrie Sphérique supposent que l'angle $Z s P$ est toujours égal à la différence entre $S s P$, et $S s Z$; mais cette règle générale n'est pas exacte. Le vertical $Z s$ peut tomber à l'autre coté de $S s$ relativement au pole élevé; ce qui a lieu quand l'astre dans sa révolution diurne passe entre le zénith et le pole élevé. On doit prendre alors la somme, et non pas la différence des angles ci-dessus, pour avoir celui qu'on cherche.

L'angle $Z S P$ peut être aussi égal au complément à 360° de la somme de $s P$, et $Z S s$; et l'attention à cette circonstance seroit nécessaire dans le cas où l'on feroit le calcul par les angles en S .

Après avoir déterminé l'angle $Z s P$, on a

$$\sin. l = \cos. Z s P \cos. a' \sin. D + \sin. a' \cos. D$$

$$\sin. l = \sin. a' \cos. D + \cos. a' \sin. D - \sin. v. Z s P \cos. a' \cos. d$$

$$\begin{aligned} \sin. l &= \sin. (D + a') - \sin. v. Z s P \cos. a' \cos. d \\ 1 + \sin. l &= 1 + \sin. (D + a') - \sin. v. Z s P \cos. a' \cos. d \\ \text{sucos. } v. l &= \text{sucos. } v. (D + a') - \sin. v. Z s P \cos. a' \cos. d \\ \text{sucos. } v. l &= \text{sucos. } v. (D + a') \left(1 - \frac{\sin. v. Z s P \cos. a' \cos. d}{\text{sucos. } v. (D + a')} \right). \end{aligned}$$

Formule pour déterminer finalement l par les sinus-verses; car on voit, qu'en faisant $\frac{\sin. v. Z s P \cos. a' \cos. d}{\text{sucos. } v. (D + a')} = \sin. v. N$, on aura $\text{sucos. } v. l = \text{sucos. } v. (D + a') \cos. N$.

En substituant dans la formule précédente $2 \sin.^2 \frac{1}{2} (90^\circ + l) = \text{sucos. } v. l$, $2 \sin.^2 \frac{1}{2} Z s P = \sin. v. Z s P$, et $2 \sin.^2 \frac{1}{2} (90^\circ + D + a') = \text{sucos. } v. (D + a')$, il résultera, pour le calcul de l par les sinus,

$$\sin. \frac{1}{2} (90^\circ + l) = \sin. \frac{1}{2} (90^\circ + D + a') \sqrt{1 - \frac{\sin.^2 \frac{1}{2} Z s P \cos. a' \cos. d}{\sin.^2 \frac{1}{2} (90^\circ + D + a')}}.$$

Par où l'on voit, qu'en faisant $\frac{\sin. \frac{1}{2} Z s P \sqrt{\cos. a' \cos. d}}{\sin. \frac{1}{2} (90^\circ + D + a')} = \sin. N$ on aura $\sin. \frac{1}{2} (90^\circ + l) = \sin. \frac{1}{2} (90^\circ + D + a') \cos. N$.

On pourroit aussi déduire

$$\begin{aligned} \cos. v. l &= \cos. v. (D + a') \left(1 + \frac{\sin. v. Z s P \cos. a' \cos. d}{\cos. v. (D + a')} \right) \\ \text{pour faire } &\frac{\sin. v. Z s P \cos. a' \cos. d}{\cos. v. (D + a')} = \cos. N, \text{ et avoir} \\ \cos. v. l &= \cos. v. (D + a') \text{ susin. } v. N. \end{aligned}$$

Aussi,

$$\cos. \frac{1}{2} (90^\circ + l) = \cos. \frac{1}{2} (90^\circ + D + a') \sqrt{1 + \frac{\sin.^2 \frac{1}{2} Z s P \cos. a' \cos. d}{\cos.^2 \frac{1}{2} (90^\circ + D + a')}}.$$

où, en faisant $\frac{\sin. Z s P \sqrt{\cos. a' \cos. d}}{\cos. \frac{1}{2} (90^\circ + D + a')} = \tan. N$, on a

$$\cos. \frac{1}{2} (90^\circ + l) = \frac{\cos. \frac{1}{2} (90^\circ + D + a')}{\cos. N}.$$

Nous examinerons à présent l'erreur qui résulte dans la latitude de celles qu'on peut commettre dans les élémens du calcul.

Supposons premièrement une erreur δt dans l'intervalle. Les analogies différentielles donnent, en supposant l'angle horaire et la latitude variables,

$$\delta b = \frac{\delta l (\tan. d - \tan. l \cos. b)}{\sin. b}$$

et
$$\delta b' = \frac{\delta l \tan. d - \tan. l \cos. b'}{\sin. b'}$$

On aura donc

$$\delta t = \delta b' - \delta b = \delta l \left(\tan. l (\cot. b - \cot. b') - \frac{\tan. d (\sin. b' - \sin. b)}{\sin. b \sin. b'} \right)$$

Par conséquent
$$\delta l = \frac{\delta t \sin. b \sin. b'}{\tan. l \sin. t - z \tan. d \cos. m \sin. \frac{1}{2} t};$$

ou bien
$$\delta l = \frac{\delta t}{\tan. l (\cot. b - \cot. b') - \tan. d (\operatorname{cosec.} b - \operatorname{cosec.} b')}.$$

En supposant une erreur δa dans la grande hauteur, on a $-\delta t = \delta b = \frac{\delta a \cos. a}{\cos. d \cos. l \sin. b}$; ce qui, étant substitué dans l'équation précédente, donne

$$\delta l = - \frac{\delta a \cos. a \sin. b'}{\cos. d \sin. l \sin. t - z \sin. d \cos. l \cos. m \sin. \frac{1}{2} t}$$

et
$$\delta l = - \frac{\delta a \cos. a \sin. b'}{\cos. d \sin. l \sin. t - \sin. d \cos. l (\sin. b' - \sin. b)}$$

Pour l'erreur de la petite hauteur on auroit aussi

$$\delta t = \delta b = \frac{\delta a' \cos. a'}{\cos. d \cos. l \sin. b'}$$
; d'où l'on déduit

$$\delta l = \frac{\delta a' \cos. a' \sin. b}{\cos. d \sin. l \sin. t - z \sin. d \cos. l \cos. m \sin. \frac{1}{2} t}$$

et
$$\delta l = \frac{\delta a' \cos. a' \sin. b}{\cos. d \sin. l \sin. t - \sin. d \cos. l (\sin. b' - \sin. b)}$$

Méthode indirecte, en déduisant premièrement l'Angle borairé moyen.

La Trigonométrie Sphérique donne $\cos. b = \frac{\sin. a - \sin. d \sin. l}{\cos. d \cos. l}$

et $\cos. b' = \frac{\sin. a' - \sin. d' \sin. l'}{\cos. d' \cos. l'}$. Par conséquent

$$\cos. b - \cos. b' = 2 \sin. m \sin. \frac{1}{2} t = \left\{ \begin{array}{l} \sin. a \cos. d' \cos. l' - \sin. a' \cos. d \cos. l \\ - \sin. d \cos. d' \sin. l \cos. l' \\ + \cos. d \sin. d' \cos. l \sin. l' \end{array} \right. / \cos. d \cos. d' \cos. l \cos. l'$$

et

$$\sin. m = \frac{\sin. a \cos. d' \cos. l' - \sin. a' \cos. d \cos. l - \sin. d \cos. d' \sin. l \cos. l' + \cos. d \sin. d' \cos. l \sin. l'}{2 \cos. d \cos. d' \cos. l \cos. l' \sin. \frac{1}{2} t}$$

Voici l'expression générale de l'horaire moyen m dans tous les cas du problème. Quand les observations ont été faites dans le même lieu on a $l = l'$, et en supposant la déclinaison constante dans l'intervalle $d = d'$, ce qui réduit la formule alors

$$\text{à } \sin. m = \frac{\sin. a - \sin. a'}{2 \cos. d \cos. l \sin. \frac{1}{2} t}. \text{ Les circonstances dans la pratique}$$

sont presque toujours différentes; mais, l'intervalle n'étant que de quelques heures, la différence entre l et l' ne peut jamais être grande, et celle entre d et d' doit être encore moins considérable. Nous pourrons donc transformer la formule générale, en supposant ces différences très petites, pour déduire des expressions propres pour le calcul.

Faisons $l = l' + \Delta l$, et $d = d' + \Delta d$; et l'on aura

$$\cos. d' = \cos. d + \Delta d \sin. d$$

$$\sin. d' = \sin. d - \Delta d \cos. d$$

$$\cos. l' = \cos. l + \Delta l \sin. l$$

$$\sin. l' = \sin. l - \Delta l \cos. l$$

Substituons y ces expressions, en négligeant les produits des deux dimensions de Δl , Δd , et nous aurons

$$\sin. m = \left\{ \begin{array}{l} (\sin. a - \sin. a') \cos. d \cos. l + \Delta l (\sin. a \cos. d \sin. l - \sin. d \cos. d) \\ + \Delta d (\sin. a \sin. d \cos. l - \sin. l \cos. l) \end{array} \right. / 2 \cos. d \cos. l \sin. \frac{1}{2} t (\cos. d \cos. l + \Delta l \cos. d \sin. l + \Delta d \sin. d \cos. l)$$

Représentons la latitude supposée du lieu où on observa la plus grande hauteur par l'' , et faisons $l = l'' + \delta l''$, en suppo-

sant toujours que la différence $\delta l''$ est petite. Si l'on calcule un angle horaire moyen M avec cette latitude, on aura

$$\sin. M = \frac{\sin. a - \sin. a'}{2 \cos. d \cos. l'' \sin. \frac{1}{2} t} = \frac{\sin. a - \sin. a'}{2 \cos. d \cos. l \sin. \frac{1}{2} t + 2 \delta l'' \cos. d \sin. l \sin. \frac{1}{2} t}$$

Par conséquent, $\sin. m = \sin. M + \dots$

$$\frac{\Delta l (\cos. d \sin. l \sin. a' - \sin. d \cos. d) + \Delta d (\sin. d \cos. l \sin. a' - \sin. l \cos. l) + \delta l'' \cos. d \sin. l (\sin. a - \sin. a')}{2 \cos. d \cos. l \sin. \frac{1}{2} t (\cos. d \cos. l + \Delta l \cos. d \sin. l + \Delta d \sin. d \cos. l + \delta l'' \cos. d \sin. l)}$$

et, à très peu près,

$$m = M + \left\{ \begin{array}{l} \frac{\Delta l (\sin. l'' \sin. a' - \sin. d)}{2 \cos. d \cos. l'' \cos. M \sin. \frac{1}{2} t} + \frac{\Delta d (\sin. d \sin. a' - \sin. l'')}{2 \cos. d \cos. l'' \cos. M \sin. \frac{1}{2} t} \\ + \frac{\delta l'' \sin. l'' (\sin. a - \sin. a')}{2 \cos. d \cos. l'' \cos. M \sin. \frac{1}{2} t} \end{array} \right.$$

Substituant $\sin. a' = \cos. b' \cos. d \cos. l'' + \sin. d \sin. l''$ dans le second et le troisième membre de la droite, et $\sin. M = \frac{\sin. a - \sin. a'}{2 \cos. d \cos. l'' \sin. \frac{1}{2} t}$ dans le dernier, il résultera

$$m = M + \left\{ \begin{array}{l} \frac{\Delta l (\cos. b' \tan. l'' - \tan. d)}{2 \cos. M \sin. \frac{1}{2} t} + \frac{\Delta d (\cos. b' \tan. d - \tan. l'')}{2 \cos. M \sin. \frac{1}{2} t} \\ + \delta l'' \tan. l'' \tan. M. \end{array} \right.$$

Formule qui donne la valeur de l'horaire moyen pour le calcul relatif au lieu de la plus grande hauteur.

Si l'on suppose $l' = l + \Delta l$, et $d' = d + \Delta d$, on aura, en substituant comme nous avons fait auparavant,

$$\sin. m = \left\{ \begin{array}{l} (\sin. a - \sin. a') \cos. d' \cos. l' + \Delta l (\sin. d' \cos. d' - \sin. a' \cos. d' \sin. l') \\ + \Delta d (\sin. l' \cos. l' - \sin. a' \sin. d' \cos. l') \\ \frac{\dots}{2 \cos. d' \cos. l' \sin. \frac{1}{2} t (\cos. d' \cos. l' + \Delta l \cos. d' \sin. l' + \Delta d \sin. d' \cos. l')} \end{array} \right.$$

En représentant par l''' la latitude estimée du lieu où on a observé la plus petite hauteur, et en faisant $l''' + \delta l''' = l'$, et

$$\sin. M' = \frac{\sin. a - \sin. a'}{2 \cos. d' \cos. l''' \sin. \frac{1}{2} t}$$

ou, ce qui revient au même,

$$\sin. M = \frac{\sin. a - \sin. a'}{2 \cos. d' \cos. l'' \sin. \frac{1}{2} t + 2 \delta l''' \cos. d' \sin. l'' \sin. \frac{1}{2} t}$$

on aura $\sin. m = \sin. M' + \dots$

$$\frac{\Delta l (\sin. d' \cos. d' - \sin. a \cos. d' \sin. l') + \Delta d (\sin. l' \cos. l' - \sin. a \sin. d' \cos. l') + \delta l''' \cos. d' \sin. l'' (\sin. a - \sin. a')}{2 \cos. d' \cos. l'' \sin. \frac{1}{2} t (\Delta l \cos. d' \sin. l'' + \Delta d \sin. d' \cos. l'' + \delta l''' \cos. d' \sin. l'')}$$

et, à très peu près,

$$m = M' + \left\{ \begin{array}{l} \frac{\Delta l (\sin. d' - \sin. l'' \sin. a)}{2 \cos. d' \cos.^2 l'' \cos. M' \sin. \frac{1}{2} t} + \frac{\Delta d (\sin. l'' - \sin. d' \sin. a)}{2 \cos.^2 d' \cos. l'' \cos. M' \sin. \frac{1}{2} t} \\ + \frac{\delta l'' \sin. l'' (\sin. a - \sin. a')}{2 \cos. d' \cos.^2 l'' \sin. \frac{1}{2} t} \end{array} \right.$$

Par où, en substituant $\sin. a = \cos. b \cos. d' \cos. l''' + \sin. d' \sin. l''''$,

et $\sin. M' = \frac{\sin. a - \sin. a'}{2 \cos. d' \cos. l'' \sin. \frac{1}{2} t}$, il résulte

$$m = M' + \left\{ \begin{array}{l} \frac{\Delta l (\tan. d' - \cos. b \tan. l'')}{2 \cos. M' \sin. \frac{1}{2} t} + \frac{\Delta d (\tan. l'' - \cos. b \tan. d')}{2 \cos. M' \sin. \frac{1}{2} t} \\ + \delta l''' \tan. l'''' \tan. M'. \end{array} \right.$$

Formule de l'horaire moyen pour le calcul relatif au lieu de la plus petite hauteur.

En considérant ces formules, on voit facilement la manière dont on doit procéder pour obtenir l'horaire moyen. De l'intervalle, et de la différence en longitude entre les lieux des observations, on déduira t . Avec cette quantité, et les données du problème, on trouvera M par l'expression $\frac{\sin. a - \sin. a'}{2 \cos. d \cos. l' \sin. \frac{1}{2} t}$, si l'on veut faire le calcul relativement au lieu de la plus grande hauteur; ou bien on trouvera M' par l'expression $\frac{\sin. a - \sin. a'}{2 \cos. d' \cos. l'' \sin. \frac{1}{2} t}$ pour faire le calcul relativement au lieu de la plus petite hauteur. Après quoi, il faudra appliquer à M , ou M' , les équations convenables pour avoir m .

Les variations de la latitude, et de la déclinaison, étant connues par la nature du problème, on pourroit calculer par les expressions ci-dessus les équations qui en dérivent; mais l'horaire moyen réseroit toujours affecté de l'erreur qui dépend de $\delta l''$, ou $\delta l'''$, dont le dégagement n'est pas praticable jusqu'à la conclusion de la latitude. Il me paroît donc préférable de laisser toutes les corrections pour le dernier résultat.

Mr. DOUWES a employé la formule $\sin. M = \frac{\sin. a - \sin. d}{2 \cos. d \cos. l \sin. \frac{1}{2} t}$ pour sa méthode, et le Dr. PEMBERTON l'a mise sous la forme $\sin. M = \frac{\cos. \frac{1}{2} (a + d) \sin. \frac{1}{2} (a' - a)}{\cos. d \cos. l \sin. \frac{1}{2} t}$, qui est propre pour le calcul par les logarithmes, sans le secours des sinus naturels.

Après avoir déterminé M, ou M', on aura (en représentant le petit horaire approché par H, et le grand horaire approché par H'), $H = M - \frac{1}{2} t$, et $H' = M' + \frac{1}{2} t$.

Avec un horaire, et la hauteur et la déclinaison correspondantes, il seroit facile de calculer la latitude par les règles ordinaires de la Trigonométrie Sphérique, mais la solution du problème exigeroit alors des distinctions des cas qui la rendroient complexe, et que l'on doit éviter autant que possible. Nous chercherons, donc, des formules pour arriver au résultat par un procédé plus simple, et nous nous proposerons de déterminer la distance méridienne du soleil au zénith $d \sim l$; car cette distance une fois connue, la conclusion de la latitude est très facile.

Reprenons la formule $\cos. b = \frac{\sin. a - \sin. d \sin. l}{\cos. d \cos. l}$, et nous aurons $\cos. b \cos. d \cos. l + \sin. d \sin. l = \sin. a$, d'où (en substituant $1 - \sin. v. b = \cos. b$), on déduit

$\cos. d \cos. l + \sin. d \sin. l = \sin. a + \sin. v. b \cos. d \cos. l$,
et par conséquent,

$$\cos. (d \sim l) = \sin. a + \sin. v. b \cos. d \cos. l$$

ou (en représentant par L la latitude qui résulte du calcul),*

$$\cos. (d \sim L) = \sin. a + \sin. v. H \cos. d \cos. l''.$$

* En substituant dans $\cos. b \cos. d \cos. l + \sin. d \sin. l = \sin. a$, l'expression $\cos. b = \sin. v. b - 1$, on déduiroit $\sin. v. b \cos. d \cos. l - \cos. d \cos. l + \sin. d \sin. l = \sin. a$, et par conséquent $\cos. (d + l) = \sin. v. b \cos. d \cos. l - \sin. a$. Je laisse pour une autre occasion le détail des applications qu'on pourroit faire de cette formule.

De cette équation on tire

$$1 - \cos. (d \sim L) = 1 - \sin. a - \sin. v. H \cos. d \cos. l''$$

$$\sin. v. (d \sim L) = \cos. v. a - \sin. v. H \cos. d \cos. l''$$

$$\sin. v. (d \sim L) = \cos. v. a \left(1 - \frac{\sin. v. H \cos. d \cos. l''}{\cos. v. a} \right).$$

Première formule, pour calculer la distance méridienne du soleil au zénith $d \sim L$, par les sinus-verses. En faisant donc

$$\frac{\sin. v. H \cos. d \cos. l''}{\cos. v. a} = \cos. N, \text{ on aura}$$

$$\sin. v. (d \sim L) = \cos. v. a \sin. v. N.$$

De l'équation $\cos. (d \sim L) = \sin. a + \sin. v. H \cos. d \cos. l''$ on tire aussi

$$1 + \cos. (d \sim L) = 1 + \sin. a + \sin. v. H \cos. d \cos. l''$$

$$\text{susin. v. } (d \sim L) = \text{sucos. v. } a + \sin. v. H \cos. d \cos. l''$$

$$\text{susin. v. } (d \sim L) = \text{sucos. v. } a \left(1 + \frac{\sin. v. H \cos. d \cos. l''}{\text{sucos. v. } a} \right)$$

Seconde formule, pour faire le calcul, par les sinus-verses. En faisant $\frac{\sin. v. H \cos. d \cos. l''}{\text{sucos. v. } a} = \cos. N$, on aura donc,

$$\text{susin. v. } (d \sim L) = \text{sucos. v. } a \text{ susin. v. } N.$$

Comme l'arc $d \sim L$ est toujours moindre que 90° ,
 $\sin. v. (d \sim L)$ sera sans exception plus petit que $\text{susin. v. } (d \sim L)$;
 et, par conséquent, la première formule préférable à la seconde.

De la première formule, on tire

$$\sin. \frac{1}{2} (d \sim L) = \cos. \frac{1}{2} (90^\circ + a) \left(1 - \frac{\sin. \frac{1}{2} H \cos. d \cos. l''}{\cos. \frac{1}{2} (90^\circ + a)} \right)$$

$$\text{et } \sin. \frac{1}{2} (d \sim L) = \cos. \frac{1}{2} (90^\circ + a) \sqrt{1 - \frac{\sin. \frac{1}{2} H \cos. d \cos. l''}{\cos. \frac{1}{2} (90^\circ + a)}}$$

Troisième formule. Au moyen de la quelle on pourra calculer

$$d \sim L \text{ par les sinus; car en faisant } \frac{\sin. \frac{1}{2} H \sqrt{\cos. d \cos. l''}}{\cos. \frac{1}{2} (90^\circ + a)} = \sin. N,$$

$$\text{on aura } \sin. \frac{1}{2} (d \sim L) = \cos. \frac{1}{2} (90^\circ + a) \cos. N.$$

De la seconde formule on tire

$$\cos. \frac{1}{2} (d \sim L) = \sin. \frac{1}{2} (90^\circ + a) \left(1 + \frac{\sin. \frac{1}{2} H \cos. d \cos. l'}{\sin. \frac{1}{2} (90^\circ + a)} \right)$$

$$\text{et } \cos. \frac{1}{2} (d \sim L) = \sin. \frac{1}{2} (90^\circ + a) \sqrt{1 + \frac{\sin. \frac{1}{2} H \cos. d \cos. l'}{\sin. \frac{1}{2} (90^\circ + a)}}$$

Quatrième formule. A l'aide de laquelle on pourra calculer $d \sim L$ par les sinus et les tangentes; car, en faisant

$$\frac{\sin. \frac{1}{2} H \sqrt{\cos. d \cos. l'}}{\sin. \frac{1}{2} (90^\circ + a)} = \tan. N, \text{ on aura } \cos. \frac{1}{2} (d \sim L) = \frac{\sin. \frac{1}{2} (90^\circ + a)}{\cos. N}.$$

On doit remarquer que $\sin. \frac{1}{2} (d \sim L)$ est toujours moindre que $\cos. \frac{1}{2} (90^\circ + a)$, et que $\cos. \frac{1}{2} (d \sim L)$ est toujours plus grand que $\sin. \frac{1}{2} (90^\circ + a)$; ce qui rend la troisième formule plus exacte pour le calcul que la formule quatrième. Cependant, comme, en faisant usage des logarithmes sinus et tangentes seulement, le total des opérations est un peu plus court par le moyen de la dernière, on pourra préférer cette formule quand les tables qu'on emploie ne contiendront pas les sécantes.

Voici une autre manière de conclure la latitude, après avoir déterminé l'angle horaire; car, au lieu de la distance méridienne du soleil au zénith, on pourroit calculer la différence entre cette distance, et la distance au zénith correspondante à l'observation près du midi, ou ce qui revient au même, la différence entre la hauteur méridienne, et la plus grande hauteur observée. La formule $\cos. (d \sim l) = \sin. a + \sin. v. b \cos. d \cos. l$, donne $\cos. (d \sim l) - \cos. (90^\circ - a) = \sin. v. b \cos. d \cos. l$, et par conséquent

$$2 \sin. \frac{1}{2} (90^\circ - a + (d \sim l)) \sin. \frac{1}{2} (90^\circ - a - (d \sim l)) = \sin. v. b \cos. d \cos. l$$

d'où l'on déduit

$$\sin. \frac{1}{2} (90^\circ - a - (d \sim l)) = \cos. \frac{1}{2} (90^\circ + a + (d \sim l))$$

$$= \frac{\sin. v. b \cos. d \cos. l}{2 \sin. \frac{1}{2} (90^\circ - a + (d \sim l))} = \frac{\sin. \frac{1}{2} b \cos. d \cos. l}{\sin. \frac{1}{2} (90^\circ - a + (d \sim l))}$$

ou

$$\sin. \frac{1}{2} (90^\circ - a - (d \sim l)) = \cos. \frac{1}{2} (90^\circ + a + (d \sim l))$$

$$= \frac{\sin. v. b \cos. d \cos. l}{2 \cos. \frac{1}{2} (90^\circ + a - (d \sim l))} = \frac{\sin. \frac{1}{2} b \cos. d \cos. l}{\cos. \frac{1}{2} (90^\circ + a - (d \sim l))}$$

Après avoir trouvé $90^\circ + a + (d \sim l)$, on déduiroit facilement la distance méridienne $d \sim l$. Avec cette formule, on épargneroit quelques logarithmes, mais l'ensemble des opérations ne seroit pas pour cela plus facile. Je crois donc avantageux de préférer l'expression qui donne directement $d \sim l$, et je supposerai qu'on fasse toujours le calcul par cette méthode.

Si l'on reprend l'équation $\cos. b' \cos. d' \cos. l' = \dots$
 $\sin. a' - \sin. d' \sin. l'$, on aura, comme auparavant, $\cos. (d' \sim l') =$
 $\sin. a' + \sin. v. b' \cos. d' \cos. l'$, ou (en représentant par L' la latitude calculée du lieu de la plus petite hauteur), $\cos. (d' \sim L') =$
 $\sin. a' + \sin. v. H' \cos. d' \cos. l''$. En suivant le procédé ci-dessus, on déduira d'ici quatre formules pour calculer la distance méridienne $d' \sim L'$, relative au lieu de la plus petite hauteur; formules qui sont analogues à celles que nous avons établies pour $d \sim L$ relativement au lieu de la plus grande hauteur.

Mais, le calcul précédent étant fait avec des élémens qui ne sont pas rigoureusement vrais, il faut à présent chercher des moyens pour porter le résultat de la méthode jusqu'au degré d'exactitude qui est nécessaire dans la pratique de la Navigation.

Considérons d'abord le calcul relativement au lieu de la plus grande hauteur.

L'expression employée est

$$\cos. (d \sim L) = \sin. a + \sin. v. H \cos. d \cos. l'',$$

où l'' représente la latitude estimée, et H le petit horaire déduit du calcul. Les erreurs de ces quantités seront toujours petites. On pourra donc avoir recours au calcul différentiel pour déterminer leur influence, et l'on aura

$$-\delta(d \sim L) \sin.(d \sim L) = \begin{cases} d H \sin. H \cos. d \cos. l'' \\ -\delta l'' \sin. v. H \cos. d \sin. l'' \end{cases}$$

et

$$-\delta(d \sim L) \sin.(d \sim L) = \begin{cases} d H. \sin. H \cos. d \cos. l'' + \\ \delta l'' \cos. H \cos. d \sin. l'' - \delta l'' \cos. d \sin. l'' \end{cases}$$

Mais nous avons trouvé

$$\delta H = \delta M = \begin{cases} \frac{\Delta l (\cos. B' \tan. l'' - \tan. d)}{2 \cos. M \sin. \frac{1}{2} t} + \frac{\Delta d (\cos. B' \tan. d - \tan. l'')}{2 \cos. M \sin. \frac{1}{2} t} \\ + \delta l'' \tan. l'' \tan. M \end{cases}$$

ou, ce qui revient au même,

$$\delta H = \delta M = \begin{cases} \frac{\Delta l (\cos. H' \tan. l'' - \tan. d)}{2 \cos. M \sin. \frac{1}{2} t} + \frac{\Delta d (\cos. H' \tan. d - \tan. l'')}{2 \cos. M \sin. \frac{1}{2} t} \\ + \delta l'' \tan. l'' \tan. M. \end{cases}$$

Donc, en substituant, et en prenant l'' pour L (car ces quantités ne diffèrent que de peu de chose), il résultera

$$\delta(d \sim L) = \begin{cases} \frac{\Delta l \sin. H. \cos. d \cos. l'' (\tan. d - \cos. H' \tan. l'')}{2 \cos. M \sin. \frac{1}{2} t \sin. (d \sim l'')} + \frac{\Delta d \sin. H \cos. d \cos. l'' (\tan. l'' - \cos. H' \tan. d)}{2 \cos. M \sin. \frac{1}{2} t \sin. (d \sim l'')} \\ + \frac{\delta l'' \cos. d \sin. l'' (\cos. M - \cos. \frac{1}{2} t)}{\cos. M \sin. (d \sim l'')} \end{cases}$$

$$\delta(d \sim L) = \begin{cases} \frac{\Delta l \sin. H (\tan. d - \cos. H' \tan. l'')}{2 \cos. M \sin. \frac{1}{2} t (\tan. d \sim \tan. l'')} + \frac{\Delta d \sin. H (\tan. l'' - \cos. H' \tan. d)}{2 \cos. M \sin. \frac{1}{2} t (\tan. d \sim \tan. l'')} \\ + \frac{\delta l'' (\cos. M - \cos. \frac{1}{2} t)}{\cos. M (\tan. d \cot. l'' \sim 1)} \end{cases}$$

$$\delta(d \sim L) = \begin{cases} \frac{\Delta l \sin. H (\tan. d \cot. l'' - \cos. H')}{(\sin. H' - \sin. H) (\tan. d \cot. l'' \sim 1)} + \frac{\Delta d \sin. H (\cot. d \tan. l'' - \cos. H')}{(\sin. H' - \sin. H) (\cot. d \tan. l'' \sim 1)} \\ + \frac{\delta l'' (\cos. M - \cos. \frac{1}{2} t)}{\cos. M (\tan. d \cot. l'' \sim 1)} \end{cases}$$

Voilà les corrections qu'on doit appliquer à la distance méridienne du soleil au zénith $d \sim L$. Les mêmes corrections ont lieu aussi pour la latitude calculée L ; car $\delta L = \delta(d \sim L)$. Le signe supérieur, quand le soleil passe par le quart de méridien où se trouve le pole élevé, le signe inférieur dans les autres cas.

A l'aide des expressions ci-dessus, on pourroit former des

tables pour avoir facilement les corrections relatives aux variations Δl , Δd ; ce qui seroit convenable pour rendre la méthode générale, et très exacte.

A l'égard de la correction relative à $d l''$, voici le procédé qui me paroit le plus simple, et le plus expéditif, et par conséquent le plus avantageux pour la pratique. On peut faire le calcul tant pour une latitude supposée l''' , que pour une autre latitude l'' , de manière que la différence entre l''' , et l'' soit peu considérable. Ainsi l'on aura (en représentant la latitude calculée résultante de l'' par L , et la latitude calculée résultante de l''' par L') $\delta L = \frac{\delta l'' (\cos. M - \cos. \frac{1}{2} l)}{\cos. M (\tan. d \cot. l'' \sim 1)}$, et à très peu près

$$\approx \delta L' = \frac{\delta l'' (\cos. M - \cos. \frac{1}{2} l)}{\cos. M (\tan. d \cot. l' \sim 1)}$$

De là on tire

$$\delta L : \delta L' :: \delta l'' : \delta l'''$$

par conséquent $(\delta L \approx \delta l''') : \delta L :: (\delta L' \approx \delta l'') : \delta L'$

et $(\delta L \approx \delta l''') \approx (\delta L' \approx \delta l'') : (\delta L \approx \delta l'') :: (\delta L \approx \delta L') : \delta L$

d'où il résulte $\delta L = \frac{(\delta L \approx \delta l''') (\delta L \approx \delta L')}{(\delta L \approx \delta l''') \approx (\delta L' \approx \delta l'')}$

c'est-à dire $\delta L = \frac{(L \sim l''') (L \sim L')}{(L' \sim l'') \approx (L' \sim l'')}$.

Expression de la correction qu'on doit appliquer à la latitude calculée L . Le signe supérieur, quand les deux latitudes calculées s'éloignent dans le même sens des respectives latitudes supposées; le signe inférieur, dans le cas contraire.

On pourroit aussi déduire la correction qu'on doit appliquer à la latitude supposée, et l'on auroit $\delta l'' = \frac{(L \sim l''') (l'' \sim l''')}{(L \sim l'') \approx (L' \sim l'')}$.

La manière d'appliquer la correction δL , ou celle $\delta l''$, est évidente, si l'on fait attention que la latitude vraie doit être comprise entre les deux latitudes supposées, ou entre les deux latitudes calculées, dans tous les cas, excepté celui où les deux

latitudes calculées s'éloignent dans le même sens des correspondantes latitudes supposées ; et que dans cette circonstance la latitude vraie se trouve près de la latitude supposée (ou calculée) qui diffère le moins de sa correspondante latitude calculée (ou supposée).

Pour le calcul relativement au lieu de la petite hauteur, on déduiroit aussi, par un procédé semblable,

$$\mp \delta L' = \delta(d' \sim L') = \begin{cases} \frac{\Delta l \sin. H'(\cos. H - \tan. d' \cot. l''')}{(\sin. H' - \sin. H)(\tan. d' \cot. l'' \sim 1)} + \frac{\Delta d \sin. H'(\cos. H - \cot. d' \tan. l''')}{(\sin. H' - \sin. H)(\cot. d' \tan. l'' \sim 1)} \\ + \frac{\delta l''(\cos. M' - \cos. \frac{1}{2} l)}{\cos. M'(\tan. d' \cot. l'' \sim 1)} \end{cases}$$

Expressions auxquelles on peut appliquer ce qui vient d'être dit au sujet des formules analogues que nous avons trouvé pour le lieu de la grande hauteur.

Après avoir établi les formules nécessaires pour calculer la latitude, nous considérerons les erreurs qui peuvent influer dans le résultat, pour déterminer les circonstances favorables à l'usage du problème. Nous examinerons aussi, s'il est indifférent de faire le calcul relativement au lieu de la grande hauteur, ou relativement au lieu de la petite hauteur, ou laquelle de ces deux manières d'opérer est la préférable.

Pour la plus grande facilité des comparaisons, nous représenterons par L la latitude calculée relativement à la grande hauteur, ou à la petite hauteur, et nous employerons les dénominations des élémens vrais, en prenant aussi indistinctement $\delta l''$, ou $\delta l'''$.

L'équation générale qui exprime la relation entre une erreur commise dans la latitude supposée, et l'erreur résultante dans la latitude calculée, est $\mp \delta L = \frac{\delta l''(\cos. m - \cos. \frac{1}{2} l)}{\cos. m(\tan. d \cot. l \sim 1)}$; ce qui prouve que l'erreur de la latitude calculée n'est pas fort différente, soit qu'on calcule pour le lieu de la grande hauteur, ou de la petite hauteur.

Comme m est plus grand ou plus petit que $\frac{1}{2}t$, selon qu'on a fait les observations du même coté du méridien, ou l'une avant et l'autre après midi, on voit 1°. Que, dans le cas où les observations sont de la même espèce, les erreurs de la latitude supposée, et de la latitude calculée ont le même signe, quand le soleil passe par le quart du méridien où se trouve le pôle élevé; et que ces erreurs ont des signes contraires, dans toutes les autres circonstances. 2°. Que la règle inverse a lieu, quand les observations sont de différente espèce.

Supposons qu'on ait commis une petite erreur δt dans l'intervalle. On aura $\delta m - \frac{1}{2}\delta t = \delta b$, et $\delta m + \frac{1}{2}\delta t = \delta b'$; et en prenant $\sin. m = \frac{\sin. a - \sin. d'}{2 \sin. \frac{1}{2}t \cos. d \cos. l}$, et différentiant, $\delta m = \dots - \frac{1}{2}\delta t \cot. \frac{1}{2}t \tan. m$. Ainsi $\delta b = -\frac{1}{2}\delta t \tan. m \cot. \frac{1}{2}t - \frac{1}{2}\delta t$ et $\delta b' = -\frac{1}{2}\delta t \tan. m \cot. \frac{1}{2}t + \frac{1}{2}\delta t$.

En différentiant l'équation

$$\cos. (d \sim L) = \sin. a + \sin. b \cos. d \cos. l$$

on aura

$$\mp \delta L = - \frac{\delta b \sin. b \cos. d \cos. l}{\sin. (d \sim L)}$$

ce qui, en substituant la valeur de δb ci-dessus, donne

$$\mp \delta L = \frac{\frac{1}{2}\delta t \sin. b \cos. d \cos. l (\tan. m \cot. \frac{1}{2}t + 1)}{\sin. (d \sim L)}$$

$$\mp \delta L = \frac{\frac{1}{2}\delta t \sin. b \sin. b' \cos. d \cos. l}{\cos. m \sin. \frac{1}{2}t \sin. (d \sim L)}$$

$$\mp \delta L = \frac{\delta t \sin. b \sin. b' \cos. d \cos. l}{(\sin. b' - \sin. b) \sin. (d \sim L)}$$

$$\mp \delta L = \frac{\delta t \sin. b \sin. b'}{(\sin. b' - \sin. b) (\tan. d \sim \tan. l)}$$

$$\mp \delta L = \frac{\delta t}{(\operatorname{cosec.} b - \operatorname{cosec.} b') (\tan. d \sim \tan. l)}$$

Expression de l'influence de l'erreur de l'intervalle, en calculant par la grande hauteur.

En différenciant l'équation

$$\cos. (d \sim L) = \sin. a' + \sin. v. b' \cos. d \cos. l$$

on aura

$$\mp \delta L = - \sin. b' \cos. d \cos. l ;$$

ce qui, en substituant la valeur de $\delta b'$ ci-dessus, donnera les mêmes expressions qu'on vient de trouver pour $\mp \delta L$.

On voit donc, que l'influence d'une erreur commise dans l'intervalle est la même dans les deux manières de faire le calcul.

De la formule qui exprime l'influence de l'erreur de la latitude supposée, on déduit

1°. Que, l'erreur de la latitude calculée est nulle quand une des hauteurs observées est la hauteur méridienne. Ainsi, il convient de faire une observation près du midi.

2°. Que, les distances au méridien étant égales, dans les deux cas, l'erreur du résultat sera plus petite si les deux observations sont de différente espèce, que si elles étoient de la même espèce.

3°. Qu'en supposant l'horaire moyen constant, il convient d'augmenter l'intervalle, quand les observations sont de la même espèce, et le diminuer quand les observations sont de différente espèce.

4°. Qu'en supposant un horaire constant, il convient toujours de diminuer l'autre horaire.

5°. Que, les circonstances les moins favorables pour l'usage de la méthode sont celles, où le soleil passe par le zénith, ou près du zénith.

De la formule qui exprime l'influence de l'erreur de l'intervalle δt , on déduit les mêmes conséquences, à l'exception d'une circonstance particulière de la quatrième ; car dans le cas des observations de la même espèce, et en supposant le petit horaire constant, il conviendrait sous ce rapport d'augmenter le grand horaire pour diminuer l'erreur de la latitude calculée.

On doit cependant remarquer que, quoique, en augmentant l'intervalle, l'on diminue l'influence d'une erreur supposée dans cet élément, par un effet de cette même augmentation, on augmente aussi la probabilité de commettre une erreur plus considérable dans la mesure du tems écoulé. Il me paroît, donc, toutes considérations faites, qu'on peut adopter les règles précédentes généralement.

Voyons à présent quelle est l'influence des erreurs qu'on peut commettre dans les hauteurs du soleil.

En différenciant $\sin. m = \frac{\sin. a - \sin. a'}{2 \sin. \frac{1}{2} t \cos. d \cos. l}$, on aura

$$\delta m = \frac{\delta a \cos. a}{2 \cos. m \sin. \frac{1}{2} t \cos. d \cos. l}; \text{ et par conséquent } \delta b = \frac{\delta a \cos. a}{2 \cos. m \sin. \frac{1}{2} t \cos. d \cos. l}$$

$$\text{ou } \delta b = \frac{\delta a \cos. a}{(\sin. b' - \sin. b) \cos. d \cos. l}; \text{ et } \delta b' = \frac{\delta a \cos. a}{2 \cos. m \sin. \frac{1}{2} t \cos. d \cos. l}$$

$$\text{ou } \delta b' = \frac{\delta a \cos. a}{(\sin. b' - \sin. b) \cos. d \cos. l}$$

En différenciant l'équation

$$\cos. (d \sim L) = \sin. a + \sin. v. b \cos. d \cos. l,$$

$$\text{on aura } \mp \delta L = - \frac{\delta a \cos. a + \delta b \sin. b \cos. d \cos. l}{\sin. (d \sim L)};$$

ce qui, en substituant la valeur de b trouvée ci-dessus, donne

$$\mp \delta L = - \frac{\delta a \cos. a \sin. b'}{(\sin. b' - \sin. b) \sin. (d \sim L)}. \text{ Expression de l'erreur résultante de l'erreur commise dans la grande hauteur, en faisant le calcul relativement à cette hauteur.}$$

En prenant l'équation

$$\cos. (d \sim L) = \sin. a' + \sin. v. b' \cos. d \cos. l$$

$$\text{on aura } \mp \delta L = - \frac{\delta b' \sin. b' \cos. d \cos. l}{\sin. (d \sim L)},$$

$$\text{et par conséquent } \mp \delta L = - \frac{\delta a \cos. a \sin. b'}{(\sin. b' - \sin. b) \sin. (d \sim L)}.$$

Expression de l'influence d'une erreur commise dans la grande hauteur, en calculant par la petite hauteur.

L'influence d'une erreur δa est donc la même dans les deux manières de faire le calcul.

Si l'on suppose une erreur $\delta a'$ dans la petite hauteur, on trouvera aussi, en suivant le même procédé, $\delta L = \frac{\delta a' \cos. a' \sin. b}{(\sin. b' - \sin. b) \sin. (d-l)}$

pour l'expression de l'erreur du résultat, soit qu'on fasse le calcul par la grande hauteur, ou par la petite hauteur.

En supposant la même erreur dans les deux hauteurs, on voit que l'erreur résultante de la grande hauteur, est à l'erreur résultante de la petite hauteur, comme $\cos. a \sin. b'$, à $\cos. a' \sin. b$, ou (parceque nous avons représenté par e l'azimuth correspondant à a , et par e' l'azimuth correspondant à a' , et considérant que $\frac{\cos. d \sin. b}{\sin. e} = \cos. a$ et $\frac{\cos. d \sin. b'}{\sin. e'} = \cos. a'$) comme $\sin. e'$, à $\sin. e$. Ainsi, l'influence d'une erreur supposée dans les deux hauteurs sera en raison inverse des sinus des azimuths.

La formule $\sin. M = \frac{\cos. \frac{1}{2} (a+a') \sin. \frac{1}{2} (a-a')}{\sin. \frac{1}{2} l \cos. d \cos. l}$ est une équation de condition, qui suppose que la déclinaison du soleil et la latitude géographique sont les mêmes pour les deux observations. Nous avons donné des formules pour corriger le résultat des erreurs qui dérivent de cette fausse supposition dans tous les cas du problème; et ces corrections pourront se trouver facilement à l'aide des expressions établies réduites en tables. Au défaut de ces moyens, on pourra réduire une des hauteurs à celle qu'on auroit observé dans le lieu où l'on a observé l'autre, comme on le pratique ordinairement dans la méthode de **DOUWES**. Mais, quoique l'identité des deux latitudes ait lieu alors, on n'évite pas pour cela l'erreur résultante du changement en déclinaison. Il s'agit à présent d'examiner l'influence de chacune de ces causes.

La correction qu'on doit appliquer à la distance méridienne, ou à la latitude calculée, en raison de la variation de la latitude est $= \frac{\Delta l \sin. b (\tan. d \cot. l - \cos. b')}{(\sin. b' - \sin. b) (\tan. d \cot. l - 1)}$, en calculant par la grande hauteur. Par conséquent, l'erreur qu'on commettra, en négligeant cette correction, sera nulle, ou négligeable, quand on aura fait une observation à midi, ou très près du midi.

La même erreur sera aussi nulle, quand l'observation de la petite hauteur aura été faite dans le premier vertical, car alors $\tan. d \cot. l = \cos. b'$.

En faisant le calcul par la petite hauteur la correction qu'on doit appliquer est $= \frac{\Delta l \sin. b' (\cos. b - \tan. d \cot. l)}{(\sin. b' - \sin. b) (\tan. d \cot. l - 1)}$. L'erreur qu'on commettra, en la négligeant, ne sera donc pas nulle dans les circonstances générales du problème; car b' aura ordinairement une valeur considérable, et l'égalité $\cos. b = \tan. d \cot. l$ n'aura pas lieu quand on fera l'observation de la grande hauteur près du midi.

L'erreur qui résulte de négliger la variation de la déclinaison est $= \frac{\Delta d \sin. b (\cot. d \tan. l - \cos. b')}{(\sin. b' - \sin. b) (\cot. d \tan. l - 1)}$, en calculant par la grande hauteur; et cette erreur sera nulle, ou négligeable, quand une des observations aura été faite à midi, ou près du midi.

La même erreur deviendra aussi nulle quand l'angle parallactique, ou de variation, à l'instant de l'observation de la petite hauteur, sera droit; car alors $\cot. d \tan. l = \cos. b'$.

L'erreur du résultat, en calculant par la petite hauteur, est $= \frac{\Delta d \sin. b' (\cos. b - \cot. d \tan. l)}{(\sin. b' - \sin. b) (\cot. d \tan. l - 1)}$. Par où l'on voit, que cette erreur sera plus grande que la précédente dans les circonstances générales du problème.

Ces réflexions rendent préférable le calcul, relativement à

la grande hauteur. Elles prouvent aussi, que, quand on aura pris une hauteur près du midi (ce qu'il convient de faire dans tous les cas possibles), on pourra se dispenser de réduire l'une des deux hauteurs à celle qu'on auroit observé dans le lieu où l'on observa l'autre.

Par la même raison, quand on employera la méthode de corriger une des hauteurs, on pourra établir comme principe général, qu'on réduise la petite hauteur à celle qui conviendrait au lieu où l'on a observé l'autre; car les circonstances qui pourroient le modifier ne sont pas assez importantes pour passer par l'inconvénient de compliquer avec des exceptions les règles de la pratique. Cependant, pour examiner toutes les circonstances de cette solution du problème, nous déterminerons les erreurs qui résultent dans la hauteur réduite, des erreurs qui peuvent affecter les élémens qu'on emploie dans la réduction.

Supposons la distance directe entre les lieux des deux observations = n , l'angle formé par l'azimuth du soleil et l'aire de vent qui conduit du lieu de la grande hauteur au lieu de l'autre = r , et l'angle formé par l'azimuth et l'aire de vent dans le lieu de la petite hauteur = r' . On aura $n \cos. r$ pour la réduction de la grande hauteur, et $n \cos. r'$ pour la réduction de la petite hauteur.

En supposant une certaine erreur dans la mesure de l'aire de vent, on aura pour les erreurs résultantes δa , $\delta a'$ dans les hauteurs, $\delta a = -\delta r n \sin. r$, et $\delta a' = -\delta r' n \sin. r'$. Mais (en représentant l'erreur de la latitude calculée par $\delta L'$, quand on opère relativement à la petite hauteur), on a trouvé ci-de-

$$\text{vant } \delta L = \frac{\delta a \cos. a \sin. b'}{(\sin. b' - \sin. b) \sin. (d \sim l)}, \text{ et } \delta L' = \frac{\delta a' \cos. a' \sin. b}{(\sin. b' - \sin. b) \sin. (d \sim l)}.$$

Donc, en substituant les expressions précédentes, on déduira

$$\delta L : \delta L' : : \delta r n \sin. r \cos. a \sin. b' : \delta r' n \sin r' \cos. a' \sin. b$$

et (parce que l'on suppose $\delta r = \delta r'$)

$$\delta L : \delta L' : : \sin. r \sin. e' : \sin. r' \sin. e.$$

Les erreurs qu'on doit craindre de l'usage du Compas dans les observations des azimuths sont comme les tangentes des hauteurs du soleil (voyez le Mémoire de M. BOUGUER, sur les meilleurs moyens d'observer en mer la déclinaison magnétique; *Prix de l'Académie des Sciences de Paris pour 1731*). Faisons donc, pour ce cas, $\delta r = B \tan. a$, et $\delta r' = B \tan. a'$. Par conséquent $\delta a = \delta r n \sin. r = n B \tan. a \sin r$, et $\delta a' = \delta r' n \sin. r' = n B \tan. a' \sin. r'$; et, en substituant ces expressions dans les formules ci-dessus, on déduira

$$\delta L : \delta L' : : n B \tan. a \cos. a \sin. r \sin. b' : n B \tan. a' \cos. a' \sin. r' \sin. b$$

et $\delta L : \delta L' : : \sin. a \sin. r \sin. b' : \sin. a' \sin. r' \sin. b$.

Pour déterminer l'influence d'une erreur commise dans la distance directe, on a $\delta a = \delta n \cos. r$, et $\delta a' = \delta n \cos. r'$; et par conséquent, en substituant dans les formules ci-dessus,

$$\delta L : \delta L' : : \delta n \cos. r \cos. a \sin. b' : \delta n \cos. r' \cos. a' \sin. b$$

c'est-à-dire $\delta L : \delta L' : : \cos. r \sin. e' : \cos. r' \sin. e$.

Il convient ici de faire une réflexion, par laquelle je terminerai cet article. Les formules que nous avons trouvé pour exprimer l'influence des erreurs sont relatives au résultat qu'on obtient par le calcul d'une latitude supposée. Mais, quand par la méthode ci-dessus, ou par la répétition du calcul, ou par quelque autre procédé, on procure l'identité de la latitude supposée et de la latitude calculée, le cas est différent, et les équations établies ne sauroient donner la valeur exacte de l'erreur du résultat. Si une des données du problème est fausse, on sent, que par la nature de ces espèces de méthodes, il faudra

employer aussi une latitude fausse pour compenser cet effet, et la faire convenir avec la latitude calculée. Généralement parlant, on pourroit dire que quand les circonstances seront favorables pour diminuer l'influence de l'erreur de la donnée, l'erreur dans la latitude supposée nécessaire pour produire l'identité sera aussi moins considérable; mais l'expression juste ne sera pas celle que nous avons déduite. Pour trouver les formules qui conviennent alors, on devroit suivre un autre procédé, dont je vais donner un exemple, en considérant l'erreur de l'intervalle.

L'erreur de la latitude calculée composée de celles qu'on peut attribuer à l'intervalle, et à la latitude supposée, est

$$\mp \delta L = \frac{\delta l' (\cos. m - \cos. \frac{1}{2} t)}{\cos. m (\tan. d \cot. l \sim 1)} + \frac{\delta t \sin. b \sin. b'}{2 \cos. m \sin. \frac{1}{2} t (\tan. d \sim \tan. l)}.$$

Mais, pour faire convenir la latitude calculée avec la latitude supposée, il faut que δL soit $= \delta l''$, donc

$$\mp \delta l'' = \frac{\delta l' \tan. l (\cos. m - \cos. \frac{1}{2} t)}{\cos. m (\tan. d \sim \tan. l)} + \frac{\delta t \sin. b \sin. b'}{2 \cos. m \sin. \frac{1}{2} t (\tan. d \sim \tan. l)}.$$

Par conséquent,

$$\mp 2 \delta l' \cos. m \sin. \frac{1}{2} t (\tan. d \sim \tan. l) - 2 \delta l'' \tan. l \sin. \frac{1}{2} t (\cos. m - \cos. \frac{1}{2} t) = \delta t \sin. b \sin. b'$$

c'est-à-dire

$$2 \delta l'' \cos. m \sin. \frac{1}{2} t (\tan. l - \tan. d) - 2 \delta l'' \tan. l \sin. \frac{1}{2} t (\cos. m - \cos. \frac{1}{2} t) = \sin. b \sin. b'$$

$$\text{d'où l'on déduit } \delta l'' = \frac{\delta t \sin. b \sin. b'}{\tan. l \sin. t - 2 \tan. d \cos. m \sin. \frac{1}{2} t}$$

$$\text{ou } \delta l'' = \frac{\delta t \sin. b \sin. b'}{\tan. l (\sin. b' \cos. b - \cos. b' \sin. b) - \tan. d (\sin. b' - \sin. b)}$$

$$\text{et finalement } \delta l'' = \frac{\delta t}{\tan. l (\cot. b - \cot. b') - \tan. d (\operatorname{cosec} b - \operatorname{cosec} b')}$$

Expressions égales à celles qu'on trouve pour la méthode directe.

Nous remarquerons, au reste, que les équations relatives à Δd , et Δl , qui sont celles qu'on doit employer d'une manière absolue, pourront être appliquées immédiatement au résultat du calcul fait par chaque supposition séparément.

Méthode indirecte, en déduisant premièrement le plus grand Angle horaire.

Avec la latitude estimée, et les autres données relatives au lieu de la petite hauteur, on calculera le grand angle horaire (que nous représenterons par H'), par l'une des formules suivantes (Voyez ci-après la démonstration de ces formules),

$$\sin. v. H' = \frac{z \cos. \frac{1}{2} (D' + l'' + a') \sin. \frac{1}{2} (D + l'' - a)}{\cos. l'' \sin. D'}$$

$$\sin. v. H' = \frac{\sqrt{\text{susin. } v. (D' + l'' + a') \sin. v. (D' + l'' - a)}}{\cos. l'' \sin. D'}$$

$$\sin. \frac{1}{2} H' = \sqrt{\frac{\cos. \frac{1}{2} (D' + l'' + a') \sin. \frac{1}{2} (D' + l'' - a)}{\cos. l'' \sin. D'}}$$

ou par toute autre formule de celles que fournit la Trigonométrie Sphérique pour le calcul de l'angle horaire. Et l'on déduira le petit angle horaire $H = H' \sim t$.

Après avoir déterminé le petit horaire, on conclura la distance méridienne du soleil au zénith, et la latitude, par le moyen des formules que nous avons établi pour la même opération dans la méthode précédente, en employant les données relatives au lieu de la grande hauteur.

On pourra aussi trouver la latitude exacte, en faisant le calcul de cette méthode avec des latitudes supposées un peu différentes des latitudes estimées, imitant le procédé que nous avons expliqué ci-dessus.

Après avoir considéré avec tant de détail la méthode qui précède, nous ne ferons qu'indiquer les formules qu'on pourra tirer des équations fondamentales de celle que nous avons à présent sous les yeux, en y ajoutant seulement quelques réflexions générales.

On aura, pour exprimer la relation entre l'erreur de la latitude estimée et l'erreur résultante dans la latitude calculée,

$$\begin{aligned} \mp \delta L &= \frac{\delta p' (\sin. H - \sin. t - \tan. d \cot. p' \sin. H)}{\sin. H' (\tan. d \cot. p' \sim 1)} \\ \mp \delta L &= \frac{\delta p' (z \sin. \frac{1}{2} H \cos. \frac{1}{2} (H' + t) - \tan. d \cot. p' \sin. H)}{\sin. H' (\tan. d \cot. p' \sim 1)} \end{aligned}$$

On voit par cette expression, que l'erreur du résultat est nulle, ou très petite, quand on observe la grande hauteur à midi, ou près du midi.

De la même formule on peut déduire les circonstances qui sont avantageuses pour que la latitude calculée s'approche de la latitude vraie; mais je ne m'arrêterai pas, à présent, à les énoncer particulièrement.

Si l'on suppose une erreur δt dans l'intervalle, on trouvera que l'erreur résultante dans la latitude calculée est

$$\mp \delta L = \frac{\delta t \sin. b}{\tan. d \sim \tan. l'}. \text{ Cette erreur sera donc nulle, quand on a observé une hauteur à midi. Et l'influence d'une erreur supposée dans l'intervalle sera la même, quels que soient l'intervalle, et la distance à midi de l'observation de la petite hauteur.}$$

L'erreur résultante d'une erreur δa supposée dans la grande hauteur est $\mp \delta L = - \frac{\delta a \cos. a}{\sin. (d \sim l)}$.

Et l'erreur résultante d'une erreur $\delta a'$ supposée dans la petite hauteur

$$\mp \delta L = \frac{\delta a' \cos. a' \sin. b}{\sin. b \sin. (d \sim l)}.$$

Si l'on fait le calcul de la distance méridienne avec la latitude du lieu où l'on a observé la petite hauteur, au lieu d'employer la latitude correspondante à l'autre hauteur, on commettra une

$$\text{erreur } \mp \delta L = \frac{\Delta l \sin. v. b \sin. l \cos d}{\sin. (d \sim l)} = \frac{\Delta l \sin. v. b}{\tan. d \cot. l \sim 1}.$$

Et si l'on fait le calcul de la distance méridienne avec la déclinaison correspondante à la petite hauteur, on commettra une

$$\text{erreur} = \delta L = \frac{\Delta d \sin. v. b \cos. l \sin. d}{\sin. (d \sim l)} = \frac{\Delta d \sin. v. b}{\cot. d \tan. l \sim 1'}$$

Quand on aura pris une hauteur près du midi, on pourra donc faire tout le calcul, en employant la latitude et la déclinaison correspondantes à la petite hauteur; et le résultat donnera avec assez d'exactitude la latitude du lieu où l'on a observé la grande hauteur.

Je dois rappeler ici la dernière réflexion que nous avons faite par rapport à la méthode précédente, car elle a lieu également pour celle-ci. Pour un plus grand éclaircissement, déduisons la relation entre l'erreur du résultat, et une erreur supposée dans l'intervalle, quand on procure l'identité de la latitude estimée, et de la latitude calculée, en employant les formules de la présente solution.

On aura

$$\mp \delta L = \frac{\delta l'' (\sin. b' - \sin. l - \tan. d \cot. l \sin. b)}{\sin. b' (\tan. d \cot. l \sim 1)} + \frac{\delta t \sin. b}{\tan. d \sim \tan. l}$$

D'où, parce que $\delta L = \delta l''$, on déduira

$$\mp \delta l'' \sin. b' (\tan. d \sim \tan. l) - \delta l'' \tan. l (\sin. b' - \sin. l - \tan. d \cot. l \sin. b) = \delta t \sin. b \sin. b'$$

et $-\delta l'' \tan. d \sin. b' + \delta l'' \tan. l \sin. l + \delta l'' \tan. d \sin. b = \delta t \sin. b \sin. b'$

$$\delta l'' = \frac{\delta t \sin. b \sin. b'}{\tan. l \sin. l + \tan. d (\sin. b - \sin. b')}$$

$$\delta l'' = \frac{\delta t \sin. b \sin. b'}{\tan. l (\sin. b' \cos. b - \cos. b' \sin. b) - \tan. d (\sin. b' - \sin. b)}$$

$$\delta l'' = \frac{\delta t}{\tan. l (\cot. b - \cot. b') - \tan. d (\operatorname{cosec.} b - \operatorname{cosec.} b')}$$

Expressions égales à celles que nous avons trouvées par les formules des deux méthodes précédentes.

Méthodes indirectes par des Équations relatives à l'Intervalle.

Avec les latitudes estimées, et les données du problème, on pourra calculer le grand angle horaire (ou celui qui répond à la petite hauteur), et le petit horaire (ou celui qui répond à la grande hauteur). En comparant l'intervalle mesuré par la montre avec l'intervalle qui résulte de ces horaires, on aura une différence δt ; et de-là on déterminera l'équation qu'on doit appliquer à la latitude supposée, en employant la formule

$$\delta l'' = \frac{\delta t}{\tan. l'' (\cot. b - \cot. b') - \tan. d (\operatorname{cosec}. b - \operatorname{cosec}. b')}, \text{ qui se réduit à}$$

$$\delta l'' = \frac{\delta t \sin. b \sin. b'}{\tan. l'' \sin. (b' - b) - 2 \tan. d \sin. \frac{1}{2} (b' - b) \cos. \frac{1}{2} (b' + b)},^* \text{ ou toute}$$

autre formule qui donne la relation entre δt , et $\delta l''$.

Voici une autre méthode qui me paroît préférable. Faites le calcul du petit horaire avec deux latitudes supposées L , L' qui ne diffèrent pas beaucoup de la latitude estimée l'' du lieu où l'on a observé la grande hauteur; et appellons les horaires b , K . Faites aussi le calcul du grand horaire avec deux latitudes supposées qui s'éloignent de la latitude l''' , où l'on a observé la petite hauteur, de la même quantité et dans le même sens que L , L' de l'' ; et appellons ces horaires b' , K' . En représentant

* Mon savant ami le Dr. MASKELYNE a publié depuis très long tems une autre formule qui détermine cette relation en termes des azimuths. En représentant par P l'angle formé par les verticaux de l'astre aux instants des observations, il trouve $\delta l'' = \frac{\delta t \cos. l \sin. c \sin. c'}{\sin. P}$. Notre auteur a déduit cette expression par le moyen des analogies différentielles. Je l'ai démontrée d'une autre manière dans mon Mémoire inséré dans la *Connoissance des Tems pour 1793*.

La même formule a été consultée pour établir les règles données, premièrement dans le *British Mariner's Guide*, et copiées depuis dans les *Requisite Tables*, et dans d'autres ouvrages, pour choisir les circonstances qui conviennent à l'usage de la méthode de DOUWES.

par t l'intervalle qui résulte de la comparaison de b , et K , par t' l'intervalle qui résulte de la comparaison de b' , et K' , et par T l'intervalle mesuré par la montre, on aura $\delta L : \delta L' :: \delta t : \delta t'$

$$\text{et par conséquent} \quad \delta L = \frac{(\delta L \approx \delta L') \delta t}{\delta t \approx \delta t'}$$

$$\text{qui se réduit à} \quad \delta L = \frac{(L \sim L') (t \sim T)}{t \sim t'}$$

C'est l'équation qu'on doit appliquer à la latitude supposée L . La latitude vraie sera comprise, ou non, entre les deux latitudes supposées, selon que les deux intervalles calculés différeront de l'intervalle observé dans des sens opposés, ou dans le même sens. Et dans le dernier cas, la latitude vraie sera plus près de la latitude supposée dont l'intervalle correspondant différera le moins de celui que donne la montre.

Toutes les solutions par des équations relatives à l'intervalle ont, cependant, un grand inconvénient; car elles supposent qu'on connoisse à quel coté du méridien appartient la plus grande hauteur. Et, comme ce cas douteux arrive précisément quand on a observé près du midi, c'est-à-dire, dans les circonstances les plus favorables pour l'exactitude du résultat, je ne crois pas qu'on puisse adopter dans la pratique ces sortes de procédés, surtout, quand on possède d'autres méthodes, qui réunissent toutes les propriétés requises.

La Latitude du Lieu, ainsi que la Hauteur, et la Déclinaison d'un Astre étant données, trouver son Angle horaire.

La Trigonométrie Sphérique donne

$$\cos. b = \frac{\sin. a - \sin. d \sin. l}{\cos. d \cos. l}$$

par conséquent

$$\sin. v. b = 1 - \frac{\sin. a - \sin. d \sin. l}{\cos. d \cos. l}$$

$$\sin. v. b = \frac{\cos. d \cos. l + \sin. d \sin. l - \sin. a}{\cos. d \cos. l}$$

$$\sin. v. b = \frac{\cos. (d \sim l) - \sin. a}{\cos. d \cos. l}$$

$$\sin. v. b = \frac{\cos. (d \sim l) - \cos. (90^\circ - a)}{\cos. d \cos. l}$$

$$\sin. v. b = \frac{2 \sin. \frac{1}{2} (90^\circ - a + (d \sim l)) \sin. \frac{1}{2} (90^\circ - a - (d \sim l))}{\cos. d \cos. l}$$

Expression propre pour employer les logarithmes sinus-verses, et ceux des doubles-sinus.

On a aussi

$$\sin. v. b = \sqrt{\frac{\cos. v. (a - (d \sim l)) \cos. v. (a + (d \sim l))}{\cos. d \cos. l}}$$

Pour employer les logarithmes sinus et tangentes, on déduit

$$\sin. \frac{1}{2} b = \sqrt{\frac{\sin. \frac{1}{2} (90^\circ - a + (d \sim l)) \sin. \frac{1}{2} (90^\circ - a - (d \sim l))}{\cos. d \cos. l}}$$

Cette formule, et l'avant-dernière, ont un avantage assez considérable, quand on emploie des tables, comme celles de SHERWIN et de GARDINER, où il faut prendre des parties proportionnelles; car elles sont additives pour les sinus et les sécantes, et par conséquent on peut les mettre au dessous des logarithmes correspondants aux arguments les plus proches, et faire ensuite une addition de tous ces nombres.

En employant les tables de TAYLOR, le calcul seroit un peu plus court par la formule suivante.

$$\sin. \frac{1}{2} b = \sqrt{\frac{\cos. \frac{1}{2} (90^\circ + a + (d \sim l)) \cos. \frac{1}{2} (90^\circ + a - (d \sim l))}{\cos. d \cos. l}}$$

Si l'on substitue l'expression de la distance polaire D, au lieu de $90^\circ \sim d$, on aura

$$\sin \frac{1}{2} b = \sqrt{\frac{\cos. \frac{1}{2} (D + l + a) \sin. \frac{1}{2} (D + l - a)}{\cos. l \sin. D}}$$

Celle-ci est la formule de M. DE BORDA, qu'on trouve dans différents ouvrages.

La Latitude géographique, ainsi que la Déclinaison, et l'Angle horaire d'un Astre étant donnés, trouver sa Hauteur.

Nous avons trouvé

$$\cos. (d \sim l) = \sin. a + \sin. v. b \cos. d \cos. l$$

Par conséquent

$$\sin. a = \cos. (d \sim l) - \sin. v. b \cos. d \cos. l$$

$$\text{et } \sin. a = \cos. (d \sim l) \left(1 - \frac{\sin. v. b \cos. d \cos. l}{\cos. (d \sim l)} \right)$$

En faisant donc $\frac{\sin. v. b \cos. d \cos. l}{\cos. (d \sim l)} = \sin. v. N$, on aura

$$\sin. a = \cos. (d \sim l) \cos. N.$$

Et l'on voit, que quand $\sin. v. N$ est plus grand que le rayon, l'astre est sous l'horizon, et la hauteur calculée est négative.

Nous avons trouvé aussi

$$\sin. v. (d \sim l) = \cos. v. a - \sin. v. b \cos. d \cos. l$$

Par conséquent

$$\sin. \frac{1}{2} (d \sim l) = \cos. \frac{1}{2} (90^\circ + a) - \sin. \frac{1}{2} b \cos. d \cos. l$$

$$\text{et } \cos. \frac{1}{2} (90^\circ + a) = \sin. \frac{1}{2} (d \sim l) \sqrt{1 + \frac{\sin. \frac{1}{2} b \cos. d \cos. l}{\sin. \frac{1}{2} (d \sim l)}}$$

En faisant donc $\frac{\sin. \frac{1}{2} b \cos. d \cos. l}{\sin. \frac{1}{2} (d \sim l)} = \tan. N$, on aura

$$\cos. \frac{1}{2} (90^\circ + a) = \frac{\sin. \frac{1}{2} (d \sim l)}{\cos. N}.$$

De l'équation établie ci-dessus relative à $\sin. v. (d \sim l)$ on tire également

$$\sin. v. (d \sim l) = \cos. v. a + \sin. v. b \cos. d \cos. l$$

$$\cos. \frac{1}{2} (d \sim l) = \sin. \frac{1}{2} (90^\circ + a) + \sin. \frac{1}{2} b \cos. d \cos. l$$

$$\sin. \frac{1}{2} (90^\circ + a) = \cos. \frac{1}{2} (d \sim l) \sqrt{1 - \frac{\sin. \frac{1}{2} b \cos. d \cos. l}{\cos. \frac{1}{2} (d \sim l)}}$$

En faisant donc $\frac{\sin. \frac{1}{2} b \sqrt{\cos. d \cos. l}}{\cos. \frac{1}{2} (d \sim l)} = \cos. N$, on aura
 $\sin. \frac{1}{2} (90^\circ + a) = \cos. \frac{1}{2} (d \sim l) \cos. N$.

Cette formule est plus commode pour la pratique que la précédente relative à $\cos. \frac{1}{2} (90^\circ + a)$. Et toutes les deux sont propres pour faire le calcul par les tables des logarithmes sinus et tangentes.

SECONDE PARTIE.

*La Distance apparente de la Lune au Soleil, ou à une Étoile, et les Hauteurs des deux Astres étant données, trouver leur Distance corrigée des Effets de la Réfraction et de la Parallaxe.**

Les mesures prises par le Bureau des Longitudes de la Grande Bretagne, pour faire calculer et publier un Almanach Nautique, avec les distances de la lune au soleil, et à plusieurs étoiles, forment une époque remarquable dans l'histoire de la Navigation, et l'utilité reconnue de cet établissement fait un grand honneur à la nation qui a fourni par là des moyens de sureté aux Navigateurs de tout le Monde. Quand ces Ephemerides eurent annoncé les élémens nécessaires avec assez d'anticipation et d'exactitude, il devint important de trouver des formules propres pour abréger la réduction des distances lunaires apparentes, afin de les dégager des effets de la réfraction et de la parallaxe. L'Abbé de

* La grande utilité de ce problème a engagé un grand nombre de géometres et d'astronomes à s'en occuper, et l'on doit des solutions à l'Abbé de LA CAILLE, au Dr. MASKELYNE, au grand EULER, à M. DE BORDA, à M. LEXELL, à M. DE LA GRANGE, à M. FUSSE, à M. KRAFFT, à &c. &c. &c. J'ai eu la curiosité de suivre les progrès de ces recherches,* mais l'histoire en est trop longue pour l'insérer ici, et je dois la remettre à une autre occasion.

la CAILLE avoit donné une méthode d'approximation; mais elle se borne aux équations qui dépendent des premières dimensions, ce qui n'est pas assez exact pour la pratique. Le Dr. MASKELYNE, à qui l'Astronomie Nautique a tant d'obligations, est le premier qui a perfectionné la solution approchée, en la poussant jusqu'à l'exactitude, et en inventant des formules pour abréger les opérations numériques. On a donné depuis différentes formes aux expressions des corrections qu'on doit appliquer à la distance observée; mais le désir de produire des nouveautés s'est souvent emparé des personnes qui manquoient de tact et de principes, et on les a vu étaler à ce sujet des règles fausses ou inexactes, qui quelquefois ont séduit les Pilotes. Tous ceux qui cultivent l'étude de la Navigation, ont sans doute rencontré des cas pareils, et se sont vu forcés quelques fois d'examiner ces idées empiriques. Quant à moi, le regret du tems que j'ai perdu à considérer chaque solution particulière qui m'a tombé sous les mains, m'a fait penser à la fin à établir des formules générales qui pussent servir de modèle, ou de termes de comparaison, pour déterminer si une méthode quelconque est vraie, ou fausse, ou bien à quel point elle est exacte. Ce projet m'a premièrement engagé dans l'analyse de la solution par approximation; et c'est aussi sous ce point de vue principal que j'ai rédigé cet article de mes Recherches.

On a aussi cherché des méthodes directes pour résoudre ce problème. Mr. DUNTHORNE en publia une de cette espèce dans les *Requisite Tables* de 1767; mais ses opérations exigent l'usage combiné des nombres naturels et artificiels. M. DE BORDA est le premier à qui l'on est redevable d'un procédé direct pour faire ce calcul par le seul moyen des logarithmes. On a depuis proposé quelques autres méthodes; mais ne pou-

vant pas les détailler ici, je me contenterai de faire ci-après mention des principales.

Je dois remarquer, que je me suis borné aux expressions qu'on peut calculer, soit par les logarithmes, soit par les nombres naturels, et que je ne me suis pas occupé de celles dont les opérations exigeroient la combinaison de ces deux moyens de calcul.

Je donnerai d'abord la théorie générale des méthodes directes, et je m'occuperai ensuite de l'analyse des solutions par approximation.

Soient, pour cette Seconde Partie, a la hauteur apparente, et A la hauteur vraie de la lune, b la hauteur apparente, et H la hauteur vraie du soleil, ou de l'étoile, d la distance apparente, et D la distance vraie des deux astres.

Méthodes Directes.

En représentant par Z l'angle au zénith, formé par les verticaux des deux astres, et considérant le triangle formé par la distance, et les compléments des hauteurs apparentes, on aura

$$\cos. D = \cos. Z \cos. A \cos. H + \sin. A \sin. H$$

Et considérant le triangle formé par les élémens vrais

$$\cos. Z = \frac{\cos. d - \sin. a \sin. b}{\cos. a \cos. b}$$

Par conséquent, en substituant cette expression dans la première équation, on déduira

$$\cos. D = (\cos. d - \sin. a \sin. b) \frac{\cos. A \cos. H}{\cos. a \cos. b} + \sin. A \sin. H.$$

Voilà l'expression générale de la relation entre la distance vraie et les données du problème. Il s'agit de chercher des formules propres pour l'usage des logarithmes, ou des nombres naturels.

La quantité $\sin. a \sin. b$ est $= -\cos. (a+b) + \cos. a \cos. b$, ou bien $= \cos. (a \sim b) - \cos. a \cos. b$. On pourra, donc, substituer l'une ou l'autre de ces expressions; et de-là résultent deux suites de transformations de l'équation fondamentale, la première par les sommes, la seconde par les différences. De chaque équation de $\cos. D$ on peut aussi conclure une valeur de $\sin. v. D$, et une valeur correspondante de $\sin. v. D$; car $\sin. v. D = 1 - \cos. D$, et $\sin. v. D = 1 + \cos. D$; et de cette manière les solutions se ramifient encore en deux autres branches. Nous suivrons cette marche pour parvenir aux formules que nous cherchons.

En substituant la première expression $\sin. a \sin. b = -\cos. (a+b) + \cos. a \cos. b$, on aura, par les sommes,

$$\cos. D = \left(\cos. d + \cos. (a+b) - \cos. a \cos. b \right) \frac{\cos. A \cos. H}{\cos. a \cos. b} + \sin. A \sin. H.$$

$$\cos. D = \left(\cos. d + \cos. (a+b) \right) \frac{\cos. A \cos. H}{\cos. a \cos. b} - \cos. A \cos. H + \sin. A \sin. H.$$

$$\cos. D = \left(\cos. d + \cos. (a+b) \right) \frac{\cos. A \cos. H}{\cos. a \cos. b} - \cos. (A+H)$$

$$\cos. D = 2 \cos. \frac{1}{2} (d+a+b) \cos. \frac{1}{2} (d \sim (a+b)) \frac{\cos. A \cos. H}{\cos. a \cos. b} - \cos. (A+H)$$

Par conséquent

1re Formule.

$$\cos. D = 2 \cos. \frac{1}{2} (d+a+b) \cos. \frac{1}{2} (d \sim (a+b)) \frac{\cos. A \cos. H}{\cos. a \cos. b} \left(1 - \frac{\cos. (A+H)}{2 \cos. \frac{1}{2} (d+a+b) \cos. \frac{1}{2} (d \sim (a+b)) \frac{\cos. A \cos. H}{\cos. a \cos. b}} \right)$$

En faisant, donc,

$$\frac{\cos. (A+H)}{2 \cos. \frac{1}{2} (d+a+b) \cos. \frac{1}{2} (d \sim (a+b)) \frac{\cos. A \cos. H}{\cos. a \cos. b}} = \sin. v. N.$$

on aura

$$\cos. D = 2 \cos. \frac{1}{2} (d+a+b) \cos. \frac{1}{2} (d \sim (a+b)) \frac{\cos. A \cos. H}{\cos. a \cos. b} \cos. N.$$

La présente méthode exige quelques modifications, car

cos. (A + H) change de signe quand A + H excède 90°. Mais je ne m'arrêterai pas à détailler les distinctions des cas qu'on devrait faire dans cette formule, et dans quelques unes de celles qui y dérivent, car cette seule circonstance suffit pour les abandonner dans la pratique.

De l'expression précédente de cos. D on tire

$$\sin.v.D = \sin.v.(A+H) - 2\cos.\frac{1}{2}(d+a+b)\cos.\frac{1}{2}(d\sim(a+b))\frac{\cos.A\cos.H}{\cos.a\cos.b}$$

Cette équation fournit les trois formules suivantes.

2me Formule.

En faisant

$$2\cos.\frac{1}{2}(d+a+b)\cos.\frac{1}{2}(d\sim(a+b))\frac{\cos.A\cos.H}{\cos.a\cos.b} = \sin.v.N$$

ou bien,

$$\sqrt{\cos.\frac{1}{2}(d+a+b)\cos.\frac{1}{2}(d\sim(a+b))\frac{\cos.A\cos.H}{\cos.a\cos.b}} = \cos.N'$$

$$\text{on aura } \sin.v.D = \sqrt{\sin.v.(N+A+H)\sin.v.(N-(A+H))}$$

$$\text{ou } \sin.v.D = 2\sin.(N'+\frac{1}{2}(A+H))\sin.(N'-\frac{1}{2}(A+H))$$

3me Formule.

En faisant

$$2\cos.\frac{1}{2}(d+a+b)\cos.\frac{1}{2}(d\sim(a+b))\frac{\cos.A\cos.H}{\cos.a\cos.b} = \sin.v.N$$

ou bien,

$$\sqrt{\cos.\frac{1}{2}(d+a+b)\cos.\frac{1}{2}(d\sim(a+b))\frac{\cos.A\cos.H}{\cos.a\cos.b}} = \sin.N'$$

$$\text{on aura } \sin.v.D = \sqrt{\sin.v.(N+A+H)\sin.v.(N\sim(A+H))}$$

$$\text{ou } \sin.v.D = 2\cos.(N'+\frac{1}{2}(A+H))\cos.(N'\sim\frac{1}{2}(A+H))$$

4me Formule.

L'équation précédente se réduit à

$$\sin.v.D = \text{susin.v.}(A+H) \left(1 - \frac{2 \cos.\frac{1}{2}(d+a+b) \cos.\frac{1}{2}(d\sim(a+b)) \cos.A \cos.H}{\text{susin.v.}(A+H) \cos.a \cos.b} \right)$$

$$\text{En faisant, donc, } \frac{2 \cos.\frac{1}{2}(d+a+b) \cos.\frac{1}{2}(d\sim(a+b)) \cos.A \cos.H}{\text{susin.v.}(A+H) \cos.a \cos.b} = \cos.N$$

$$\text{on aura } \sin.v.D = \text{susin.v.}(A+H) \sin.v.N.$$

5me Formule.

De la même expression de $\cos.D$ on déduit aussi

$$\text{susin.v.D} = \sin.v.(A+H) + 2 \cos.\frac{1}{2}(d+a+b) \cos.\frac{1}{2}(d\sim(a+b)) \frac{\cos.A \cos.H}{\cos.a \cos.b}$$

$$\text{susin.v.D} = \sin.v.(A+H) \left(1 + \frac{2 \cos.\frac{1}{2}(d+a+b) \cos.\frac{1}{2}(d\sim(a+b)) \cos.A \cos.H}{\sin.v.(A+H) \cos.a \cos.b} \right)$$

$$\text{En faisant, donc, } \frac{2 \cos.\frac{1}{2}(d+a+b) \cos.\frac{1}{2}(d\sim(a+b)) \cos.A \cos.H}{\sin.v.(A+H) \cos.a \cos.b} = \cos.N$$

$$\text{on aura } \text{susin.v.D} = \sin.v.(A+H) \text{susin.v.N.}$$

Je remarquerai ici, qu'on pourroit substituer dans les formules de ces méthodes $\sqrt{\text{susin.v.}(d+a+b) \text{susin.v.}(d\sim(a+b))}$, à la place de $2 \cos.\frac{1}{2}(d+a+b) \cos.\frac{1}{2}(d\sim(a+b))$, pour employer les susinus-verse au lieu des ccsinus.

En substituant la seconde expression $\cos.(a\sim b) - \cos.a \cos.b = \sin.a \sin.b$, dans la formule fondamentale, on aura par les différences

$$\cos.D = \left(\cos.d - \cos.(a\sim b) + \cos.a \cos.b \right) \frac{\cos.A \cos.H}{\cos.a \cos.b} + \sin.A \sin.H$$

$$\cos.D = \left(\cos.d - \cos.(a\sim b) \right) \frac{\cos.A \cos.H}{\cos.a \cos.b} + \cos.(A\sim H)$$

$$\cos.D = \cos.(A\sim H) - 2 \sin.\frac{1}{2}(d+(a\sim b)) \sin.\frac{1}{2}(d-(a\sim b)) \frac{\cos.A \cos.H}{\cos.a \cos.b}$$

Par conséquent :

6me Formule.

$$\cos. D = \cos. (A \sim H) \left(1 - \frac{2 \sin. \frac{1}{2} (d + (a \sim b)) \sin. \frac{1}{2} (d - (a \sim b)) \cos. A \cos. H}{\cos. (A \sim H) \cos. a \cos. b} \right)$$

En faisant, donc, $\frac{2 \sin. \frac{1}{2} (d + (a \sim b)) \sin. \frac{1}{2} (d - (a \sim b)) \cos. A \cos. H}{\cos. (A \sim H) \cos. a \cos. b} = \sin. v. N$

on aura $\cos. D = \cos. (A \sim H) \cos. N.$

Et D sera plus grand ou moindre que 90°, selon que sin. v. N sera plus grand ou plus petit que le rayon.

De l'expression précédente de cos. D on tire ce qui suit.

7me Formule.

$$\sin. v. D = \sin. v. (A \sim H) + 2 \sin. \frac{1}{2} (d + (a \sim b)) \sin. \frac{1}{2} (d - (a \sim b)) \frac{\cos. A \cos. H}{\cos. a \cos. b}$$

$$\sin. v. D = \sin. v. (A \sim H) \left(1 + \frac{2 \sin. \frac{1}{2} (d + (a \sim b)) \sin. \frac{1}{2} (d - (a \sim b)) \cos. A \cos. H}{\sin. v. (A \sim H) \cos. a \cos. b} \right)$$

En faisant, donc, $\frac{2 \sin. \frac{1}{2} (d + (a \sim b)) \sin. \frac{1}{2} (d - (a \sim b)) \cos. A \cos. H}{\sin. v. (A \sim H) \cos. a \cos. b} = \cos. N$

on aura $\sin. v. D = \sin. v. (A \sim H) \text{ susin. v. } N.$

De la même expression de cos. D on déduit aussi

$$\text{susin. v. } D = \text{susin. v. } (A \sim H) - 2 \sin. \frac{1}{2} (d + (a \sim b)) \sin. \frac{1}{2} (d - (a \sim b)) \frac{\cos. A \cos. H}{\cos. a \cos. b}$$

Cette équation fournit les trois formules suivantes.

8me Formule.

En faisant

$$2 \sin. \frac{1}{2} (d + (a \sim b)) \sin. \frac{1}{2} (d - (a \sim b)) \frac{\cos. A \cos. H}{\cos. a \cos. b} = \text{susin. v. } N$$

ou bien,

$$\sqrt{\sin. \frac{1}{2} (d + (a \sim b)) \sin. \frac{1}{2} (d - (a \sim b)) \frac{\cos. A \cos. H}{\cos. a \cos. b}} = \cos. N'$$

on aura $\text{susin. v. } D = \sqrt{\sin. v. (N + (A \sim H)) \sin. v. (N - (A \sim H))}$

ou $\text{susin. v. } D = 2 \sin. (N' + \frac{1}{2} (A \sim H)) \sin. (N' - \frac{1}{2} (A \sim H))$

9me Formule.

En faisant

$$2 \sin. \frac{1}{2} (d + (a \sim b)) \sin. \frac{1}{2} (d - (a \sim b)) \frac{\cos. A. \cos. H}{\cos. a \cos. b} = \sin. v. N$$

ou bien,

$$\sqrt{\sin. \frac{1}{2} (d + (a \sim b)) \sin. \frac{1}{2} (d - (a \sim b)) \frac{\cos. A \cos. H}{\cos. a \cos. b}} = \sin. N'$$

on aura

$$\text{susin. v. D} = \sqrt{\text{susin. v. } (N + (A \sim H)) \text{susin. v. } (N \sim (A \sim H))}$$

$$\text{ou } \text{susin. v. D} = 2 \cos. (N' + \frac{1}{2} (A \sim H)) \cos. (N' \sim \frac{1}{2} (A \sim H))$$

10me Formule.

L'équation précédente se réduit à

$$\text{susin. v. D} = \text{susin. v. } (A \sim H) \left(1 - \frac{2 \sin. \frac{1}{2} (d + (a \sim b)) \sin. \frac{1}{2} (d - (a \sim b)) \cos. A \cos. H}{\text{susin. v. } (A \sim H) \cos. a \cos. b} \right)$$

$$\text{En faisant, donc, } \frac{2 \sin. \frac{1}{2} (d + (a \sim b)) \sin. \frac{1}{2} (d - (a \sim b)) \cos. A \cos. H}{\text{susin. v. } (A \sim H) \cos. a \cos. b} = \cos. N$$

$$\text{on aura } \text{susin. v. D} = \text{susin. v. } (A \sim H) \sin. v. N.$$

Je remarquerai, qu'on pourroit aussi substituer dans les quatre formules précédentes $\sqrt{\sin. v. (d + (a \sim b)) \sin. v. (d - (a \sim b))}$ au lieu de $2 \sin. \frac{1}{2} (d + (a \sim b)) \sin. \frac{1}{2} (d - (a \sim b))$.

Les formules que nous venons d'établir sont propres pour le calcul par les logarithmes sinus-verses, et l'on pourroit combiner aussi l'usage des logarithmes doubles-sinus. Cherchons à présent des expressions pour employer seulement les logarithmes sinus et tangentes, tels qu'on les trouve dans les Tables de GARDINER et de TAYLOR.

11^{me} Formule.

Dans la 1^{re} formule on pourroit faire

$$\sqrt{\frac{\cos. (A + H)}{4 \cos. \frac{1}{2} (d + a + b) \cos. \frac{1}{2} (d \sim (a + b)) \frac{\cos. A \cos. H}{\cos. a \cos. b}}} = \sin. N$$

pour avoir

$$\cos. D = 2 \cos. \frac{1}{2} (d + a + b) \cos. \frac{1}{2} (d \sim (a + b)) \frac{\cos. A \cos. H}{\cos. a \cos. b} \cos. 2 N$$

Mais je dois rappeler ici ce que j'ai dit à l'occasion de la 1^{re} méthode.

12^{me} Formule.

En substituant dans la 2^{me} formule $2 \sin. \frac{1}{2} D = \sin. v. D$, et faisant aussi

$$\sqrt{\cos. \frac{1}{2} (d + a + b) \cos. \frac{1}{2} (d \sim (a + b)) \frac{\cos. A \cos. H}{\cos. a \cos. b}} = \cos. N'$$

on aura

$$\sin. \frac{1}{2} D = \sqrt{\sin. \left(N' + \frac{1}{2} (A + H) \right) \sin. \frac{1}{2} \left(N' \sim \frac{1}{2} (A + H) \right)}$$

13^{me} Formule.

En faisant, comme dans la 3^{me} méthode,

$$\sqrt{\cos. \frac{1}{2} (d + a + b) \cos. \frac{1}{2} (d \sim (a + b)) \frac{\cos. A \cos. H}{\cos. a \cos. b}} = \sin. N'$$

on aura

$$\sin. \frac{1}{2} D = \sqrt{\cos. \left(N' + \frac{1}{2} (A + H) \right) \cos. \left(N' \sim \frac{1}{2} (A + H) \right)}$$

14^{me} Formule.

En substituant dans la 4^{me} formule

$$2 \sin. \frac{1}{2} D = \sin. v. D, \text{ et } 2 \cos. \frac{1}{2} (A + H) = \text{sin. } v. (A + H),$$

on tire

$$\sin. \frac{2}{3} D = \cos. \frac{2}{3} (A+H) \left(1 - \frac{\cos. \frac{3}{2} (d+a+b) \cos. \frac{1}{2} (d \sim (a+b)) \cos. A \cos. H}{\cos. \frac{2}{3} (A+H) \cos. a \cos. H} \right)$$

$$\sin. \frac{1}{2} D = \cos. \frac{1}{2} (A+H) \sqrt{1 - \frac{\cos. \frac{3}{2} (d+a+b) \cos. \frac{1}{2} (d \sim (a+b)) \cos. A \cos. H}{\cos. \frac{2}{3} (A+H) \cos. a \cos. b}}$$

En faisant, donc,

$$\frac{1}{\cos. \frac{1}{2} (A+H)} \sqrt{\frac{\cos. \frac{1}{2} (d+a+b) \cos. \frac{1}{2} (d \sim (a+b)) \cos. A \cos. H}{\cos. a \cos. b}} = \sin. N$$

on aura $\sin. \frac{1}{2} D = \cos. \frac{1}{2} (A+H) \cos. N.$

Cette méthode est celle de M. DE BORDA, que les Navigateurs du continent employent avec succès depuis plusieurs années.

15me Formule.

De la 5me formule on déduit

$$\cos. \frac{1}{2} D = \sin. \frac{1}{2} (A+H) \sqrt{1 + \frac{\cos. \frac{1}{2} (d+a+b) \cos. \frac{1}{2} (d \sim (a+b)) \cos. A \cos. H}{\sin. \frac{2}{3} (A+H) \cos. a \cos. b}}$$

En faisant, donc,

$$\frac{1}{\sin. \frac{1}{2} (A+H)} \sqrt{\frac{\cos. \frac{1}{2} (d+a+b) \cos. \frac{1}{2} (d \sim (a+b)) \cos. A \cos. H}{\cos. a \cos. b}} = \tan. N$$

on aura $\cos. \frac{1}{2} D = \frac{\sin. \frac{1}{2} (A+H)}{\cos. N}.$

16me Formule.

Par les formules de la 6me méthode on voit, qu'en faisant

$$\sqrt{\frac{\sin. \frac{1}{2} (d+(a \sim b)) \sin. \frac{1}{2} (d-(a \sim b)) \cos. A \cos. H}{\cos. (A \sim H) \cos. a \cos. b}} = \sin. N$$

on aura $\cos. D = \cos. (A \sim H) \cos. 2 N.$

Et la distance D sera toujours de la même espèce que l'arc 2 N.

17me Formule.

De la 7me formule on déduit

$$\sin. \frac{1}{2} D = \sin. \frac{1}{2} (A \sim H) \sqrt{1 + \frac{\sin. \frac{1}{2} (d + (a \sim b)) \sin. \frac{1}{2} (d - (a \sim b)) \cos. A \cos. H}{\sin. \frac{1}{2} (A \sim H) \cos. a \cos. b}}$$

En faisant, donc,

$$\frac{1}{\sin. \frac{1}{2} (A \sim H)} \sqrt{\frac{\sin. \frac{1}{2} (d + (a \sim b)) \sin. \frac{1}{2} (d - (a \sim b)) \cos. A \cos. H}{\cos. a \cos. b}} = \tan. N$$

on aura
$$\sin. \frac{1}{2} D = \frac{\sin. \frac{1}{2} (A \sim H)}{\cos. N}.$$

Le Dr. MASKELYNE nous a donné les règles pratiques de cette méthode dans son Introduction aux Tables des Logarithmes de TAYLOR.

18me Formule.

En faisant, comme dans la 8me méthode,

$$\sqrt{\sin. \frac{1}{2} (d + (a \sim b)) \sin. \frac{1}{2} (d - (a \sim b)) \frac{\cos. A \cos. H}{\cos. a \cos. b}} = \cos. N'$$

on aura
$$\cos. \frac{1}{2} D = \sqrt{\sin. (N' + \frac{1}{2} (A \sim H)) \sin. (N' - \frac{1}{2} (A \sim H))}$$

19me Formule.

En faisant, comme dans la 9me méthode,

$$\sqrt{\sin. \frac{1}{2} (d + (a \sim b)) \sin. \frac{1}{2} (d - (a \sim b)) \frac{\cos. A \cos. H}{\cos. a \cos. b}} = \sin. N'$$

on aura
$$\cos. \frac{1}{2} D = \sqrt{\cos. (N' + \frac{1}{2} (A \sim H)) \cos. (N' - \frac{1}{2} (A \sim H))}.$$

Cette méthode est celle de Mr. DUNTHORNE perfectionnée par le Dr. MASKELYNE, dont les règles de calcul se trouvent dans les *Requisite Tables* de 1781.

20me Formule.

De la 10me formule on déduit

$$\cos. \frac{1}{2} D = \cos. \frac{1}{2} (A \sim H) \sqrt{1 - \frac{\sin. \frac{1}{2} (d + (a \sim b)) \sin. \frac{1}{2} (d - (a \sim b)) \cos. A \cos. H}{\cos. a \cos. b \cos. \frac{1}{2} (A \sim H)}}$$

En faisant, donc,

$$\frac{1}{\cos. \frac{1}{2} (A \sim H)} \sqrt{\frac{\sin. \frac{1}{2} (d + (a \sim b)) \sin. \frac{1}{2} (d - (a \sim b)) \cos. A \cos. H}{\cos. a \cos. b}} = \sin. N$$

on aura $\cos. \frac{1}{2} D = \cos. \frac{1}{2} (A \sim H) \cos. N.$

On remarquera que dans toutes ces formules se trouve la quantité $\frac{\cos. A \cos. H}{\cos. a \cos. b}$, pour laquelle on pourra prendre les différences logarithmiques de DUNTHORNE (Voyez les *Requisite Tables*), au lieu de prendre les quatre logarithmes séparément.

Les méthodes établies pour trouver la demi-distance vraie méritent que nous y fassions quelques réflexions. Les 14me, 15me, 17me, et 20me formules sont les plus commodes; mais on peut demander laquelle d'entre elles est la préférable, ou bien quels sont les avantages ou désavantages de chacune. J'en dirai ici quelques mots, d'autant plus volontiers, que je profiterai de cette occasion pour rectifier quelques opinions prématurées que j'avois eu à ce sujet, faute de l'avoir bien examiné.

La préparation des arguments dans les formules par les sommes est un peu plus courte que dans les formules par les différences; mais cet avantage, à la vérité, est très peu considérable, et ne vaut la peine d'y avoir égard, qu'en parité de toutes les autres circonstances.

Il y a deux formules (14me et 20me) où l'on cherche le cosinus de l'angle subsidiaire par le sinus, et deux autres

formules (15me et 17me) où l'on cherche ce cosinus par la tangente; il s'agit de déterminer lequel de ces deux moyens est le plus utile, pour l'exactitude du calcul. Dans les formules par les sinus, on peut supposer une erreur dans $\cos. \frac{1}{2} (A + H)$, ou $\cos. \frac{1}{2} (A \sim H)$; mais la quantité $\cos. \frac{1}{2} (A + H) \cos. N$, ou $\cos. \frac{1}{2} (A \sim H) \cos. N$, étant toujours plus petite, l'erreur résultante sera aussi plus petite. La même chose a lieu relativement à l'erreur qu'on peut commettre, en cherchant $\cos. N$ par le moyen de $\sin. N$. Au contraire, dans les formules par la tangente, l'erreur de $\sin. \frac{1}{2} (A + H)$, ou $\sin. \frac{1}{2} (A \sim H)$, produit toujours une erreur plus considérable; car $\frac{\sin. \frac{1}{2} (A + H)}{\cos. N}$, ou $\frac{\sin. \frac{1}{2} (A \sim H)}{\cos. N}$ est plus grand que $\sin. \frac{1}{2} (A + H)$, ou $\sin. \frac{1}{2} (A \sim H)$; et quant à l'erreur de N , l'effet qui y résulte sera plus grand ou plus petit, selon que $\frac{\sin. \frac{1}{2} (A + H)}{\cos. N}$, ou $\frac{\sin. \frac{1}{2} (A \sim H)}{\cos. N}$ sera aussi plus grand ou plus petit que $\cos. N$. On voit, donc, que les formules où il n'y a que des sinus sont préférables à celles qui contiennent la tangente de l'angle subsidiaire.

Nous voilà réduits aux formules 14me et 20me, dont l'une donne le sinus, et l'autre le cosinus de la demi-distance. Pour bien faire, il conviendrait d'employer la première quand la distance est moindre de 90° , et la seconde quand la distance excède le quart de cercle. Mais, en cas qu'on veuille adopter l'une d'entre elles, pour en user généralement sans distinction, on aperçoit que la 14me formule est la plus avantageuse; car les distances que donnent les Ephémérides, étant toujours à peu près entre les limites de 20° et 120° , il vaut mieux chercher les sinus compris entre 10° et 60° , que les cosinus correspondans au même espace, ou les sinus d'entre 30° et 80° . La méthode de M. DE BORDA réunit donc le plus de propriétés utiles, et mérite qu'on la

préfère aux autres dans la pratique, quand on se bornera à une seule manière de calcul, comme les Navigateurs ont coutume de faire.

Nous procéderons à présent à établir des formules pour faire le calcul par les sinus naturels.

Faisons la quantité commune $\frac{\cos. A \cos. H}{\cos. a \cos. b} = 2 \cos. M$, et substituons cette expression dans les formules précédentes. En nous rappelant que $2 \cos. \frac{1}{2} (d + a + b) \cos. \frac{1}{2} (d \sim (a + b))$ est $= \cos. d + \cos. (a + b)$, nous aurons, par les sommes, les formules que voici.

21^{me} Formule.

L'expression de $\cos. D$ se réduit à

$$\cos. D = \begin{cases} \cos. (d + M) + \cos. (d \sim M) + \cos. (a + b + M) \\ + \cos. ((a + b) \sim M) - \cos. (A + H). \end{cases}$$

22^{me} Formule.

De l'équation fondamentale des 2^{me}, 3^{me}, et 4^{me} formules on déduit

$$\sin. v. D = \begin{cases} \text{susin. } v. (A + H) - \cos. (d + M) - \cos. (d \sim M) \\ - \cos. (a + b + M) - \cos. ((a + b) \sim M). \end{cases}$$

23^{me} Formule.

La formule précédente se réduit à celle-ci

$$\sin. v. D = \begin{cases} \text{susin. } v. (A + H) + \sin. v. (d + M) + \sin. v. (d \sim M) \\ + \sin. v. (a + b + M) + \sin. v. ((a + b) \sim M) - 4. \end{cases}$$

J'ai publié il y a quelque tems cette formule, avec quelques autres notions sur la réduction des distances lunaires.

24^{me} Formule.

La même formule donne

$$\sin. v. D = \begin{cases} \text{susin. v. } (A + H) - \text{susin. v. } (d + M) \\ - \text{susin. v. } (d \sim M) - \text{susin. v. } (a + b + M) \\ - \text{susin. v. } ((a + b) \sim M) + 4. \end{cases}$$

25^{me} Formule.

La même formule donne aussi

$$\sin. v. D = \begin{cases} - \sin. v. (A + H) + \sin. v. (d + M) + \sin. v. (d \sim M) \\ + \sin. v. (a + b + M) + \sin. v. ((a + b) \sim M) - 2. \end{cases}$$

26^{me} Formule.

De la 5^{me} formule, on déduit

$$\text{susin. v. } D = \begin{cases} \sin. v. (A + H) + \cos. (d + M) + \cos. (d \sim M) \\ + \cos. (a + b + M) + \cos. ((a + b) \sim M). \end{cases}$$

27^{me} Formule.

La formule précédente se réduit à celle-ci

$$\text{susin. v. } D = \begin{cases} \sin. v. (A + H) - \sin. v. (d + M) - \sin. v. (d \sim M) \\ - \sin. v. (a + b + M) - \sin. v. ((a + b) \sim M) + 4. \end{cases}$$

28^{me} Formule.

La même formule donne aussi

$$\text{susin. v. } D = \begin{cases} \sin. v. (A + H) + \text{susin. v. } (d + M) + \text{susin. v. } (d \sim M) \\ + \text{susin. v. } (a + b + M) + \text{susin. v. } ((a + b) \sim M) \\ - 4. \end{cases}$$

29me Formule.

La même formule donne encore

$$\text{susin. v. D} = \begin{cases} - \text{susin. v. } (A + H) + \text{susin. v. } (d + M) \\ + \text{susin. v. } (d \sim M) + \text{susin. v. } (a + b + M) \\ + \text{susin. v. } ((a + b) \sim M) - 2. \end{cases}$$

En faisant la même substitution de $2 \cos. M$, et en nous rappelant que $2 \sin. \frac{1}{2} (d + (a \sim b)) \sin. \frac{1}{2} (d - (a \sim b))$ est $= \cos. (a \sim b) - \cos. d$, nous aurons, par les différences, les formules qui suivent.

30me Formule.

L'expression de $\cos. D$ se convertit en

$\cos. D = \cos. (A \sim H) + 2 \cos. M \cos. d - 2 \cos. M \cos. (a \sim b)$
et par conséquent

$$\cos. D = \begin{cases} \cos. (A \sim H) + \cos. (d + M) + \cos. (d \sim M) \\ - \cos. ((a \sim b) + M) - \cos. ((a \sim b) \sim M) \end{cases}$$

31me Formule.

De la 7me formule on déduit

$$\text{sin. v. D} = \begin{cases} \text{sin. v. } (A \sim H) - \cos. (d + M) - \cos. (d \sim M) \\ + \cos. ((a \sim b) + M) + \cos. ((a \sim b) \sim M). \end{cases}$$

Cette formule donne les trois suivantes.

32me Formule.

$$\text{sin. v. D} = \begin{cases} \text{sin. v. } (A \sim H) + \text{sin. v. } (d + M) + \text{sin. v. } (d \sim M) \\ - \text{sin. v. } ((a \sim b) + M) - \text{sin. v. } ((a \sim b) \sim M). \end{cases}$$

Mr. KRAFFT nous a donné cette formule dans un beau Mémoire qui fait partie des Actes de l'Académie de Petersbourg.

33me Formule.

$$\sin. v. D = \begin{cases} \sin. v. (A \sim H) + \sin. v. (d \sim M) + \sin. v. (d \sim M) \\ + \text{susin. v. } \{(a \sim b) + M\} + \text{susin. v. } \{(a \sim b) \sim M\} - 4. \end{cases}$$

34me Formule.

$$\sin. v. D = \begin{cases} - \text{susin. v. } (A \sim H) - \text{susin. v. } (d + M) \\ - \text{susin. v. } (d \sim M) + \text{susin. v. } \{(a \sim b) + M\} \\ + \text{susin. v. } \{(a \sim b) \sim M\} + 2. \end{cases}$$

35me Formule.

De l'équation fondamentale des 8me, 9me, et 10me formules on déduit

$$\text{susin. v. } D = \begin{cases} \text{susin. v. } (A \sim H) + \cos. (d + M) + \cos. (d \sim M) \\ - \cos. \{(a \sim b) + M\} - \cos. \{(a \sim b) \sim M\}. \end{cases}$$

Cette formule donne les trois suivantes.

36me Formule.

$$\text{susin. v. } D = \begin{cases} \text{susin. v. } (A \sim H) - \sin. v. (d + M) - \sin. v. (d \sim M) \\ + \sin. v. \{(a \sim b) + M\} + \sin. v. \{(a \sim b) \sim M\}. \end{cases}$$

37me Formule.

$$\text{susin. v. } D = \begin{cases} \text{susin. v. } (A \sim H) + \text{susin. v. } (d + M) \\ + \text{susin. v. } (d \sim M) + \sin. v. \{(a \sim b) + M\}. \\ + \sin. v. \{(a \sim b) \sim M\} - 4 \end{cases}$$

38me Formule.

$$\text{susin. v. D} = \begin{cases} - \sin. v. (A \sim H) + \text{susin. v. } (d + M) \\ + \text{susin. v. } (d \sim M) + \sin. v. ((a \sim b) + M) \\ + \sin. v. ((a \sim b) \sim M) - 2. \end{cases}$$

Les méthodes, dont les formules renferment des cosinus, ont l'inconvénient de se diviser en différens cas, selon que les arcs correspondans sont plus ou moins grands que le quart de cercle. Toutes les autres formules admettent des règles constantes, et les 23me, 28me, 33me, et 37me réunissent aussi l'avantage de n'exiger que la simple somme des six sinus-verses ou susinus-verses, pour avoir celui de la distance vraie.

Entre les formules par les sommes, et les formules par les différences, les premières sont préférables; car on peut déduire la somme des hauteurs vraies de la somme des hauteurs apparentes d'une manière très simple; pendant que, pour avoir la différence des hauteurs apparentes, il n'y a pas de meilleur procédé que celui de corriger séparément chaque hauteur apparente, pour faire la soustraction ensuite.

J'ai calculé les sinus-verses naturels pour chaque dix secondes de la demi-circonférence, ainsi qu'une table très complète des angles M. Par ces moyens la réduction des distances lunaires deviendra très commode, en employant celle qu'on jugera convenable des formules précédentes.

Pour rendre les opérations encore plus faciles, j'ai calculé une table à double argument (savoir l'angle M, et un autre angle quelconque) qui donne à la fois la quantité

$$\sin. v. (d + M) + \sin. v. (d \sim M)$$

ou la quantité

$$\sin. v. (d + a + b) + \sin. v. ((a + b) \sim M)$$

Ainsi, pour le calcul de la 23^{me} formule, on réduira les opérations des sinus-verses à la simple somme de trois nombres, et par là on diminuera aussi les opérations préliminaires avec les élémens; car alors il suffira de prendre l'angle auxiliaire (pour argument des nombres sommaires), et de déduire la somme des hauteurs apparentes et des hauteurs vraies.

Voici deux formules par les différences, dont le calcul admet l'application de ces nombres sommaires, quoique d'une manière moins commode que celle de la 23^{me} méthode.

39^{me} Formule.

$$\sin. v. D = \begin{cases} \sin. v. (A \sim H) + \sin. v. (d + M) + \sin. v. (d \sim M) \\ + \sin. v. (180^\circ \sim ((a \sim b) + M)) \\ + \sin. v. (180^\circ - ((a \sim b) \sim M)) - 4. \end{cases}$$

40^{me} Formule.

$$\text{susin. v. } D = \begin{cases} \text{susin. v. } (A \sim H) + \sin. v. (180^\circ \sim (d + M)) \\ + \sin. v. (180^\circ - (d \sim M)) + \sin. v. ((a \sim b) + M) \\ + \sin. v. ((a \sim b) \sim M) - 4. \end{cases}$$

Je ne m'arrêterai pas à d'autres transformations qu'on pourroit faire; celles qui viennent d'être établies étant suffisantes pour l'objet que je me suis proposé.

Méthodes d'Approximation.

L'expression du cosinus de la distance vraie en termes des données du problème est, comme nous avons vu ci-dessus,

$$\cos. D = \frac{\cos. d \cos. A \cos. H}{\cos. a \cos. b} - \tan. a \tan. b \cos. A \cos. H + \sin. A \sin. H.$$

Représentons par u la parallaxe moins la réfraction en hauteur de la lune, par v la réfraction moins la parallaxe en hauteur du soleil, ou la simple réfraction de l'étoile, et par δ la correction totale de la distance apparente, telle que $D = d + \delta$.

Il s'agit à présent de trouver la valeur de δ en termes des corrections u, v , et de la distance et des hauteurs apparentes.

On a $A = a + u$, et $H = b - v$; et par conséquent

$$\sin. A = \sin. a \cos. u + \cos. a \sin. u$$

$$\cos. A = \cos. a \cos. u - \sin. a \sin. u$$

$$\sin. H = \sin. b \cos. v - \cos. b \sin. v$$

$$\cos. H = \cos. b \cos. v + \sin. b \sin. v$$

d'où l'on déduit

$$\sin. A \sin. H = \begin{cases} \sin. a \sin. b \cos. u \cos. v + \cos. a \sin. b \sin. u \cos. v \\ - \sin. a \cos. b \cos. u \sin. v - \cos. a \cos. b \sin. u \sin. v \end{cases}$$

$$\cos. A \cos. H = \begin{cases} \cos. a \cos. b \cos. u \cos. v - \sin. a \cos. b \sin. u \cos. v \\ + \cos. a \sin. b \cos. u \sin. v - \sin. a \sin. b \sin. u \sin. v \end{cases}$$

Pour obtenir toute l'exactitude nécessaire, il suffira de porter les approximations jusqu'aux produits du second ordre, ou de deux dimensions des petits élémens u, v, δ . Or, un petit arc et son sinus ne différant entre eux que d'un produit du troisième ordre, on pourra prendre u pour $\sin. u$, et v pour $\sin. v$; mais, comme la différence entre le rayon et le cosinus d'un petit arc va jusqu'au second ordre, on devra substituer $1 - \frac{1}{2}u^2 = \cos. u$, et $1 - \frac{1}{2}v^2 = \cos. v$. En y introduisant ces valeurs, on aura,

en négligeant les produits de trois dimensions de u , v (ce que nous ferons aussi par la suite),

$$\sin. A \sin. H = \begin{cases} \sin. a \sin. b + u \cos. a \sin. b - v \sin. a \cos. b \\ -uv \cos. a \cos. b - \frac{1}{2}u^2 \sin. a \sin. b - \frac{1}{2}v^2 \sin. a \sin. b. \end{cases}$$

$$\cos. A \cos. H = \begin{cases} \cos. a \cos. b - u \sin. a \cos. b + v \cos. a \sin. b \\ -uv \sin. a \sin. b - \frac{1}{2}u^2 \cos. a \cos. b - \frac{1}{2}v^2 \cos. a \cos. b. \end{cases}$$

et, en substituant ces valeurs dans l'expression de $\cos. D$, et faisant les réductions nécessaires, il résultera

$$\cos. D = \begin{cases} \cos. d + \frac{u \sin. b}{\cos. a} - u \cos. d \tan. a - \frac{v \sin. a}{\cos. b} + v \cos. d \tan. b \\ + \frac{uv (\sin.^2 a - \cos.^2 b - \cos. d \sin. a \sin. b)}{\cos. a \cos. b} - \frac{1}{2}u^2 \cos. d - \frac{1}{2}v^2 \cos. d. \end{cases}$$

Reprenons à présent $D = d + \delta$, et l'on aura

$$\cos. D = \cos. d \cos. \delta - \sin. d \sin. \delta, \text{ ou (parce que } \cos. \delta = 1 - \frac{1}{2}\delta^2),$$

$\cos. D = \cos. d - \delta \sin. d = \frac{1}{2}\delta^2 \cos. d$, ce qui, étant substitué dans l'équation précédente, donne

$$\delta = \begin{cases} -\frac{u \sin. b}{\cos. a \sin. d} + u \cot. d \tan. a + \frac{v \sin. a}{\cos. b \sin. d} - v \cot. d \tan. b \\ + \frac{uv (\sin.^2 a - \cos.^2 b + \cos. d \sin. a \sin. b)}{\sin. d \cos. a \cos. b} + \frac{1}{2}u^2 \cot. d + \frac{1}{2}v^2 \cot. d \\ - \frac{1}{2}\delta^2 \cot. d. \end{cases}$$

c'est-à-dire,

$$\delta = \begin{cases} -u \left(\frac{\sin. b - \cos. d \sin. a}{\sin. d \cos. a} \right) + v \left(\frac{\sin. a - \cos. d \sin. b}{\sin. d \cos. b} \right) \\ + uv \left(\frac{\cos. d \sin. a \sin. b - \sin.^2 a + \cos.^2 b}{\sin. d \cos. a \cos. b} \right) + \frac{1}{2}u^2 \cot. d \\ + \frac{1}{2}v^2 \cot. d - \frac{1}{2}\delta^2 \cot. d. \end{cases}$$

Mais, on voit par cette même formule que (en continuant de négliger les produits de deux dimensions de u , v , δ), l'on a

$$\delta^2 = \begin{cases} u^2 \left(\frac{\sin. b - \cos. d \sin. a}{\sin. d \cos. a} \right)^2 + v^2 \left(\frac{\sin. a - \cos. d \sin. b}{\sin. d \cos. b} \right)^2 \\ - 2uv \left(\frac{\sin. b - \cos. d \sin. a}{\sin. d \cos. a} \right) \left(\frac{\sin. a - \cos. d \sin. b}{\sin. d \cos. b} \right) \end{cases}$$

donc, en substituant dans la formule précédente, il résultera

$$\delta = \begin{cases} -u \left(\frac{\sin. b - \cos. d \sin. a}{\sin. d \cos. a} \right) + v \left(\frac{\sin. a - \cos. d \sin. b}{\sin. d \cos. b} \right) \\ + u v \left(\frac{2 \cos. d \sin. a \sin. b + \sin.^2 d - \sin.^2 a - \sin.^2 b}{\sin.^3 d \cos. a \cos. b} \right) \\ + \frac{1}{2} u^2 \cot. d \left(1 - \left(\frac{\sin. b - \cos. d \sin. a}{\sin. d \cos. a} \right)^2 \right) \\ + \frac{1}{2} v^2 \cot. d \left(1 - \left(\frac{\sin. a - \cos. d \sin. b}{\sin. d \cos. b} \right)^2 \right) \end{cases}$$

ou bien,

$$\delta = \begin{cases} -u \left(\frac{\sin. b - \cos. d \sin. a}{\sin. d \cos. a} \right) + v \left(\frac{\sin. a - \cos. d \sin. b}{\sin. d \cos. b} \right) \\ + u v \left(\frac{2 \cos. d \sin. a \sin. b + \sin.^2 d - \sin.^2 a - \sin.^2 b}{\sin.^3 d \cos. a \cos. b} \right) \\ + \frac{1}{2} u^2 \cot. d \left(\frac{2 \cos. d \sin. a \sin. b + \sin.^2 d - \sin.^2 a - \sin.^2 b}{\sin.^2 d \cos.^2 a} \right) \\ + \frac{1}{2} v^2 \cot. d \left(\frac{2 \cos. d \sin. a \sin. b + \sin.^2 d - \sin.^2 a - \sin.^2 b}{\sin.^2 d \cos.^2 b} \right). \end{cases}$$

Voilà la formule qui exprime généralement les corrections qu'on doit appliquer à la distance apparente d , pour avoir la distance vraie D , ayant égard à toutes les équations qui dérivent de u , v , et des produits du second ordre de ces éléments. On peut, à son aide, prouver l'exactitude d'une méthode d'approximation quelconque. Il ne faut, pour cela, que transformer les expressions des corrections proposées, de manière à les mettre toutes en termes de la distance apparente, des hauteurs apparentes, et des corrections des hauteurs; et les comparer ainsi aux précédentes. Ce procédé m'a été fort utile pour examiner différentes méthodes, et découvrir leurs erreurs: mais je ne m'arrêterai pas, à présent, à ces détails; et pour donner un exemple de l'application de ma formule, je me bornerai à la considération de la solution du Dr. MASKELYNE.

Soient un arc M , tel que $\tan. M = \tan. \frac{1}{2} (a \sim b) \cot. \frac{1}{2} (a + b)$ (c'est le premier arc des préceptes de l'auteur), et un autre

arc N, tel que $\tan. N = \tan. M \cot. \frac{1}{2} d$ (c'est le second arc des dits préceptes), et exprimons par R la réfraction qui convient à la hauteur de 45° . La correction totale qu'on doit appliquer à la distance par rapport aux réfractions des deux astres est, selon la méthode dont il s'agit, $= \frac{2 R \tan. 2 M}{\sin. 2 N}$, ou (parceque, en représentant la réfraction en hauteur de l'étoile par r' , on a

$$R = r' \tan. b) = \frac{2 r' \tan. b \tan. 2 M}{\sin. 2 N}. \text{ Réduisons l'expression } \frac{2 R \tan. 2 M}{\sin. 2 N}$$

aux termes dont nous avons besoin.

En substituant $\tan. 2 M = \frac{2 \tan. M}{1 - \tan.^2 M}$, et $\sin. 2 N = \frac{2 \tan. N}{1 + \tan.^2 N}$ on aura $\frac{2 R \tan. M (1 + \tan.^2 N)}{\tan. N (1 - \tan.^2 M)}$. Mettons y $\tan. N = \tan. M \cot. \frac{1}{2} d$, et il résultera $\frac{2 R (1 + \tan.^2 M \cot.^2 \frac{1}{2} d)}{\cot. \frac{1}{2} d (1 - \tan.^2 M)}$, qui, en substituant

$$\tan. M = \tan. \frac{1}{2} (a \sim b) \cot. \frac{1}{2} (a + b), \text{ se convertit en } \frac{2 R (1 + \tan.^2 \frac{1}{2} (a \sim b) \cot.^2 \frac{1}{2} (a + b)) \cot.^2 \frac{1}{2} d}{\cot. \frac{1}{2} d (1 - \tan.^2 \frac{1}{2} (a \sim b) \cot.^2 \frac{1}{2} (a + b))} = \frac{2 R (\cos.^2 \frac{1}{2} (a \sim b) \sin.^2 \frac{1}{2} (a + b) + \sin.^2 \frac{1}{2} (a \sim b) \cos.^2 \frac{1}{2} (a + b)) \cot.^2 \frac{1}{2} d}{\cot. \frac{1}{2} d (\cos.^2 \frac{1}{2} (a \sim b) \sin.^2 \frac{1}{2} (a + b) - \sin.^2 \frac{1}{2} (a \sim b) \cos.^2 \frac{1}{2} (a + b))}$$

ce qui, en substituant

$$\cos.^2 \frac{1}{2} (a \sim b) \sin.^2 \frac{1}{2} (a + b) = \frac{1}{4} (\sin.^2 a + \sin.^2 b + 2 \sin. a \sin. b)$$

et

$$\sin.^2 \frac{1}{2} (a \sim b) \cos.^2 \frac{1}{2} (a + b) = \frac{1}{4} (\sin.^2 a + \sin.^2 b - 2 \sin. a \sin. b),$$

et, faisant les réductions nécessaires, donne

$$R \frac{(\sin.^2 a + \sin.^2 b + 2 \sin. a \sin. b) \sin.^2 \frac{1}{2} d + (\sin.^2 a + \sin.^2 b - 2 \sin. a \sin. b) \cot.^2 \frac{1}{2} d}{2 \sin. \frac{1}{2} d \cos. \frac{1}{2} d \sin. a \sin. b}$$

d'où, en mettant $\frac{1}{2} - \frac{1}{2} \cos. d = \sin.^2 \frac{1}{2} d$, et $\frac{1}{2} + \frac{1}{2} \cos. d = \cos.^2 \frac{1}{2} d$,

$$\text{on tire } \frac{R (\sin.^2 a + \sin.^2 b - 2 \cos. d \sin. a \sin. b)}{\sin. d \sin. a \sin. b}.$$

Cette formule se résout en deux expressions, ou parties,

$$\frac{R (\sin. a - \cos. d \sin. b)}{\sin. d \sin. b}, \text{ et } \frac{R (\sin. b - \cos. d \sin. a)}{\sin. d \sin. a}.$$

Appellons r la réfraction en hauteur de la lune. On aura

par la loi des réfractions* $R = r' \tan. b$, ou $R = r \tan. a$.
Mettant ces valeurs dans les équations précédentes, elles se
réduiront à $\frac{r' (\sin. a - \cos. d \sin. b)}{\sin. d \cos. b}$, et $\frac{r (\sin. b - \cos. d \sin. a)}{\sin. d \cos. a}$; qui ex-
priment les corrections dépendantes de la réfraction de chaque
astre.

Soit Q un arc $= N \approx \frac{1}{2} d$ (c'est le troisième arc des pré-
ceptes; le signe supérieur quand la hauteur du soleil ou de
l'étoile est plus grande que celle de la lune, le signe inférieur
dans le cas contraire) et représentons par P la parallaxe †
horizontale de la lune. La correction de la distance relative à
la parallaxe est, d'après le Dr. MASKELYNE, $= P \sin. a \tan. Q$.

On a $\tan. Q = \tan. (N \approx \frac{1}{2} d) = \frac{\tan. N \approx \tan. \frac{1}{2} d}{1 \approx \tan. N \tan. \frac{1}{2} d}$. Mais
 $\tan. N = \tan. \frac{1}{2} (a \sim b) \cot. \frac{1}{2} (a + b) \cot. \frac{1}{2} d$, ou
 $\tan. N = \frac{(\sin. a \sim \sin. b) \cot. \frac{1}{2} d}{\sin. a + \sin. b}$. Donc, en substituant et en faisant
les réductions nécessaires, on déduira

$$\tan. Q = \frac{(1 + \cos. d) (\sin. a \sim \sin. b) \approx (1 - \cos. d) (\sin. a + \sin. b)}{2 \sin. d \sin. a}$$

c'est-à-dire, $\tan. Q = \frac{\sin. b - \cos. d \sin. a}{\sin. d \sin. a}$

Ainsi, la correction relative à la parallaxe est

$$\frac{P \sin. a (\sin. b - \cos. d \sin. a)}{\sin. d \sin. a} = \frac{P (\sin. b - \cos. d \sin. a)}{\sin. d}$$

* Le Dr. MASKELYNE ne néglige pas d'avoir égard aux corrections que demande
cette supposition, parceque la loi des réfractions est un peu différente; ce que l'auteur
fait par un procédé très simple, qu'il facilite par le moyen des deux Tables subsidiaires.

† C'est à P que le Dr. MASKELYNE applique une équation pour compenser la
petite erreur qui résulte de la loi des réfractions adoptée auparavant. Ainsi, au lieu
de la parallaxe horizontale, il emploie ce qu'il appelle la *parallaxe horizontale cor-
rigée*.

ce qui, en représentant la parallaxe en hauteur par p , et substituant $P = \frac{p}{\cos. a}$, se réduit à $\frac{p (\sin. b - \cos. d \sin. a)}{\sin. d \cos. a}$.

On voit, par les équations ci-dessus, que la correction composée de la parallaxe et de la réfraction de la lune est $\frac{p (\sin. b - \cos. d \sin. a)}{\sin. d \cos. a} - \frac{r (\sin. b - \cos. d \sin. a)}{\sin. d \cos. a} = \frac{(p-r) (\sin. b - \cos. d \sin. a)}{\sin. d \cos. a}$.

Expression identique à celle que nous avons trouvé relativement à u . L'expression $\frac{r' (\sin. a - \cos. d \sin. b)}{\sin. d \cos. b}$ convient aussi avec celle qui dérive de v . Il nous reste à examiner les corrections relatives aux produits du second ordre.

En faisant $\tan. Q \tan. a = \cos. S$ (S est le quatrième arc des dits préceptes), on a pour la troisième correction du Dr.

MASKELYNE * $\frac{(P - \frac{r}{\cos. a})^2 \cos.^2 a \sin.^2 S}{2 \tan. d}$. Cette expression se convertit en $\frac{(P \cos. a - r)^2 \sin.^2 S}{2 \tan. d} = \frac{(p-r)^2 \sin.^2 S}{2 \tan. d}$.

En prenant la valeur de $\tan. Q$ établie ci-dessus, on a

$$\cos. S = \frac{(\sin. b - \cos. d \sin. a) \tan. a}{\sin. d \sin. a} = \frac{\sin. b - \cos. d \sin. a}{\sin. d \cos. a}, \text{ d'où l'on déduit}$$

$$\sin.^2 S = 1 - \cos.^2 S = \frac{2 \cos. d \sin. a \sin. b + \sin.^2 d - \sin.^2 a - \sin.^2 b}{\sin.^2 d \cos.^2 a};$$

et, en substituant cette expression dans la précédente, il résultera, pour la correction dont il s'agit,

$$\frac{1}{2} (p-r)^2 \cot. d \left(\frac{2 \cos. d \sin. a \sin. b + \sin.^2 d - \sin.^2 a - \sin.^2 b}{\sin.^2 d \cos.^2 a} \right).$$

Expression identique à celle que nous avons trouvé relativement à u^2 .

La quatrième correction est (en représentant la troisième par m), $= \frac{2 r' m}{\cos. d \cos. b} - \frac{r}{\cos. a}$; qui en substituant la valeur de m ,

* La quantité $P = \frac{r}{\cos. a}$ est ce que l'auteur appelle *parallaxe horizontale diminuée*.

et celle de $P = \frac{p}{\cos. a^2}$ se réduit à

$$r' (p - r) \left(\frac{2 \cos. d \sin. a \sin. b + \sin.^2 d - \sin.^2 a - \sin.^2 b}{\sin.^2 d \cos. a \cos. b} \right).$$

Expression identique à celle que nous avons trouvée relativement à $u v$.

La cinquième correction est $= \frac{m r'^2}{\cos.^2 b \left(P - \frac{r}{\cos. a} \right)}$, qui se réduit

facilement à l'expression déduite ci-dessus relativement à v^2 .

On voit, par cet examen, que la méthode en question a toute l'exactitude qu'on peut désirer; et c'est la raison qui m'a déterminé à la choisir, entre toutes celles que je connois, pour donner un exemple satisfaisant et complet de la manière d'employer mes formules dans ces sortes d'analyses.

Je finirai cet article en donnant quelques formules, qu'on pourra employer pour calculer les corrections qu'on doit appliquer à la distance apparente pour avoir la distance vraie.

Reprenons l'équation

$$d = \left\{ \begin{aligned} & -u \left(\frac{\sin. b - \cos. d \sin. a}{\sin. d \cos. a} \right) + v \left(\frac{\sin. a - \cos. d \sin. b}{\sin. d \cos. b} \right) + uv \left(\frac{2 \cos. d \sin. a \sin. b + \sin.^2 d - \sin.^2 a - \sin.^2 b}{\sin.^2 d \cos. a \cos. b} \right) \\ & + \frac{1}{2} u^2 \cot. d \left(1 - \left(\frac{\sin. b - \cos. d \sin. a}{\sin. d \cos. a} \right)^2 \right) + \frac{1}{2} v^2 \cot. d \left(1 - \left(\frac{\sin. a - \cos. d \sin. b}{\sin. d \cos. b} \right)^2 \right). \end{aligned} \right.$$

On voit facilement que

$$\frac{2 \cos. d \sin. a \sin. b + \sin.^2 d - \sin.^2 a - \sin.^2 b}{\sin.^2 d \cos. a \cos. b} = \sqrt{\left(1 - \left(\frac{\sin. b - \cos. d \sin. a}{\sin. d \cos. a} \right)^2 \right) \left(1 - \left(\frac{\sin. a - \cos. d \sin. b}{\sin. d \cos. b} \right)^2 \right)}$$

Donc, en substituant cette expression dans la formule précédente, elle se réduit à

$$d = \left\{ \begin{aligned} & -u \left(\frac{\sin. h - \cos. d \sin. a}{\sin. d \cos. a} \right) + v \left(\frac{\sin. a - \cos. d \sin. h}{\sin. d \cos. h} \right) + \frac{uv}{\sin. d} \sqrt{\left(1 - \left(\frac{\sin. h - \cos. d \sin. a}{\sin. d \cos. a} \right)^2 \right) \left(1 - \left(\frac{\sin. a - \cos. d \sin. h}{\sin. d \cos. h} \right)^2 \right)} \\ & + \frac{1}{2} u^2 \cot. d \left(1 - \left(\frac{\sin. h - \cos. d \sin. a}{\sin. d \cos. a} \right)^2 \right) + \frac{1}{2} v^2 \cot. d \left(1 - \left(\frac{\sin. a - \cos. d \sin. h}{\sin. d \cos. h} \right)^2 \right). \end{aligned} \right.$$

Faisons $\frac{\sin. b - \cos. d \sin. a}{\sin. d \cos. a} = \cos. L$, et $\frac{\sin. a - \cos. d \sin. b}{\sin. d \cos. b} = \cos. S$,

(et l'on voit, que L représente l'angle formé par la distance apparente des deux astres et la distance apparente de la lune au zénith, et S l'angle formé de la même manière au lieu apparent du soleil ou de l'étoile), et la formulè se convertira en

$$\delta = \left\{ \begin{array}{l} -u \cos. L + v \cos. S + u v \operatorname{cosec}. d \sin. L \sin. S \\ + \frac{1}{2} u^2 \cot. d \sin.^2 L + \frac{1}{2} v^2 \cot. d \sin.^2 S. \end{array} \right.$$

Les principales corrections sont $-u \cos. L + v \cos. S$, ou

$$-u \left(\frac{\sin. b - \cos. d \sin. a}{\sin. d \cos. a} \right) + v \left(\frac{\sin. a - \cos. d \sin. b}{\sin. d \cos. b} \right). \text{ Appellons les } \Sigma,$$

et nous aurons

$$\Sigma = -u \cos. L + v \cos. S$$

$$\Sigma = -u + u \sin. v. L + v - v \sin. v. S$$

$$\Sigma = -u + u (1 - \cos. L) + v - v (1 - \cos. S)$$

$$\Sigma = -u + u \left(1 - \frac{\sin. b - \cos. d \sin. a}{\sin. d \cos. a} \right) + v - v \left(1 - \frac{\sin. a - \cos. d \sin. b}{\sin. d \cos. b} \right)$$

$$\Sigma = \left\{ \begin{array}{l} -u + u \left(\frac{\sin. d \cos. a + \cos. d \sin. a - \sin. b}{\sin. d \cos. a} \right) \\ + v - v \left(\frac{\sin. d \cos. b + \cos. d \sin. b - \sin. a}{\sin. d \cos. b} \right) \end{array} \right.$$

$$\Sigma = -u + u \left(\frac{\sin. (d+a) - \sin. b}{\sin. d \cos. a} \right) + v - v \left(\frac{\sin. (d+b) - \sin. a}{\sin. d \cos. b} \right)$$

$$\Sigma = \left\{ \begin{array}{l} -u + u \frac{2 \cos. \frac{1}{2} (d+a+b) \sin. \frac{1}{2} (d+a-b)}{\sin. d \cos. a} \\ + v - v \frac{2 \cos. \frac{1}{2} (d+a+b) \sin. \frac{1}{2} (d+b-a)}{\sin. d \cos. b} \end{array} \right.$$

L'application de ces formules n'exige aucune distinction de cas; car il faudra toujours ajouter à la distance apparente les quantités $v + u \frac{2 \cos. \frac{1}{2} (d+a+b) \sin. \frac{1}{2} (d+a-b)}{\sin. d \cos. a}$, et retrancher

la somme $u + v \frac{2 \cos. \frac{1}{2} (d+a+b) \sin. \frac{1}{2} (d+b-a)}{\sin. d \cos. b}$. Les opérations sont

d'ailleurs assez faciles, car on n'a besoin de chercher que six logarithmes; le cosinus de $\frac{1}{2}(d+a+b)$ et le sinus de d se trouvant dans les deux expressions qu'on calcule.

Cette méthode me paroît utile, pour le calcul des deux corrections principales. Quant aux autres corrections, je me bornerai à indiquer quelques expressions nouvelles, qui dérivent des précédentes, sans y entrer dans les détails de leurs propriétés particulières.

Représentons la correction relative à u^2 par m . Dans le calcul de la correction relative à u , l'on trouve le logarithme de $\sin. v. L.$ En faisant usage des tables des sinus-verses, on pourra, donc, déduire m par l'une des expressions $\frac{1}{2}u^2 \cot. d \sin.^2 L.$, ou $\frac{1}{2}u^2 \cot. d \sin. v. L. \text{ susin. } v. L.$

J'observerai ici que, comme cette formule contient le carré de l'arc u^2 , en parties de la circonférence, il faudra diviser dans le calcul par R'' , c'est-à-dire, par la valeur du rayon ou du sinus total, en secondes, pour avoir l'équation aussi en secondes. La même remarque a lieu pour toutes les expressions semblables à la précédente.

Le logarithme de R'' est 5.3144251. Donc, pour le calcul de m par la formule précédente, on pourra se servir du logarithme constant négatif 5.3144251 + 0.3010300 = 5.6154551, ou, ce qui revient au même, du logarithme constant positif 4.3845449.

Si l'on emploie les logarithmes logistiques, ou proportionels pour 3^h ou 10800'', ces logarithmes étant réciproques, on aura pour logarithme constant positif

$$5.3144251 + 0.3010300 - 4.0334238 = 1.5820313.$$

En employant seulement les tables des sinus, on pourra prendre la moitié des quatre logarithmes $\frac{\cos. \frac{1}{2}(d+a+b) \sin. \frac{1}{2}(d+b-a)}{\sin. d \cos. b}$

dans le calcul de la première correction, ce qui donne $\log. \sin. \frac{1}{2}L$ et trouver ensuite m par l'expression $2u^2 \cot. d \sin. \frac{1}{2}L \cos. \frac{1}{2}L$.

Pour le calcul de cette formule, on aura le logarithme constant positif 4.9866049, en employant les logarithmes ordinaires, et 0.9799713, en employant les logarithmes proportionels.

Les formules que nous avons établies fournissent une autre méthode pour déterminer m .

En reprenant $m = \frac{1}{2}u^2 \cot. d \sin. v. L \text{ susin. } v. L$, et substituant $2 - \sin. v. L = \text{susin. } v. L$, on déduit

$$m = u^2 \cot. d \sin. v. L - \frac{1}{2}u^2 \cot. d \sin. v.^2 L$$

Or, $u \sin. v. L$ n'est autre chose que l'équation

$$u \frac{2 \cos. \frac{1}{2}(d+a+b) \sin. \frac{1}{2}(d+a-b)}{\sin. d \cos. a}$$

, qu'on calcule pour la correction principale relative à u ; donc, en représentant cette équation par μ , on aura

$$m = u \mu \cot. d - \frac{1}{2}\mu^2 \cot. d$$

ou $m = \mu (u - \frac{1}{2}\mu) \cot. d.$

Cette manière de calculer m est très commode, et je crois qu'on doit surtout la préférer, quand on se bornera à ce degré d'approximation, qui sera suffisant dans la plupart des circonstances, en négligeant les équations relatives à uv et v^2 .

Pour le calcul de $(u - \frac{1}{2}\mu) \cot. d$, ou de $u^2 \cot. d \sin. v. L$, on aura le logarithme constant positif 4.6855749, en employant les logarithmes ordinaires, et 1.2810013, en employant les logarithmes proportionels. Pour le calcul de $\frac{1}{2}\mu^2 \cot. d$ le logarithme constant positif est 4.3845449 ou 1.5820313.

Représentons par n la correction relative à v^2 , et l'on aura de même les expressions suivantes.

$$n = \frac{1}{2}v^2 \cot. d \sin. v. S \text{ susin. } v. S$$

$$n = 2v^2 \cot. d \sin. \frac{1}{2}S \cos. \frac{1}{2}S.$$

Et, en représentant par π l'équation principale relative à v ,

c'est-a-dire, $v \frac{2 \cos. \frac{1}{2} (d + a + b) \sin. \frac{1}{2} (d + b - a)}{\sin. d \cos. b}$, on déduira aussi

$$n = v \pi \cot. d - \frac{1}{2} \pi^2 \cot. d$$

ou

$$n = \pi (v - \frac{1}{2} \pi) \cot. d.$$

Pour les logarithmes constans, qui conviennent à ces formules, je m'en rapporte à ce que j'ai dit au sujet des corrections relatives à u^2 .

Quant à la correction relative à uv , que nous appellerons ω , on a

$$\omega = \frac{uv \sin. L \sin. S}{\sin. d}.$$

Ayant recours aux corrections précédentes, on voit que $uv \sin. L \sin. S = 2 \tan. d \sqrt{mn}$; donc, en substituant, il résultera

$$\omega = \frac{2 \sqrt{mn}}{\cos. d}.$$

De l'expression qui précède, l'on tire celle-ci

$$\omega = \frac{2 \sqrt{\mu \pi (u - \frac{1}{2} \mu) (v - \frac{1}{2} \pi)}}{\sin. d}.$$

De l'expression trouvée $\omega = \frac{uv \sin. L \sin. S}{\sin. d}$, qu'on pourra employer, quand on calculera les autres corrections par les sinus-verses, on déduit $\omega = \frac{4uv \sin. \frac{1}{2} L \cos. \frac{1}{2} L \sin. \frac{1}{2} S \cos. \frac{1}{2} S}{\sin. d}$, dont on pourra faire usage, quand on calculera seulement par les sinus.

Nous remarquerons, qu'en se bornant à la correction relative à u^2 , et négligeant les autres équations qui dépendent des produits de deux dimensions, comme l'on pratique dans quelques méthodes connues, on pourra faire le calcul des deux corrections principales par les formules précédentes, et puis trouver la troisième correction de la manière adoptée par Mr. LYONS, en se servant de la Table XIII. des *Requisite Tables* de 1781. (Mr. LYONS avoit donné cette Table dans l'édition de 1767) Ce procédé seroit très commode et assez exact pour les cas ordinaires, où l'on se contente des méthodes qui ne sont pas rigoureuses. Au reste, le calcul de cette correction par la der-

nière formule que nous avons donnée, est presque aussi commode, et réunit outre cela l'avantage de ne pas demander des tables subsidiaires.

Remarques générales sur les Méthodes précédentes.

Les méthodes directes par les logarithmes, au moins, les meilleures de cette espèce, procurent la distance réduite, avec exactitude, et suivant des règles constantes. Les opérations sont, d'ailleurs, assez simples, et n'exigent pas un grand nombre de logarithmes. Mais ces avantages se trouvent diminués dans la pratique. En effet, on est obligé d'employer les logarithmes avec plusieurs décimales, ce qui augmente la masse du calcul dans une certaine proportion, et produit d'autres inconvéniens ; car on ne peut pas se dispenser de calculer et d'appliquer des parties proportionnelles, quand on fait usage des tables ordinaires, et si, pour éviter cette peine, on a recours aux tables qui donnent les logarithmes de seconde en seconde, la facilité qu'elles offrent est moins considérable qu'on ne pourroit le penser, par l'embarras d'un gros volume, où l'on ne laisse pas de perdre du tems à feuilleter, pour trouver l'endroit qu'on cherche.

Les propriétés caractéristiques des méthodes d'approximation sont différentes. Elles sont indirectes, et demandent plus ou moins de distinctions des cas. Les opérations sont, outre cela, longues et complexes, surtout quand on veut arriver à un résultat exact. En revanche, comme ce qu'on calcule n'est pas le total de la distance vraie, mais seulement les corrections qu'on doit appliquer à la distance apparente (quantités qui ne sont pas très considérables), il suffit d'employer les logarithmes avec peu de décimales ; et de cette manière on peut faire le calcul avec des tables très courtes, et négliger les parties proportionnelles.

Les méthodes par les sinus-verses naturels me paroissent réunir les avantages des deux sortes de procédés que nous venons de considérer, sans être sujettes à leurs inconvéniens. Mais comme l'utilité de ces méthodes dépend des tables que j'ai calculées, et que j'ai annoncées depuis plusieurs années, mais qui ne sont pas encore connues, je crois superflu d'ajouter plus de réflexions à ce sujet; m'en rapportant là-dessus à ce qu'on trouvera dans cet ouvrage, actuellement sous presse, et qui est destiné à faciliter les opérations pratiques.

Méthode pour avoir égard à la Figure elliptique de la Terre.

La figure elliptique de la Terre peut influer de deux manières dans la réduction des distances lunaires. La 1^{re}, en ce que les éphémérides donnant la parallaxe horizontale de la lune pour un lieu particulier du globe, si on l'emploie pour un autre lieu, on commet une erreur qui dépend de la différence des parallaxes qui conviennent aux deux latitudes. La 2^{de}, en ce que la verticale hors de l'équateur et des poles n'aboutissant pas au centre de la Terre, les hauteurs observées ne sont pas celles qu'on prendroit si le globe étoit sphérique. On remédieroit à la première cause, en appliquant à la parallaxe horizontale de la lune tirée des éphémérides, l'équation nécessaire pour la réduire à la situation actuelle du vaisseau. Pour la seconde cause, l'on pourroit appliquer à chaque hauteur observée la correction convénable, qui est égale à l'angle formé par la verticale et le rayon terrestre, multiplié par le cosinus de l'azimuth de l'astre. Mais ce procédé seroit embarrassant, et peu précis; car il exige qu'on prenne les azimuths de la lune et du soleil, en même tems qu'on observe leur dis-

tance, et les azimuths donnés par le compas doivent en général être très fautifs. Nous chercherons, donc, des formules pour arriver au même but seulement par le calcul.

Entre les équations précédentes, il n'y a que celle qui dépend de u , où l'influence des causes mentionnées mérite d'être considérée; car la correction v est ordinairement trop petite pour y avoir égard. Nous pourrons aussi négliger dans u la réfraction, en nous bornant à la parallaxe, qui est l'élément le plus considérable. Ainsi l'équation $-u \left(\frac{\sin. b - \cos. d \sin. a}{\sin. d \cos. a} \right)$,

se réduit à $-\rho \left(\frac{\sin. b - \cos. d \sin. a}{\sin. d \cos. a} \right)$, ou $-P \left(\frac{\sin. b - \cos. d \sin. a}{\sin. d} \right)$.

Supposons que P est la parallaxe horizontale équatoriale, et différencions en supposant P, a, b variables. On aura, en considérant que la différentielle de P est constamment négative,

$$\delta P \left(\frac{\sin. b - \cos. d \sin. a}{\sin. d} \right) = P \left(\frac{\delta b \cos. b - \delta a \cos. d \sin. a}{\sin. d} \right)$$

Ce sont les corrections qu'on doit appliquer à la distance vraie calculée par les méthodes ordinaires, où δP exprime la différence entre la parallaxe équatoriale et celle qui convient à la latitude du lieu de l'observation. Il s'agit à présent de déduire des formules propres pour le calcul.

Représentons l'azimuth de la lune (compté depuis le quart de méridien où se trouve le pôle élevé) par F , sa déclinaison par B ; l'azimuth du soleil ou de l'étoile (comptés de la même manière) par f , sa déclinaison par b ; et l'angle de la verticale et du rayon terrestre pour le lieu de l'observation par n . Nous aurons

$$\delta a = -n \cos. F = -n \left(\frac{\sin. B - \sin. l \sin. a}{\cos. l \cos. a} \right)$$

et
$$\delta b = -n \cos. f = -n \left(\frac{\sin. b - \sin. l \sin. b}{\cos. l \cos. b} \right)$$

Substituant ces expressions, et faisant la somme des corrections = x , on déduira

$$\begin{aligned} x &= \delta P \left(\frac{\sin. h - \cos. d \sin. a}{\sin. d} \right) + Pn \left(\frac{\sin. b \cos. h - \sin. l \sin. h \cos. h}{\sin. d \cos. l \cos. h} - \frac{\sin. B \cos. d \cos. a - \sin. l \cos. d \sin. a \cos. a}{\sin. d \cos. l \cos. a} \right) \\ x &= \delta P \left(\frac{\sin. b - \cos. d \sin. a}{\sin. d} \right) + Pn \left(\frac{\sin. b - \sin. l \sin. b - \sin. B \cos. d + \sin. l \cos. d \sin. a}{\sin. d \cos. l} \right) \\ x &= \delta P \left(\frac{\sin. b - \cos. d \sin. a}{\sin. d} \right) - Pn \tan. l \left(\frac{\sin. b - \cos. d \sin. a}{\sin. d} \right) + Pn \left(\frac{\sin. b - \sin. B \cos. d}{\sin. d \cos. l} \right) \\ x &= (\delta P - Pn \tan. l) \left(\frac{\sin. b - \cos. d \sin. a}{\sin. d} \right) + Pn \left(\frac{\sin. b - \sin. B \cos. d}{\sin. d \cos. l} \right). \end{aligned}$$

Représentons l'applatissage de la Terre par e , en supposant le demi-diamètre de l'équateur = 1, et nous aurons $\delta P = Pe \sin.^2 l$, et $n = 2e \sin. l \cos. l$; * ce qui, étant substitué, donne

$$\begin{aligned} x &= (\delta P - 2 \delta P \tan. l \cot. l) \left(\frac{\sin. b - \cos. d \sin. a}{\sin. d} \right) + 2Pe \sin. l \left(\frac{\sin. b - \sin. B \cos. d}{\sin. d} \right) \\ x &= -\delta P \left(\frac{\sin. b - \cos. d \sin. a}{\sin. d} \right) + 2Pe \sin. l \left(\frac{\sin. b - \sin. B \cos. d}{\sin. d} \right). \end{aligned}$$

* Voici la démonstration.

Représentons le demi-axe terrestre par b , supposant le demi-diamètre de l'équateur = 1, et l'applatissage $1 - b = e$. Et soit, pour un lieu particulier, l la latitude, r le rayon terrestre, c l'angle formé au centre de la terre par le rayon et le demi-diamètre de l'équateur, et n l'angle de la verticale et du rayon terrestre.

On aura (Voyez la *Trigonométrie* de Mr. CAGNOLI) $\tan. c = b^2 \tan. l$. Faisons aussi $\tan. z = b \tan. l$.

On déduira $r^2 = \frac{\cos.^2 z}{\cos.^2 c} = \frac{\sec.^2 c}{\sec.^2 z} = \frac{1 + b^2 \tan.^2 l}{1 + b^2 \tan.^2 l}$. Mais on a, à très peu près, $b^2 = 1 - 2e$, $b^4 = 1 - 4e$, et $1 - r^2 = 2 - 2r$; donc, en substituant, on tirera $2 - 2r = 1 - \frac{1 + b^2 \tan.^2 l}{1 + b^2 \tan.^2 l}$, et $2r = 1 + \frac{1 + b^2 \tan.^2 l}{1 + b^2 \tan.^2 l}$, qui se réduit à $2r = \frac{2e \tan.^2 l}{1 + \tan.^2 l} = \frac{2e \tan.^2 l}{\sec.^2 l} = 2e \sin.^2 l$. Ainsi $1 - r = e \sin.^2 l$, et par conséquent $P - Pr = Pc \sin.^2 l$.

L'angle n est = $l - c$. Par conséquent $\tan. n = \frac{\tan. l - \tan. c}{1 + \tan. l \tan. c} = \frac{\tan. l - b^2 \tan. l}{1 + b^2 \tan.^2 l}$, et substituant $b^2 = 1 - 2e$, on aura $\tan. n = \frac{2e \tan. l}{1 + \tan.^2 l - 2e \tan.^2 l}$, qui se réduit à $\tan. n = \frac{2e \tan. l}{\sec.^2 l} = 2e \sin. l \cos. l$, d'où, parceque n est toujours petit, il résulte $n = 2e \sin. l \cos. l$.

Or, si, dans la réduction de la distance, l'on emploie la parallaxe horizontale pour l'équateur, augmentée de la différence δP entre cette parallaxe et celle qui convient à la latitude du lieu de l'observation, la distance vraie, ainsi calculée, se trouvera corrigée de la quantité $\delta P \left(\frac{\sin. b - \cos. d \sin. a}{\sin. d} \right)$; car l'équation dépendante de cet élément, sera alors $-(P + \delta P) \left(\frac{\sin. b - \cos. d \sin. a}{\sin. d} \right)$.

On pourra, donc, employer la parallaxe préparée de cette manière;* ce qu'on pourra faire très facilement, car si les éphémérides donnent la parallaxe pour un lieu particulier, il suffira d'y ajouter l'équation relative à la latitude de ce lieu, ainsi que l'équation relative au lieu de l'observation.

A la distance obtenue, l'on devra appliquer la quantité

$$2 P e \sin. l \left(\frac{\sin. b - \sin. B \cos. d}{\sin. d} \right). \text{ Soit cette équation.} = \epsilon, \text{ et nous aurons}$$

$$\epsilon = 2 P e \sin. l \cos. B \left(\frac{\sin. b - \sin. B \cos. d}{\cos. B \sin. d} \right)$$

$$\epsilon = 2 P e \sin. l \cos. B \left(1 - 1 + \frac{\sin. b - \sin. B \cos. d}{\cos. B \sin. d} \right)$$

$$\epsilon = 2 P e \sin. l \cos. B - 2 P e \sin. l \cos. B \left(1 + \frac{\sin. B \cos. d - \sin. b}{\cos. B \sin. d} \right)$$

$$\epsilon = 2 P e \sin. l \cos. B - 2 P e \sin. l \left(\frac{\cos. B \sin. d + \sin. B \cos. d - \sin. b}{\sin. d} \right)$$

$$\epsilon = 2 P e \sin. l \cos. B - 2 P e \sin. l \left(\frac{\sin. (d + B) - \sin. b}{\sin. d} \right)$$

$$\epsilon = 2 P e \sin. l \cos. B - 4 P e \sin. l \frac{\cos. \frac{1}{2} (d + B + b) \sin. \frac{1}{2} (d + B - b)}{\sin. d}$$

Cette expression a l'avantage de ne demander aucune distinction de cas, car on devra toujours ajouter à la distance cal-

* C'est ainsi que le savant Mr. DE BORDA le pratique, dans la méthode qu'il nous a donnée à ce sujet (voyez son *Traité du Cercle de Reflexion*), et que je n'ai pas manqué de consulter avant de travailler à la rédaction de cet article.

culée l'équation $2Pe \sin. l \cos. B$, et retrancher l'équation $4Pe \sin. l \frac{\cos. \frac{1}{2}(d+B+b) \sin. \frac{1}{2}(d+B-b)}{\sin. d}$; et le résultat restera, ainsi, depouillé des erreurs qui dépendent de l'appatissement de la Terre.

On pourra, dans ces opérations, employer toujours pour P la parallaxe horizontale moyenne, $57'$, et l'on aura 1.32855 pour le logarithme constant de $2Pe$, et 1.62958 pour le logarithme constant de $4Pe$, en supposant l'appatissement $= \frac{1}{321}$.

Pour faciliter le calcul, j'ai construit deux tables, dont l'une donne l'équation $2Pe \sin. l \cos. B$, et l'autre le logarithme de $4Pe \sin. l$.

On pourroit aussi trouver

$$\varepsilon = -2Pe \sin. l \cos. B + 2Pe \sin. l \cos. B \left(1 + \frac{\sin. b - \sin. B \cos. d}{\cos. B \sin. d} \right)$$

$$\varepsilon = -2Pe \sin. l \cos. B + 2Pe \sin. l \left(\frac{\sin. b + \cos. B \sin. d - \sin. B \cos. d}{\sin. d} \right)$$

$$\varepsilon = -2Pe \sin. l \cos. B + 2Pe \sin. l \left(\frac{\sin. b + \sin. (d-B)}{\sin. d} \right)$$

$$\varepsilon = -2Pe \sin. l \cos. B + 4Pe \sin. l \frac{\sin. \frac{1}{2}(b+d-B) \cos. \frac{1}{2}(b-(d-B))}{\sin. d}$$

Si l'on préféroit d'employer les distances au pôle élevé, au lieu des déclinaisons des astres, on auroit (en appellant B' , et b' les distances polaires correspondantes à B , et b)

$$\varepsilon = -2Pe \sin. l \sin. B' + 4Pe \sin. l \frac{\sin. \frac{1}{2}(d+B'+b') \sin. \frac{1}{2}(d+B'-b')}{\sin. d}$$

ou bien

$$\varepsilon = 2Pe \sin. l \sin. B' - 4Pe \sin. l \frac{\sin. \frac{1}{2}(b'+(d-B')) \sin. \frac{1}{2}(b'-(d-B'))}{\sin. d}$$

APPENDICE.

Exemples des calculs de quelques unes des Solutions établies ci-dessus, par les Tables ordinaires.

EXEMPLE I.

Calcul de la Latitude du lieu par deux Hauteurs du Soleil, et l'Intervalle de Tems écoulé entre les Observations.

Observations faites dans l'hémisphère septentrional.

Hauteurs vraies ☉	Demi-intervalle	Déclinaison ☉	Distance polaire ☉
45° 5' 42" 5 30 6	1 ^h 30' = 22° 30'	12° 0' N	78° 0'
	L. cos. déclinaison	9.99040	
	L. sin. demi-intervalle	9.58284	
	Somme - - -	9.57324	L. sin. A - 21° 58' 56"
	L. sin. - - -	9.91867	Dist. polaire 78 0 0
	L. sin. demi-intervalle	9.58284	Différence 56 1 4
2A - - - 43° 57' 52"	C. l. sin - - -	0.15851	2A - - - 43 57 52
Petite hauteur 5 36 6	Somme - - -	10.66002	L. sin. - 42 32 22
Grande hauteur 45 5 42	Demi-somme - -	9.83001	
Somme 94 39 40	L. cos. - - -	9.83108	
Demi-somme - 47 19 50	L. sin. - - -	8.59115	
Différence - 2 14 8	C. l. sin. 2A - -	0.15851	
	C. l. cos. petite hauteur	0.00208	
	Somme - - -	18.58282	
	Demi-somme - -	9.29141	L. sin. - 11 16 51
Distance polaire 78 0 0	L. sin. - - -	9.71509	Différence 31 15 31
Petite hauteur 5 36 6	Demi-l. cos. pet. haut.	4.99896	
Somme (+ 90°) 173 36 6	Demi-l. cos. declin.	4.99520	
Demi-somme 86 48 3	C. sin. - - -	0.00068	
	L. sin. N (somme)	9.70993	
	L. cos. N - - -	9.93375	
	Différence - -	9.93307	L. sin. (B) - 59 0 3
Latitude du lieu (2 B - 90°)			- 28 0 6

EXEMPLE II.

Calcul de la Latitude du lieu par deux Hauteurs du Soleil, et l'Intervalle de Tems écoulé entre les Observations, ayant d'ailleurs la Latitude estimée.

En déduisant premièrement l'Angle horaire moyen.

	Hauteurs vraies ☉	Heures des observ.	Lat. estimée	Déclinaison ☉
1 ^{re} Observation	30° 13' 14" - -	7 ^h 32' 16"		
2 ^{de} Observation	50 3 55 - -	10 27 48 - - -	56° 29' S	- 20° 6' 40" S
	Intervalle -	2 55 32		
	Demi-intervalle	1 27 46 = 21° 56' 30"		
Grande hauteur	- 50° 3' 55"			
Petite hauteur	- 30 13 14		1 ^{re} supposition.	2 ^{me} supposition.
Somme -	- 80 17 9			
Demi-somme -	- 40 8 34	L. cos. - -	9 88334	
Différence -	- 9 55 20	L. sin. - -	9 23631	
Demi-intervalle -	- 21 56 30	C. l. sin. - -	0 42752	
Déclinaison -	- 20 6 40	C. l. cos. - -	0 02732	
		Somme - -	9 57449	9 57449
Latitude estimée (-30')	55 59 0	C. l. cos. - -	0 25225	+ 1° 0 26370
Horaire moyen -	42 8 47	L. sin. (somme)	9 82674	43° 32' 50" 9 83819
Demi-intervalle -	21 56 30	- - - -	- - -	21 56 30
Petit horaire -	20 12 17	- - - -	- - -	21 36 20
Demi-petit horaire -	10 6 8	C. l. sin. - -	0 75596	10 48 10 0 72716
		Demi-c. l. cos. decl. -	0 01366	- - 0 01366
		Demi-c. l. cos. lat. -	0 12612	- - 0 13185
Demi-(gr. haut. +90°)	70 1 57	L. sin. - -	9 97308	- - 9 97308
		L. tan. A (somme)	10 86882	- - 10 84575
		L. sin. A -	9 99606	- - 9 99563
Demi-dist. méridienne	18 28 30	L. cos. (différence)	9 97702	18 18 16 9 97745
Distance méridienne	36 57 0	- - - -	- - -	36 36 32
Déclinaison -	20 6 40	- - - -	- - -	20 6 40
Latitude calculée -	57 4	- - - -	- - -	56 43
Latitude supposée -	55 59	- - - -	- - -	56 59
	1 5	- Somme 1° 21'	- - -	0 16
Équation de la deuxième latitude supposée		$\frac{16' \times 60'}{81'}$	0 12	
Latitude du lieu -		- - - -	- - -	56 47

Remarque. Pour appliquer l'équation trouvée à l'une des latitudes calculées de la manière convenable, afin de déduire la latitude corrigée, on pourra consulter ce qui a été dit ci-dessus dans les pages 60, et 61.

EXEMPLE III.

Calcul de la Latitude du lieu par deux Hauteurs du Soleil, et l'Intervalle de Tems écoulé entre les Observations, ayant d'ailleurs la Latitude estimée.

En déduisant premièrement le grand Angle borair.

	Hauteurs vraies \odot	Heures des observ.	Lat. estimée	Déclinaison \odot
1 ^{re} Observation	68° 29' 50"	11 ^h 30' 20",5	39° 38' N	20° 41' 33" N
2 ^{de} Observation	71 9 15	12 27 1	-	20 41 7
Intervalle	-	0 56 40,5 =	-	14 10 7,5
Différence en longitude contractée par le vaisseau entre les observations	-	-	-	0 7 0 à l'ouest
Intervalle préparé pour le calcul	-	-	-	14 3 7,5
			1 ^{re} supposition.	2 ^{me} supposition.
Petite hauteur	68° 29' 50"			
Latitude estimée (-30')	39 8 0	C. l. cos.	0.11032	+ 1° 0.11660
Distance polaire	69 18 27	C. l. sin.	0.02896	- - 0.02896
Somme	176 56 17			
Demi-somme	88 28 8	L. cos.	8.42683	+ 30' 8.25516
Différence	19 58 18	L. sin.	9.53346	+ 30' 9.54375
		Somme	18.09957	- - 17.94447
Demi-grand horaire	6 26 20	L. sin. (demi-somme)	9.04978	5° 22' 57" 8.97223
Demi-intervalle	7 1 34			7 1 34
Demi-petit horaire	0 35 14	C. l. sin.	1.98933	1 38 37 1.54238
		Demi-c. l. cos. lat.	0.05516	- - 0.05830
		Demi-c. l. sin. dist. p.	0.01448	- - 0.01448
Demi-(gr. haut. +90°)	80 34 37	L. sin.	9.99410	- - 9.99410
		L. tan. A (somme)	12.05307	- - 11 60926
		L. sin. A	9.99998	- - 9.99987
Demi-dist. méridienne	9 24 25	L. cos. (différencé)	9.99412	9 19 9 9.99423
Distance méridienne	18 48 50			18 38 18
Déclinaison	20 41 7			20 41 7
Latitude calculée	39 30			39 19
Latitude supposée	39 5			40 5
Différence	0 25	Somme 71'		0 46
		Equation de la deuxième latitude supposée $\frac{46' \times 60'}{71'}$		0 39
		Latitude du lieu		39 26

Remarque. Sur la manière d'appliquer l'équation à la latitude calculée par l'une des suppositions, pour déduire la latitude corrigée, je dois aussi renvoyer ici aux pages 60, et 61.

EXEMPLE IV.

Calcul de l'Angle horaire d'un Astre, par sa Hauteur et sa Déclinaison, et la Latitude du lieu.

Hauteur	$45^{\circ} 21' 54''$.	Déclinaison	$13^{\circ} 41' 36''$ N.	Lat. du lieu	$23^{\circ} 20' N.$
Latitude	- - - -	$23^{\circ} 20' 0''$	C. l. cos.	- - -	0.0370551
Déclinaison	- - - -	<u>13 41 36</u>	C. l. cos.	- - -	[0.0125045
					184
Distance méridienne au zénith		9 38 24			
Complém. de la haut. a 90°	-	<u>44 38 6</u>			
Somme	- - - -	54 16 30			
Demi-somme	- - - -	27 8 15	L. sin.	- - -	[9.6590240
					616
Différence	- - - -	17 29 51	L. sin.	- - -	[9.4777409
					3408
			Somme	- - -	19.1867459
Demi-angle horaire	- - -	23 5 2	L. sin. (Demi-somme)	-	<u>9.5933729</u>
Angle horaire	- - -	46 10 4	=	$3^h 4' 40'' 16''$.	

Remarque. Je ne place ici cet exemple que pour en donner un des avantages qu'on peut tirer de disposer les formules de manière à rendre les quantités et leurs variations, ou différences, additives ; en réduisant par ce moyen les opérations à la simple addition totale, et en épargnant la peine d'appliquer séparément les parties proportionnelles. Dans le calcul précédent (qui a été fait avec des tables qui donnent les logarithmes de minute en minute), on voit que pour chaque sinus, ou chaque complément arithmétique de cosinus (ou sécante), j'ai pris ce qui convient aux degrés et aux minutes, et que j'ai écrit dessous les parties proportionnelles pour les secondes, afin d'ajouter le tout ensemble.

EXEMPLE V.

Calcul des Équations qu'on doit appliquer à la Distance apparente de la Lune au Soleil, ou à une Étoile, pour avoir la Distance vraie.

Hauteur appar. \odot	$6^{\circ} 27' 34''$	Hauteur appar. \llcorner	$54^{\circ} 11' 57''$	Distance appar. \odot	\llcorner	$108^{\circ} 42' 3''$	
Correction de la haut.	$\odot 7' 33''$	Correction de la haut.	$\llcorner 31' 42''$	Parallaxe horizontale \llcorner	\llcorner	$55' 19''$	
Distance \llcorner	$108^{\circ} 42' 3''$	C. l. sin.	0.0236			0.0236	
Hauteur \odot	$6 27 34$	C. l. cos.	0.0028				
Hauteur \llcorner	$54 11 57$	C. l. cos.				0.2329	
Somme	$169 21 34$	L. cos.	8.9669			8.9669	
Demi-somme	$84 40 47$	L. sin.				9.9908	
Première différence	$78 13 13$	L. sin.	9.7053			0.3010	
Deuxième différence	$30 28 50$	L. constant	0.3010			0.3010	
Correction haut. \odot	453	L. -	2.6561	C. h. \llcorner	$1902''$	L. - - - 3.2792	
Première équation	$45,3$	L. (somme)	<u>1.6557</u>	Deux. éq.	$622,9$	L. (somme)	<u>2.7944</u>

Distance apparente	- - - -	- - - -	- - - -	$108^{\circ} 42' 3''$
Correction haut. \llcorner	- - - $31' 42''$	Correction haut. \odot	+	$0 7 33$
Première équation	- - - $45,3$	Deuxième équation	+	<u>$0 10 22,9$</u>
				$108 59 58,9$
				<u>$0 32 27,3$</u>
Distance corrigée des équations principales				<u>$108 27 31,6$</u>

Remarque. La distance vraie, selon la méthode de M. DE BORDA, est presque la même (voyez l'exemple dans les Tables de Logarithmes de CALLET), mais, cependant, je déduirai les autres corrections, pour montrer la manière de faire ces calculs.

Correct. haut. \odot	$453''$	L. Prem. éq.	1.6557	Correct. haut. \llcorner	$1902''$	L. Deux. éq.	2.7944
Demi-prem. éq.	23	L. - - -	2.6335	Demi-deux. équat.	311	L. - - -	3.2017
Différence - -	430	L. cot. -	9.5295	Différence - -	1591	L. cot. - -	9.5295
Distance appar. - -		L. constant	<u>4.6856</u>	Distance appar. - -		L. const. -	<u>4.6856</u>
Troisième équat. $0,0$	L. (somme).	8.5043 (-10)		Quatrième équat. $1,6$	L. (somme)	<u>8.2112</u>	
Troisième équat. - -	Demi-L -	4.2521 (-5)		Distance précédente	- - - -	$108^{\circ} 27' 31'',6$	
Quatrième équat. - -	Demi-L -	0.1056		Troisième éq. - $0'',0$			
Distance appar. - -	C. l. cos. -	0.4940		Quatrième éq. - $1,6$			
	L. constant	<u>0.3010</u>		Cinquième éq. + $1,4$			$0,2$
Cinquième correct. $1,4$	L. (somme)	<u>0.1527</u>		Distance réduite	- - - -		<u>$108 27 31,4$</u>

Les équations troisième et quatrième seroient positives, si la distance n'excédoit pas 90° , et c'est la seule distinction de cas qu'il faut faire dans le procédé ci-dessus.

EXEMPLE VI.

Calcul des Équations qu'on doit appliquer à la Distance apparente de la Lune au Soleil, ou à une Étoile, pour avoir la Distance vraie.

En se servant des Requisite Tables.

Hauteur apparente ζ	-	49° 57'	Hauteur apparente *	-	64° 19'
Parallaxe horizontale ζ	-	57 8".	Distance apparente ζ *	-	29 24 46'
Distance	-	29° 24' 46"	L. sin.	-	9 6912
Hauteur ζ	-	49 57 0	L. cos.	-	9 6912
Hauteur *	-	64 19 0	L. cos.	-	9 8085
Somme	-	143 40 46			
Demi-somme	-	71 50 23	L. sec.	-	0.5061
Première différence	21 53 23		L. cosec.	-	0.4286
Deuxième différence	7 31 23				
Correct. de la haut. *	0 0 27		L. p.	-	2.6021
			L. constant	-	9.6990
Première équation	-	0 0 29	L. p. (somme)	2.5639	Deux. éq. 9'17" L.p. (som.) 1.2875
Correct. haut. ζ	-	0 35 58			
Demi-deux. équation	0 4 38		L. p. deux. éq.	1.2875	Distance appar. - - 29° 24' 46"
Différence	-	0 31 20	L. p.	0.7592	Correct. de la haut. * + 0 0 27
			L. tan. distance appar.	9.7512	Deuxième équation + 0 9 17
			L. constant	1 2810	
			L. p. (somme)	3 0789	Troisième équation + 0 0 9
			Correct. de la haut. ζ	- 35' 58"	29 34 39
			Première équation	- 29	0 36 27
					Distance réduite - - 28 58 12

Remarque. Quand la distance excède 90°, la troisième équation devient négative.

Je ne déduirai pas les autres équations ; car le degré d'approximation du calcul qui précède est celui que la plupart des Navigateurs estimeront suffisant pour la pratique.

Le même exemple calculé par la méthode de Mr. WITCHELL (voyez les *Requisite Tables*) donne 28° 58' 11", pour la distance réduite.

DE MENDOZA Y RIOS.

A D D I T I O N.

Contenant une Méthode pour réduire les Distances lunaires. Par Mr. H. Cavendish, Membre de la Société Royale, &c.

MR. CAVENDISH m'ayant fait l'honneur de me communiquer la méthode qu'il a trouvé pour réduire les distances lunaires, je profite de la permission de ce savant, pour la faire connoître au public, en plaçant ici un extrait de ce qu'il m'a écrit à ce sujet, dans les propres mots de l'auteur.

Extract of a Letter from Henry Cavendish, Esq. to Mr. Mendoza y Rios, January, 1795.

“ The methods in which the whole distance of the moon and star is computed, particularly yours, require fewer operations than those in which the difference of the true and apparent places is found; but yet, as in the former methods, it is necessary either to take proportional parts, or to use very voluminous tables; I am much inclined to prefer the latter. This induced me to try whether a convenient method of the latter kind might not be deduced from the fundamental proposition used in your paper, and I have obtained the following, which has the advantage of requiring only short tables, and wanting only one proportional part to be taken, and I think seems shorter than any of the kind I have met with.

“ Let b and H be the apparent and true altitude of the star;

l and L the apparent and true altitude of the moon, g and G the apparent and true distance of the moon and star. Let the sine and cosine of $g = d$ and δ , the sine and cosine of $l = a$ and α , the sine and cosine of $b = b$ and β ; and the sine of the actual and mean horizontal parallax $= p$ and π ; and let the sine of $L = a - m + p e$, and its cosine $= \alpha (1 + \mu - p \epsilon)$ and let the sine of $H = b - n$, and its cosine $= \beta (1 + \nu)$.

“ Then the cosine of $G = \delta(1 + \mu - p \epsilon)(1 + \nu) + (a - m + p e)(b - n) - ab(1 + \mu - p \epsilon)(1 + \nu)$, which equals $\delta + \delta \mu + \delta \nu - \delta p \epsilon + \delta \mu \nu - \delta p \epsilon \nu + ab - bm + b p e - an + nm - n p e - ab - \alpha b \mu + ab p \epsilon - ab \nu - ab \mu \nu + ab \nu p \epsilon = \delta + \delta \mu + \delta \nu - \delta p \epsilon - bm - b a \mu + b p e + b a p \epsilon - an - ab \nu + nm - n p e - ab \mu \nu + ab \nu p \epsilon + \delta \mu \nu - \delta p \epsilon \nu$.

“ To make use of this rule, it must be considered that the quantity $\delta \mu \nu - \delta p \epsilon \nu$ is so small that it may safely be disregarded; but $nm - n p e - ab \mu \nu + ab \nu p \epsilon$, if the altitudes are not more than 5° , may amount to about $12''$, and therefore ought not to be neglected. The quantity $e + a \epsilon$ also differs very little from one, but is not quite equal to it. Let therefore a table be made under a double argument, namely, the altitudes of the moon and star, giving the value of $nm - n p e - ab \mu \nu + ab \nu p \epsilon + b p e + b a p \epsilon - b \pi$, answering to different values of these altitudes, which call A. Let a second table be made under a double argument, namely, the altitude of the star and the apparent distance of the moon and star, giving the value of $\delta \nu$, which call D. Let a third table be made with the observed altitude for argument, giving the logarithm of $am + a^2 \mu$; and let this quantity, answering to the moon's altitude, be called M, and that answering to the

star's altitude, N; observing that the same table will do for the moon and star; but a fourth table should be made for the sun, so as to include its parallax; and, lastly, let a fifth table be made, with the moon's altitude for argument, giving the logarithm of $\frac{\varepsilon}{a} - \frac{\mu}{\pi a}$, which call C. Then will $\cos. G = \delta - \delta a p C - \frac{bM}{a} - \frac{aN}{b} + b p + D - A$.

“It must be observed that $\delta a p C = \delta p \varepsilon - \frac{\delta \mu p}{\pi}$, whereas it ought to equal $\delta p \varepsilon - \delta \mu$; but μ cannot exceed $57''$, and the horizontal parallax cannot differ from the mean by more than $\frac{1}{15}$ part of the whole; so that the error arising from thence cannot exceed $3''$ or $4''$. This small error however may be diminished by giving the quantity C for more than one horizontal parallax.”

Addition to the foregoing Letter.

“I have procured tables of the above-mentioned kind to be computed, which are intended to be inserted in a work now printing by Mr. MENDOZA Y RIOS. Allowance is made in them for the alteration of the refractive power of the atmosphere, which is done by two new tables, one giving the correction of the logarithms M and N, and the other the sum of the corrections of $\delta \mu$ and $\delta \nu$. Now it must be observed, that the quantities μ and ν vary only from $57''$ to $51''$; and therefore the corrections of $\delta \mu$ and $\delta \nu$, may, without any material error, be considered as the same at all altitudes; and therefore the sum of the corrections may be comprehended in a table, under a double argument, namely, the refractive power of the atmosphere and the apparent distance.

“ In order to avoid as much as possible the inconvenience arising from using negative quantities, or giving different cases, the table D is continued to 125° of apparent distance, and the numbers in the table A are increased by 0,0003, so as to make them always positive; and to compensate this, the numbers in D are increased by 0,0002, and those in the correction of $\delta\mu + \delta\nu$ by 0,0001. It was found proper also to give the table C for four different values of horizontal parallax.

“ The above tables are short, and do not require proportional parts to be taken. The only part of the work in which this is wanted, is in finding the angle answering to the natural cosine of the true distance. In finding the natural cosine of the apparent distance this is avoided, by neglecting the odd seconds in working the problem, and adding them to the result.”

IV. *On the Nature of the Diamond.* By Smithson Tennant,
Esq. F. R. S.

Read December 15, 1796.

SIR ISAAC NEWTON having observed that inflammable bodies had a greater refraction, in proportion to their density, than other bodies, and that the diamond resembled them in this property, was induced to conjecture that the diamond itself was of an inflammable nature. The inflammable substances which he employed were camphire, oil of turpentine, oil of olives, and amber; these he called "fat, sulphureous, unctuous bodies;" and using the same expression respecting the diamond, he says, it is probably "an unctuous body coagulated." This remarkable conjecture of Sir ISAAC NEWTON has been since confirmed by repeated experiments. It was found that, though the diamond was capable of resisting the effects of a violent heat when the air was carefully excluded, yet that on being exposed to the action of heat and air, it might be entirely consumed. But as the sole object of these experiments was to ascertain the inflammable nature of the diamond, no attention was paid to the products afforded by its combustion; and it still therefore remained to be determined whether the diamond was a distinct substance, or one of the known inflammable bodies. Nor was any attempt made to decide this question till M. LAVOISIER, in 1772, undertook a series of

experiments for this purpose. He exposed the diamond to the heat produced by a large lens, and was thus enabled to burn it in close glass vessels. He observed that the air in which the inflammation had taken place had become partly soluble in water, and precipitated from lime-water a white powder which appeared to be chalk, being soluble in acids with effervescence. As M. LAVOISIER seems to have had little doubt that this precipitation was occasioned by the production of fixed air, similar to that which is afforded by calcareous substances, he might, as we know at present, have inferred that the diamond contained charcoal; but the relation between that substance and fixed air, was then too imperfectly understood to justify this conclusion. Though he observed the resemblance of charcoal to the diamond, yet he thought that nothing more could be reasonably deduced from their analogy, than that each of those substances belonged to the class of inflammable bodies.

As the nature of the diamond is so extremely singular, it seemed deserving of further examination; and it will appear from the following experiments, that it consists entirely of charcoal, differing from the usual state of that substance only by its crystallized form. From the extreme hardness of the diamond, a stronger degree of heat is required to inflame it, when exposed merely to air, than can easily be applied in close vessels, except by means of a strong burning lens; but with nitre its combustion may be effected in a moderate heat. To expose it to the action of heated nitre free from extraneous matters, I procured a tube of gold, which by having one end closed might serve the purpose of a retort, a glass tube being adapted to the open end for collecting the air produced. To be certain that the gold vessel was perfectly closed,

and that it did not contain any unperceived impurities which could occasion the production of fixed air, some nitre was heated in it till it had become alkaline, and afterwards dissolved out by water; but the solution was perfectly free from fixed air, as it did not affect the transparency of lime-water. When the diamond was destroyed in the gold vessel by nitre, the substance which remained precipitated lime from lime-water, and with acids afforded nitrous and fixed air; and it appeared solely to consist of nitre partly decomposed, and of aerated alkali.

In order to estimate the quantity of fixed air which might be obtained from a given weight of diamonds, two grains and a half of small diamonds were weighed with great accuracy, and being put into the tube with a quarter of an ounce of nitre, were kept in a strong red heat for about an hour and a half. The heat being gradually increased, the nitre was in some degree rendered alkaline before the diamond began to be inflamed, by which means almost all the fixed air was retained by the alkali of the nitre. The air which came over was produced by the decomposition of the nitre, and contained so little fixed air as to occasion only a very slight precipitation from lime-water. After the tube had grown cold, the alkaline matter contained in it was dissolved in water, and the whole of the diamonds were found to have been destroyed. As an acid would disengage nitrous air from this solution as well as the fixed air, the quantity of the latter could not in that manner be accurately determined. To obviate this inconvenience, the fixed air was made to unite with calcareous earth, by pouring into the alkaline solution a sufficient quantity of a saturated solution of marble in marine acid. The vessel which contained

them being closed, was left undisturbed till the precipitate had fallen to the bottom, the solution having been previously heated that it might subside more perfectly. The clear liquor being found, by means of lime-water, to be quite free from fixed air, was carefully poured off from the calcareous precipitate.* The vessel which was used on this occasion was a glass globe, having a tube annexed to it, that the quantity of the fixed air might be more accurately measured. After as much quick-silver had been poured into the glass globe containing the calcareous precipitate as was necessary to fill it, it was inverted in a vessel of the same fluid. Some marine acid being then made to pass up into it, the fixed air was expelled from the calcareous earth; and in this experiment, in which two grains and a half of diamonds had been employed, occupied the space of a little more than 10.1 ounces of water.

The temperature of the room when the air was measured, was at 55°, and the barometer stood at about 29.8 inches.

From another experiment made in a similar manner with one grain and a half of diamonds, the air which was obtained occupied the space of 6.18 ounces of water, according to which proportion the bulk of the fixed air from two grains and a half would have been equal to 10.3 ounces.

The quantity of fixed air which was thus produced by the diamond, does not differ much from that which, according to M. LAVOISIER, might be obtained from an equal weight of charcoal. In the Memoirs of the French Academy of Sciences for

* If much water had remained, a considerable portion of the fixed air would have been absorbed by it. But by the same method as that described above, I observed, that as much fixed air might be obtained from a solution of mineral alkali, as by adding an acid to an equal quantity of the same kind of alkali.

the year 1781, he has related the various experiments which he made to ascertain the proportion of charcoal and oxygen in fixed air. From those which he considered as most accurate, he concluded that 100 parts of fixed air contain nearly 28 parts of charcoal and 72 of oxygen. He estimates the weight of a cubic inch of fixed air under the pressure and in the temperature abovementioned, to be .695 parts of a grain. If we reduce the French weights and measures to English, and then compute how much fixed air, according to this proportion, two grains and a half of charcoal would produce, we shall find that it ought to occupy very nearly the bulk of 10 ounces of water.

M. LAVOISIER seems to have thought that the aerial fluid produced by the combustion of the diamond was not so soluble in water as that procured from calcareous substances. From its resemblance, however, in various properties, hardly any doubt could remain that it consisted of the same ingredients; and I found, upon combining it with lime, and exposing it to heat with phosphorus, that it afforded charcoal in the same manner as any other calcareous substance.

V. *A Supplement to the Measures of Trees, printed in the Philosophical Transactions for 1759.* By Robert Marsham, Esq. F. R. S.

Read December 22, 1796.

THESE measures were all taken by myself, except the second, of the ash in Scotland; and that I believe is fair. As that is the largest ash, and as thriving as any I had seen, I was desirous to procure a second measure of it. The measures (where there was no impediment) were taken at five feet from the earth, as the easiest height to run the line even, and a fair height for the bulk of the body. For most trees (at least oaks and chesnuts) are frequently found to be one-third more in circumference at one foot than at five. Where I have measures of more than one tree of the same kind, I give the largest and a smaller, to show the different proportion of the increase of their different sizes: and as trees standing single generally increase more than those in groves, I mark them with an S. and a G. as the difference is more than would be expected by those that think little of trees.

In 1719 I had about two acres sowed with acorns, and from 1729 to 1770 I planted oaks from this grove, always leaving the best plants standing for the future grove: but most of the transplanted trees are already larger than those that were not removed; the largest of which is now (1795) but five feet

6 inches 8 tenths in circumference; and the largest transplanted tree (which was planted in 1735) is 8 ft. 8 in. 7 tenths, viz. near 38 inches gained by transplanting in 60 years. And in beeches from seed, in 1733, the largest is now (1795) but 6 feet 9 inches; and the largest transplanted beech is 7 feet 5 inches 1 tenth, viz. 8 inches larger, although the transplanted beech is eight years younger than that from the seed. This proves that it is better to plant a grove, than to raise one from the seed. The expence of planting is inconsiderable, and the planted trees are full as good and handsome; and many years are saved, beside the extra growth of planted trees. But this extra growth will not prove near so great in groves as in single trees. The first grove I planted from these acorns of 1719, was in 1731. In 1732 I made another grove from them; and in 1735 I planted a third grove from them; and in 1753 the last considerable number of plants were taken from the grove, and these are very good trees: so 34 years may be saved. But I would by no means advise the planting trees so large, as the trouble and expence will be too much, unless where a shelter or screen is wanted.

Whether a grove is to be raised from seeds, or planted, it is advisable to shelter it round; if from the seed, with such sorts as will grow quicker; and if by planting, with larger and taller trees. The soil in Norfolk is unfavourable to elms; therefore in planting I will venture to recommend hornbeams, as they may be planted large trees. I planted some hornbeams (rather large) in 1757, and disliking their situation, in 1792 I removed them when they were about three feet in circumference, and did not lose one tree; and they made shoots of

near half a yard that year; but I ought to say I cut off their heads.

Before I quit this subject, I will presume to recommend, if young oaks are unthriving, there is reason to hope they may be helped by cutting them down to a foot or six inches: for in 1750 I planted some oaks from my grove of 1719 into a poorer soil, and although they lived, they were sickly; so in 1761 I cut most of them down to one foot, and then by cutting off the side shoots, in three or four years led them into a single stem, and most of them are now thriving and handsome trees; and you can hardly see where they were cut off, and some are four feet round; and I have used the same method with unhealthy chesnuts, beech, hornbeam, and wych elm, and with the same success.

R. MARSHAM.

Stratton, May 29, 1796.

The aggregate Increase in Circumference of different Trees, divided into tenths of Inches of their annual Growth.

	Dates.	Feet.	Inches, 10ths of In.	Feet.	Inches, 10ths of In.	Years.	10ths of In.
S. Oak, in the Holt Forest, by the Lodge -	1759	34	0 2 +				
	1778	34	0 7 +				
S. Oak, in Stratton, planted in 1580, at 4 feet	1760	15	2 9	1	2	9	21
	1781	16	5 8				
S. Oak, planted by me, in 1720 - -	1742	2	11 2	5	3	4	39
	1781	8	2 6				
S. Oak, acorn in 1719, and transplanted 1735	1756	3	6 0	3	8	2	25
	1781	7	2 2				
S. Wych elm, in Stratton Hollow, at 4 feet	1760	29	5 6	0	4	4	20
	1780	29	10 0				
S. Wych elm, by Bradly church, Suffolk -	1754	25	5 4	0	7	2	11
	1765	26	0 6				
G. Wych elm, in Stratton - - -	1787	3	9 0	0	9	0	8
	1795	4	6 0				
S. Ash, in Benelch. yd. N. of Dunbarton, Scotland	1768	16	9 0	1	3	0	15
	1783	18	0 0				
S. Ash, in Stratton, planted after 1647 -	1742	9	10 5	3	0	7	40
	1782	12	11 2				
S. Ash, planted in 1725, in very poor land -	1769	5	5 0	1	1	1	12
	1781	6	6 1				
S. Chesnut, in Christ Church Park, by Ipswich	1747	15	8 5	1	2	7	16
	1763	16	11 2				
S. Chesnut, in Hevingham, Norfolk, planted 1610	1742	12	7 0	2	4	2	39
	1781	14	11 2				
S. Beech, in Christ Church Park, by Ipswich	1755	15	7 5	0	3	1	8
	1763	15	10 6				
S. Beech, in Stratton, seed 1741, washed and dried	1778	3	7 4	0	9	0	3
	1781	4	4 4				
G. Beech, same age - - - -	1785	3	10 5	1	3	0	10
	1795	5	1 5				
S. Plane, in Shottisham, Norfolk - -	1755	3	10 3	3	10	9	19
	1774	7	9 2				
S. Poplar, black, set in my father's time -	1756	11	5 0	0	9	4	12
	1768	12	2 4				
S. Poplar, black, in Horstead, Norfolk -	1750	6	1 0	1	3	0	4
	1754	7	4 0				
S. Poplar, white Abele - - - -	1760	0	7 0	3	8	5	21
	1781	4	3 5				

	Dates.	Fect.	Inches.	toths of In.	Fect.	Inches.	toths of In.	Years.	toths of In.
S. Willow	1756 1765	5 6	0 4	0 2	1	4	2	9	- 18
G. Alder, in sandy soil	1759 1776	2 3	0 4	4 7	1	4	3	17	- + 9 $\frac{1}{2}$
S. Asp	1772 1781	2 4	8 2	7 0	1	5	3	9	- + 19
G. Mountain ash	1759 1781	2 4	2 2	7 4	1	11	7	22	- + 10 $\frac{1}{2}$
G. Birch	1759 1768	2 3	10 6	4 2	0	7	8	9	- 8 $\frac{3}{4}$
G. Horsechestnut	1758 1779	1 3	4 0	4 2	1	7	8	21	near 9 $\frac{1}{2}$
G. Lime, in sandy soil	1777 1783	3 3	2 9	5 0	0	6	5	6	near 11
G. Cedar, one foot high in 1748	1777 1795	3 6	1 1	6 5	2	11	9	18	almost 20
G. Silver fir, planted in 1746	1758 1781	1 4	6 10	5 6	3	4	1	23	near 18
G. Scotch fir, planted in 1735	1756 1781	4 6	1 8	5 0	2	6	5	25	- 12 $\frac{1}{2}$
G. Spruce fir, planted 1735	1756 1781	3 5	4 2	4 0	1	9	1	25	near 8 $\frac{1}{2}$
S. Weymouth pine, planted in 1747	1756 1781	1 4	4 8	1 5	3	4	4	25	- + 16
G. Pinaster, planted in 1738	1756 1762	4 4	0 11	7 5	0	10	8	6	- 16
G. Larch, planted in 1749	1758 1781	1 4	5 2	5 5	2	9	3	23	near 14 $\frac{1}{2}$
S. Holly, from seed, by me, and transplanted	1749 1781	1 3	10 9	4 1	1	10	7	32	- + 7
S. Hawthorn, by Hethel church, Norfolk, at 4 ft.	1755 1781	9 9	1 8	0 5	0	7	5	26	near 3

VI. *On the periodical Changes of Brightness of two fixed Stars.*

By Edward Pigott, Esq. Communicated by Sir Henry C. Englefield, Bart. F. R. S.

Read January 12, 1797.

Bath, August, 1796.

ALTHOUGH those far distant suns, the fixed stars, have baffled all investigation with regard to our knowledge of their distance, magnitudes, and attractions; we have, nevertheless, by determining their periodical changes of light, established a strong affinity between them and our sun; and among such an inconceivable number, we may expect to find some with periods of rotation much longer and shorter than those we are already acquainted with, and with changes perhaps even sufficiently rapid to afford a ready means for determining accurately differences of terrestrial longitudes. This would be a most satisfactory, useful, and profitable discovery, and may be the lot of those who have but a slight knowledge of astronomy, provided that with great exactness, and a good memory, a constant look out be given. The discoveries which at present I have the honour of laying before the Society, are the periodical changes of brightness of two stars, one in *Sobieski's Shield*, the other in the *Northern Crown*.

The constellation of *Sobieski's Shield* consists of a very few stars, and was formed by HEVELIUS, in honour of a king of Poland; the variable star that now appears in it was, doubtless, not noticed by him, as he has set down stars near it, which are by times much less conspicuous. It has nearly the same right ascension as the star *l*, and is about one degree more

south : this, for the present, suffices to point out its place ; for as I wish to proceed immediately to the results, I shall, for greater perspicuity, collect at the end of this account, a more exact determination of its right ascension and declination, as also a plan of the stars situated near it.

When at its full and least brightness, it attains in different periods, different degrees of brightness : I have never yet seen it of a greater magnitude than of the 5th, nor when at its least, less than the 7.8th. It completes all its changes in about 63 days, being 14 \pm at its full brightness, without any perceptible change : 9 \pm at its least, also without any perceptible change ; 28 \pm days decreasing from the middle of its full brightness to the middle of its least ; and 35 \pm increasing from the middle of its least brightness to the middle of its full. These results being deduced from only the few observations I have made, cannot, of course, be very accurate, but may easily and soon be corrected by comparing any future observation with those communicated in this paper ; not relying much on the estimated magnitudes, but principally on its comparative brightness with the stars there mentioned and marked in the plan, the magnitudes of which, by a mean of several observations, I have settled thus :

Magnitudes.	}
λ 3	}
<i>i</i> 4	
<i>m</i> 4	
<i>l</i> 4.5	
<i>o</i> 4.5	
<i>k</i> 5	
<i>n</i> 5	
<i>b</i> 5.6	
<i>g</i> 5.6	
* above <i>l</i> 6	
P 6.7	
neb. 6.7	
<i>r</i> 7	
T 8	

The nine first letters are according to *F L A M I T E D*, the others as affixed by me.

Extract from my Journal of the Observations on the Variable
in *Sobieski's Shield*; made at Bath.

Dates.	Magnit.	
1795. Sept. 25	5	brighter than <i>k</i> , and less than <i>l</i> ; it has lately been increasing.
Oct. between 1 & 8	5	ditto ditto.
	26	rather less than <i>k</i> ; much brighter than P.
	30	much less than <i>k</i> , and rather brighter than P.
Novemb. 6 & 7	6	much less than <i>k</i> , and rather brighter than P.
	14	almost equal to <i>k</i> , and much brighter than P.
	27	I think rather less than <i>k</i> .
Decemb. 14	5	I could not determine which was brightest, the variable, or <i>k</i> .
1796. Feb. 12 & 13	6	considerably less than <i>k</i> , and rather brighter than P.
March 4	7	much less than P.
	12	rather brighter than P; considerably less than <i>k</i> .
April 7, 17, 19	5	considerably brighter than P, and rather less than <i>k</i> .
	30	less than P; brighter than <i>r</i> .
May 4, 10, 12, 13	7.8	{ much less than P, and rather less than <i>r</i> . The observation of the 12th seems to express most decidedly its being less than <i>r</i> .
	16	equal, or rather brighter than <i>r</i> ; much less than P.
	19	rather brighter than P.
	24	brighter than P; much less than <i>k</i> .
	31	much brighter than P; rather less than <i>k</i> .
June 4	5	not quite so bright as <i>k</i> .
	9, 10	rather brighter than <i>k</i> ; considerably less than <i>l</i> .
	14	brighter than <i>k</i> ; much less than <i>l</i> .
15, 20, 24	5	ditto, ditto, ditto.
	25	rather brighter than <i>k</i> .
	29	if any difference, brighter than <i>k</i> ; decreased.
July 7, 8	5	equal to <i>k</i> .
	16	rather less than <i>k</i> ; considerably brighter than P; D near its full.
	19	less than <i>k</i> ; much brighter than P. ditto.
	26, 27	rather less than <i>k</i> ; considerably brighter than P.
August 4, 7	5.6	less than <i>k</i> ; much brighter than P.
	12, 15	ditto ditto; moon near them.
	19, 21, 22	$\frac{1}{2}$ between the brightness of <i>k</i> and P.
	27	ditto ditto, or less bright.
	29	much less than <i>k</i> ; rather brighter than P.
Sept. 4, 5	6	considerably less than <i>k</i> ; rather brighter than P.
	7	ditto ditto; I think it rather increased.
	8	less than <i>k</i> ; brighter than P.
	16	rather less than <i>k</i> ; considerably brighter than P.

From these observations the periodical changes were deduced as follows :

The length of a single period being first settled of 67 days, from a succession of observations between March and May, and of 69 between April and June, we may proceed to obtain a greater exactness from distant dates, thus :

		Middle of its greatest brightness.	DAYS.
1795.	Oct. 1st.	}	Interval of four periods, making the length of a single one - - -
1796.	June 18		
1795.	Oct. 1	}	Interval of three periods, making the length of a single one - - -
1796.	April 10		
		Middle of its least brightness.	
1795.	Nov. 6	}	Interval of three periods, making the length of a single one - - -
1796.	May 10		
1795.	Nov. 6	}	Interval of two periods, making the length of a single one - - -
1796.	March 4		
A single period, on a mean			$62\frac{1}{4}$

Had it been requisite to have given any preference to one of these four results, I should have chosen the third; not only on account of the exactness of the observations themselves, but particularly because the changes when near its least brightness are quicker; however, they all agree more satisfactorily than I think could be expected; still it must be remembered, that the mean period here determined is merely for this set of observations, it being yet unknown what kind of irregularities it is liable to; for while I am now writing, in the month of August, its changes seem different from those of the four preceding periods; and how these perturbations will terminate,

cannot be settled in the present account, as I mean here to conclude it; but will add in the Journal, observations of as late a date as possible.

The mean right ascensions of the stars here given, were deduced from observations made in the meridian with a small transit instrument, and are, I believe, accurate. The declinations are not settled with greater precision than to two or three minutes; and although quite sufficient to prevent any mistake, I have, for the satisfaction of those who wish to make further observations on them, drawn up the annexed plan, in which all the stars they were compared to, can easily be found; no greater exactness is intended. (See Tab. II.)

	Mean right ascension.			Declination		
	In Time. h m s	in Degrees, &c.			°	'
Computed for June 25th, 1796.						
The little star T in my plan, in Sobieski's shield	18 36 16,7	279	4	10	6	7 $\frac{1}{4}$ S
The variable in Sobieski's shield - - -	18 36 38,5	279	9	37	5	56 S
Computed for June 1st, 1796.						
The little star o of my plan in the Northern crown	15 39 20,6	234	50	9	29	8 N
The variable in the Northern crown - - -	15 40 11,4	235	2	51	28	49 $\frac{1}{2}$ N

THE other Variable that I have discovered is, as already mentioned, in the *Northern Crown*. Its right ascension and declination have just been given, as likewise the plan of the stars near it. This star, although not in FLAMSTEAD's catalogue, is marked on BAYER's maps of the 6th magnitude. Several years ago, in 1783, 1784, and 1785, I suspected it to be changeable, which induced me to make the memorandums here copied in the Journal, since which time I have often seen it, but not perceiving any alteration, the dates were neglected until the spring of 1795; I then had the satisfaction of finding my suspicions confirmed, it being invisible; but on the 20th of June, it appeared of the 9.10th magnitude, and went through

its various changes as follows : in six weeks it had increased to its full brightness, the middle time of which was August 11th, 1795. At its full brightness it was of the 6.7th magnitude, and remained the same without any perceptible alteration for about three weeks : it then was three weeks and a half in decreasing to the 9.10th magnitude, and disappeared a few days after. Having reappeared in the following April, 1796, it was on the 7th of May again of the 9.10th magnitude, and increasing nearly in a similar manner as on the 20th of June the preceding year ; which completes all its changes, and gives a period of ten months and a half.

Very remarkable and perplexing it was, that just after I had made out the periods of these two variable stars, their changes should appear different from those before observed ; the particulars concerning that in *Sobieski's Shield* have been noticed : as for this in the *Northern Crown*, it shews at present (being the computed time of its full brightness), great unsteadiness, more so, I think, than any of the variables whose periods have been settled with certainty ; for having increased as before, with tolerable regularity, till it attained the 7.8th magnitude, it then kept wavering between those magnitudes, and is still so at the present time (August) that I am closing my account of it. I nevertheless hope to add a few more remarks in the Journal, as I have done for the other variable. Future observations will determine how far the period of ten months and a half is rightly settled. I am greatly inclined to think it the true one, as the star went through all its changes progressively and steadily. Many of the variables are occasionally liable to unexpected changes, particularly at the attainment of their full brightness in different periods ; such perturbed periods may

perhaps be found to return after a certain number of more regular ones; but to ascertain this, requires probably a long series of observations. The magnitude of the stars in the *Northern Crown*, marked on my plan, and to some of which the variable was compared, are here accurately fixed by a mean of many observations. (See Tab. II.)

Magnitudes.	}	
α	2.3	
β	}	
θ	4.3	
γ	}	
ϵ	4	
δ	}	All these characters are according to BAYER, except the four last, which I have added.
ι	5	
ξ	6	
π	6.7	
w	7	
o	8.9	
P	9	
x	10	

I have in this paper followed, as much as possible, the same method and deductions as in my others, which the Society have done me the honour of publishing.* The subject of them all being very similar, it was difficult to avoid sometimes repeating the same remarks, which, if omitted, might perhaps occasion some uncertainty, and perplex those who do not recollect or have not read the former papers. I shall now conclude with my observations on the variable in the *Crown*.

* See *Phil. Trans.* Vol. 75, and 76, &c.

Extracts from my Journal, of the Observations on the Variable
in the *Northern Crown*; made at Bath.

Dates.	Magnit.		
1783. July 27	7.8	seen with difficulty with an opera-glass.	} as, in these four observations, it was not compared to any star, they are less to be relied on.
30	7	much brighter.	
31	7	though the air was hazy, I could see it <i>with D^o</i> .	
August 8	7	saw it distinctly—opera-glass.	
1784. July 11	6.7	{ thought it considerably brighter than last year.	
14	6.7	{ rather less than π , but evidently brighter than w .	
1785. May 20	7	{ not so bright as ϵ , equal to π , and brighter than w .	
1795. May 28	.	{ it is marked less than π , and brighter than the 7.8th magnitude	
June 20	9.10	not visible with an opera-glass.	
23	9	evidently less than o ; rather less than P; rather brighter than x .	
29	8.9	equal to, or brighter than P.	
July 6	.	evidently brighter than P; nearly equal to o .	
7	.	.	
13	7	evidently brighter than o ; nearly equal to w .	
24	.	.	
25	6.7	certainly brighter than w , and rather less than π ϵ .	
31	.	.	
August 2	.	.	
6	.	.	
11	6.7	nearly equal to π ; no perceptible alteration during these dates.	
17	.	.	
21	.	.	
28	7.6	less than π ; moon nearly full.	
Sept. 4	7	evidently less than w ; if any difference brighter than w .	
6	8.7	evidently less than w ; if any difference brighter than o .	
13	9	less than o , and equal to P.	
15	9.10	equal to, or less than P; brighter than x .	
16	.	{ not visible with an excellent night-glass; therefore less than the	} 11th magnitude; a remarkably rapid disappearance; air clear.
20	.	.	
22	.	.	
Nov. 1	.	.	
Dec. 12	.	.	
1796. Jan. 11	.	{ not visible with an opera-glass, with which I can, when the air	} is very clear, see the star o of my plan.
Feb. 12	.	.	
March 27	.	.	
28	.	{ not visible with the night-glass; therefore not of the 11th magnit.	
April 14	10	visible with night-glass; less than x .	
17	.	.	
25	9.10	brighter than x ; rather less than P.	
May 1	.	.	
10	9	less than o , and equal to, or rather brighter than P.	
12	.	.	
19	8.9	equal to, or rather brighter than o . } near full.	

Continuation of the Observations on the variable Star in the
Northern Crown. Bath.

Dates.	Magnit.	
1796. May 24	8	rather brighter than <i>o</i> .
June 31	7.8	brighter than <i>o</i> ; less than <i>w</i> .
9		
10	7.8	{ between the 10th and 24th I often tried to see it with an opera- glass, but owing to the moon and twilight, I could not, though the <i>w</i> was by times perceptible, therefore it could not be brighter than the 7.8th magnitude.
24		
25	8	<i>rather</i> brighter than <i>o</i> ; considerably less than <i>w</i> .
29		
July 7		
8		
23	7.8	{ during these dates it has in general been set down much brighter than <i>o</i> , and rather less than <i>w</i> , though sometimes more de- cidedly less than <i>w</i> ; but these very small differences are ever difficult to ascertain, owing to the disposition of the eye, at- mosphere, and various lights.
25		
26		
27		
Aug. 30		
4		
7	7	equal to <i>w</i> ; no moonlight.
12		
15		
19		
21	7	equal to, or rather less than <i>w</i> .
22		
27	7	equal to, if not brighter than <i>w</i> .
Sept. 4		
5		
8	7	equal to, if not less than <i>w</i> .

VII. *Experiments and Observations, made with the View of ascertaining the Nature of the Gaz produced by passing Electric Discharges through Water.* By George Pearson, M. D. F. R. S.

Read February 2, 1797.

§ 1.

IN the *Journal de Physique* for the month of November, 1789, were published the very curious and interesting experiments of MESSRS. PAETS VAN TROOSTWYK and DEIMAN; which were made with the assistance of Mr. CUTHBERTSON; on the apparent decomposition of water by electric discharges.

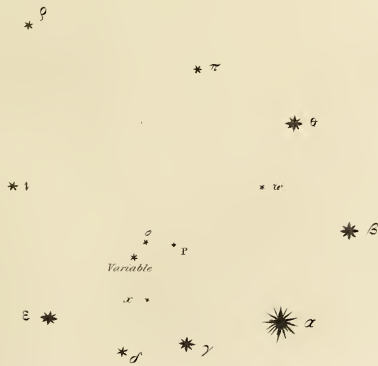
The apparatus employed was a tube 12 inches in length, and its bore was $\frac{1}{8}$ of an inch in diameter, English measure; which was hermetically sealed at one end, but before it was sealed, $1\frac{1}{2}$ inch of gold or platina wire was introduced within the tube, and fixed into the closed end by melting the glass around the extremity of the wire. Another wire of platina, or of gold with platina wire at its extremity, immersed in quicksilver, was introduced at the open end of the tube, which extended to within $\frac{5}{8}$ of an inch of the upper wire, which, as was just said, was fixed into the sealed extremity.

The tube was filled with distilled water, which had been freed from air by means of CUTHBERTSON'S last improved air pump, of the greatest rarefying power. As the open end of the tube was immersed in quicksilver, a little common air was

Sobieski's Shield & part of Antinous.



The Northern Crown.





let up into the convex part of the curved end of the tube, with the view of preventing fracture from the electrical discharge.

The wire which passed through the sealed extremity was set in contact with a brass insulated ball; and this insulated ball was placed at a little distance from the prime conductor of the electrical machine. The wire of the lower or open extremity, immersed in quicksilver, communicated by a wire or chain with the exterior coated surface of a Leyden jar, which contained about a square foot of coating; and the ball of the jar was in contact with the prime conductor.

The electrical machine consisted of two plates of 31 inches in diameter, and was similar to that of TEYLER. It had the power of causing the jar to discharge itself 25 times in 15 revolutions. When the brass ball and that of the prime conductor were in contact, no air or gaz was disengaged from the water by the electrical discharges; but on gradually increasing their distance from one another, the position was found in which gaz was disengaged; and which ascended immediately to the top of the tube. By continuing the discharges, gaz was discharged till it reached to nearly the lower extremity of the upper wire, and then a discharge occasioned the whole of the gaz to disappear, a small portion excepted, and its place was consequently supplied by water.

From my own experience I should venture to affirm, that a more particular and more accurate account than that published is requisite, to enable the student, or even the proficient, to institute the above experiment with success. Hence, during the six or seven years which have elapsed since its publication, no confirmation has been published, except the experiment repeated by Mr. CUTHBERTSON for my satisfaction, as related in

my work on the Chemical Nomenclature; although I have heard of many persons, and some of them experienced electricians and chemists, who have made the attempt. But by labouring with Mr. CUTHBERTSON, since he came to reside in London, I have learned the circumstances on which the success of the experiment depends; and I have received from him effectual aid in continuing a process, with the objects I had in view, the tediousness and even difficulties of which can only be conceived by those who have been engaged in the same pursuit.

In the course of my experiments on this subject, Mr. CUTHBERTSON invented a new method of disengaging gaz from water, by means of the electrical discharges, namely, by means of *uninterrupted or complete discharges*; whereas the method of Mr. VAN TROOSTWYK was by *interrupted discharges*. The *rationale* of the process according to these two methods, I apprehend, cannot be understood without an explanation; for I find books on electricity do not contain the necessary information.

In the experiment of Mr. VAN TROOSTWYK, it must be considered, that if in place of water the tubes be filled with air, the whole of the charge of the Leyden jar will pass, at each explosion, from the upper to the under wire, and no interruption in the discharge will happen; but if they are filled with water, then an *interrupted discharge* may be caused: by which is meant, that a part of the charge only passes at each explosion through the water from wire to wire, and with much diminished velocity. The residuary electricity in the Leyden jar is nearly one half, as may be accurately demonstrated. The reason of these differences must be assigned from the difference in point of density, elasticity, and conducting power, of the medium of

water and of air. It must be added, that although water in large quantity is a good conductor, and air is not, yet water being here in very small quantity it proves a bad conductor; as is the case with the very best conductors. A cubic foot of water is only just capable of receiving, or letting pass through it, a full discharge from a jar of one foot of coated surface; and the quantity of water employed in this experiment not being $\frac{1}{100,000}$ part of a cubic foot it is a very imperfect conductor; so that an interrupted discharge only can pass through the tube, without dispersing the whole of the water. But if the discharge be not seemingly as strong as the tube can bear without breaking, the gaz is not produced from it; and on this point hinges this extremely delicate process.

The situation of the different parts of the apparatus for the interrupted discharge is shewn by Tab. III. fig. 5.

To succeed by the method of the *complete* or *uninterrupted discharge*, the apparatus now to be described must be used, and the following rules must be observed.

1. A tube, fig. 6. is employed, about four or five inches in length, and its bore one-fifth or one-sixth of an inch in diameter. One end is mounted with a brass tube, fig. 7. and the other end is sealed at the lamp with a wire, about $\frac{1}{8}$ of an inch in thickness, fixed into it, as above described; which extends into the brass tube, so as to be almost in contact where the explosion is made. If the wire touches the brass tube, there will be no gaz produced. The tube being filled with water, and set in a cup of water, the discharge may be made into it, as in the above described process of Mr. VAN TROOSTWYK; but here the insulated ball must be placed at a greater distance from the prime conductor, and a Leyden jar with only fifty square inches of coating

will answer the purpose. In this way of making the experiment gaz is produced by each discharge, in the brass tube; and in much greater quantity, and with much less frequent accidents, and less trouble, than in the former method with the interrupted discharge. But the gaz obtained with this apparatus always contains a large proportion of atmospherical air, on account of the quantity of water and more immediate and extensive communication of it with the atmosphere. By repeated discharges there is an impression made in the brass tube, in the part where the discharge passes through it, and at last a small hole is made in that part. On this account the same mounted tube cannot serve for producing a large quantity of gaz.

2. The other sort of apparatus, invented by Mr. CUTHBERTSON, is represented by fig. 8. At first it consisted of a glass tube half an inch wide, and about five inches in length, mounted at one end with a brass funnel, and inverted in a brass dish; but afterwards the tube was blown funnel-wise at the end, as shewn by fig. 9. The other end must have a wire, about $\frac{1}{40}$ of an inch thick, sealed into it at the lamp; which wire extends to nearly the bottom of the brass dish in which the tube stands.

The exact distance between the end of the wire and brass dish must be found by trials; that which generally answered in my experiments was about $\frac{1}{20}$ of an inch. If it be properly arranged, gaz will be produced at each discharge.

The Leyden jar used with this apparatus, must contain about 150 square inches of coating.

The distance between the insulated ball and the prime conductor, at which the experiment succeeded, was commonly about half an inch.

If experiments be proposed, in which electric discharges must

be passed through water, or other fluids, for even a much longer time than was consumed in performing those referred to, or related in this paper; it may be an object to employ the wind, or perhaps the power of a horse, to turn the electrical machines; the expence of labourers being considerable.

§ 2. EXPERIMENTS.

From my journal of the numerous experiments, made during the course of nearly two years, I shall select those which will serve to explain the nature of the process, and show the power of the plate electrical machines; and I shall particularly relate those experiments which afforded the most useful results concerning the nature of the gaz obtained.

1. *With interrupted Discharges.*

Experiment A. About 1600 of these discharges, by means of a thirty-four inch single plate electrical machine, in nearly three hours, produced, from New River water taken from the cistern, and which had not been freed from air by the air pump or boiling, a column of gaz two-thirds of an inch in length and one-ninth of an inch wide. On passing through this gaz, between the two wires of the tube in which it was produced, a single electric spark, its bulk was instantly diminished to two-thirds. In other experiments the bulk of gaz was only diminished to about one half. And the result was the same with distilled water.

B. The experiment A being repeated several times, with distilled and New River water, freed from air by the air pump or long boiling, the quantity of gaz just mentioned was obtained in about four hours.

On passing an electric spark through this gaz, in the situation

above mentioned, its bulk was instantly diminished, in some cases $\frac{1}{16}$, and in others $\frac{1}{20}$.

C. 1600 interrupted discharges, by means of a thirty-two inch plate machine, produced, from New River water and distilled water freed from their air by the air pump, a column of gaz about three-fourths of an inch in length, and one-ninth of an inch in diameter, in the space of three hours. It was reduced in bulk $\frac{1}{20}$ by passing through it a single electric spark.

D. 500 revolutions of the thirty-two inch plate machine, in three quarters of an hour, produced 600 interrupted discharges in river water, freed from air by the air pump, by which a column of gaz, half an inch in length and one-tenth of an inch in diameter, was obtained. It was diminished, as usual, by an electric spark, $\frac{1}{20}$ of its bulk.

E. Nearly four days incessant labour, with the thirty-two inch plate machine, produced only 56,5488 cubes of gaz, of one-tenth of an inch each; on account of the usual accidents during the process. The air had been exhausted, by setting the water under the receiver of the air pump.

F. It was found that 6000 interrupted discharges produced about three inches in length of gaz, measured in a tube $\frac{3}{20}$ of an inch in width, from water out of which its air had been drawn by the air pump.

G. It appeared, from many experiments, that the same un-boiled water, or water from which the air had not been exhausted by the air pump, which had repeatedly yielded gaz by passing through it electric discharges, always left a residue of gaz, which the electric spark did not diminish; and this residue was in nearly the same quantity, after six or seven experiments, each of which afforded a column of gaz, half an inch in length,

and one-ninth of an inch in diameter, as was left on passing the electric spark through the gaz, afforded by the third or fourth experiment.

Hence it seems, that water is decomposed by the electric discharge, before the whole of the common or atmospherical air is detached from the water, by merely the impulse of each discharge. Yet I think it probable that, after the discharges have been passed through the same water for a certain time, the whole of the air contained in water will be expelled, and no gaz be produced, but that compounded by means of the electric fire from water; in which case, supposing the gaz so produced to be at last merely hydrogen and oxygen gaz, it will totally disappear on passing through it an electric spark. But I have never been able to determine this point; because the tubes were always broken after obtaining a few products, or long before it could reasonably be supposed the whole of the air of the water was expelled from it.

H. To the gaz obtained in the experiment E was added, over water, an equal bulk of almost pure nitrous gaz. Fumes of nitrous acid appeared, and the gaz examined was reduced almost one-third of its bulk. A small bubble more of nitrous gaz being let up no further diminution took place. To this residue was added half its bulk of oxygen gaz, obtained from oxy muriate of potash. This mixture of gazes having stood several days over well burnt lime and boiled quicksilver, an electric spark was passed through the mixture, over quicksilver; by which its bulk was instantly diminished one-fourth. But no moisture could be perceived upon the sides of the tube, or on the quicksilver. The failure of the appearance of moisture was imputed to a bit of lime accidentally left in the tube,

which was burst by the explosion and dispersed through the tube; or else the quantity of water produced was so small, comparatively with the residuary gaz, that the water was dissolved by it in the moment of its composition. For supposing water to have been compounded, it could not amount to the $\frac{1}{100}$ part of a grain; and the residuary gaz was at least two thousand times this bulk.

That a quantity of water can be compounded, under the same circumstances as in this experiment, and be apparently dissolved in air, so as to escape observation, even with a lens, was proved by passing an electric spark through a mixture of hydrogen and oxygen gaz, well dried by standing over lime.

2. With complete or uninterrupted Discharges.

The gaz obtained by the first described kind of apparatus, for the uninterrupted discharges, p. 145, and fig. 6 and 7, always left a residue of at least one-fourth of its bulk on passing through it the electric spark; even when water was used, which had been freed from air by boiling, or the air pump. Nor will this result appear surprising, when it is considered how liable the water in this apparatus is to mix and absorb air during the experiment. However, this method would have been extremely valuable if the next other method had not been discovered; for gaz may be obtained by it with fewer accidents, and much more rapidly, than with the interrupted discharges. The apparatus is also much more easily fitted up, and is more simple. But I think it unnecessary to particularly relate any experiments, as they afforded the same results as those already described, and as those next to be related.

The following experiments were made with the apparatus described p. 146, and shown by fig. 8, 9, and 10.

Experiment 1. At 0^h 40' P. M. began to produce discharges with a double plate twenty-four inch machine, in water taken from the cistern: and at 12^h 6' P. M. of the same day there had been written down 10200 discharges, each of which occasioned air to ascend from the bottom of the wire and brass cup. The quantity of air obtained was now apparently about one-fourth of a cubical inch, and it occupied nearly half of the tube; the water in which was by this time very muddy.

After standing till the day following at noon, when the process was again commenced, it did not appear that any of the gaz had been absorbed by the water over which it stood.

At 2^h 35' P. M. began to produce discharges, and at 8^h P. M. had passed 6636; which, together with those of the preceding day, amounted to 16836. The tube was now $\frac{5}{8}$ full of gaz, and there seemed to be almost half a cubical inch; for it was observed, that the gaz was this day yielded at double the rate it had been the day before. This was accounted for from the diminished pressure upon the electric fire, by the tube containing gaz instead of water.

At this time, namely, at 8^h P. M. I was surprised, on the passing of a discharge, by a vivid illumination of the whole tube, and a violent commotion within it; with, at the same time, the rushing up of water, instantly to occupy rather more than $\frac{5}{8}$ of the space which had been occupied by gaz.

The residue of gaz was not diminished further by an electric spark; and to the test of nitrous gaz it appeared to be rather worse than atmospherical air, as it consisted of rather less than one part of oxygen, and three parts of nitrogen or azotic gaz.

It seemed as if the electrical discharge had kindled the oxygen and hydrogen gaz of the decomposed water, by flying from the bottom of the wire to the brass funnel; so that the fire returned into the tube where it passed through the gaz. Or the combustion might be occasioned by a chain of bubbles, reaching from the brass dish to the surface of the water in the tube, which was set on fire in its ascent, and thus produced combustion of the whole of the gaz of decomposed water.

That this phænomenon was from the combustion here supposed, was in some degree proved by finding that the mixture of hydrogen gaz and atmospherical air, under the same circumstances, was kindled in the same manner.

Experiment 11. With a double plate electrical machine, 24 inches in diameter, and a similar apparatus to that in the last experiment, 14600 discharges produced, at least, one-third of a cubical inch of gaz. While I was measuring with a pair of compasses the quantity of gaz produced, the points of them being in contact with the part of the tube occupied by gaz, I was again surprised, on the passing of a discharge, by an illumination of the whole tube, and the rushing up, with considerable commotion, of water, to occupy about two-thirds of the space filled by gaz.

The residuary air was found, as in the former experiment, to be rather worse than atmospherical air.

It was concluded that the points of the compasses had attracted electrical fire from the wire to the sides of the glass, and thereby kindled the hydrogen and oxygen gaz of decomposed water. But to determine this question, I introduced into the same tube a mixture of one measure of oxygen and two measures of hydrogen gaz, to occupy nearly the same

space in the tube as the gaz had occupied: then passing an electrical discharge through it no combustion was excited; but on passing a discharge while the compasses were in contact with the tube, as just mentioned, an illumination and violent commotion were produced, with the rushing up of water, to leave only $\frac{1}{6}$ of the gaz as a residue. On repeating this experiment with one measure of atmospherical air and two of hydrogen gaz, combustion could not be excited; nor with two measures of atmospherical air and one of hydrogen; nor with two measures of hydrogen gaz and one of atmospherical air; but on adding to this last mixture one measure of oxygen gaz, the electrical discharge produced the phænomena of combustion just mentioned, with the rushing up of water, to occupy about two-thirds of the space which was occupied by the gazes.

Experiment III. Having passed 12000 discharges through water, with the apparatus of the preceding experiment, and thereby obtained only one-fifth of a cubical inch of gaz; and having observed, that the quantity of gaz was not greater than it was when only 8000 discharges had been passed, and yet bubbles had been seen to be produced on each discharge as copiously, or more so, by the last 3 or 4000 discharges as before; I began to suspect that part of the gaz had been destroyed during the process, or had been absorbed. While I was considering how to account for this disappearance of gaz, and was at the same time looking at the tube through which the discharges were passing, I observed one of them to be attended with a diminution, instantly, of about one-fifth of the gaz produced, and with a slight commotion. I was now sure, from this phænomenon, and from the unequal augmentation of the bulk of the gaz at given

times during the process, that combustion had been excited several times before; not only in the present experiment, but perhaps in the former ones, without observing it. I conceived that a gradual combustion also, very probably, took place in this process, by the kindling of bubbles of gaz in their ascent through the water. I now perceived that the discharges ought to be produced more slowly, or the tubes to be wider, to allow the bubbles to pass quite through the water, in order to avoid the accension of gaz during the process. My calculation also, that 35 to 40000 discharges were requisite to produce one cubical inch of gaz from water, containing its usual quantity of common air, was rendered much more vague by this accension, so often liable to be occasioned.

To the gaz which remained in the tube in this experiment was added an equal bulk of nitrous gaz; the mixture diminished to 1,5; and on adding to the residue half its bulk of oxygen gaz, and passing through it the electrical spark, no accension or diminution of bulk was produced. Hence all the hydrogen gaz and oxygen gaz, produced by the decomposition of the water, had been burnt during the process; the oxygen gaz thus detected being considered to be only that expelled from the water.

Experiment iv. By means of electrical discharges, with the apparatus used in the preceding experiment, I obtained gaz from New River water; letting it up into a reservoir as soon as about $\frac{1}{20}$ of a cubic inch was produced, till I had collected $\frac{1}{8}$ of a cubic inch. To this was added an equal bulk of nitrous gaz; on which the mixture diminished to 1,2; and on the addition of a little more nitrous gaz, no further diminution took place. To this residue half its bulk of oxygen

gaz was added, and this mixture of gazes being well dried by standing over lime and boiled quicksilver, an electric spark was passed through it, by which a diminution of $\frac{1}{6}$ of its bulk took place. A little dew was then seen upon the sides of the tube where the quicksilver had risen; and, with the aid of a lens, the same appearance was perceived on the part of the tube containing the residue of gaz.

It may now be expected, that I should have made the experiments with this apparatus on distilled water freed from its air, not only by long boiling, or the air pump, but by passing through it several hundred electrical discharges. It would also have been, to some persons, more satisfactory, if the experiments had been made upon a larger scale, so as to have produced the combustion of a much larger quantity of gaz, and consequently have produced a greater quantity of water. As, however, I apprehend, the experiments contained in this paper, when well considered, by competent judges, will be found to explain the nature of the gaz procured from water by electrical discharges; and as another very important subject demands my attention, the honour of more splendid and convincing experiments must be reserved for other inquirers. If the same sacrifices be made by them, which have been made in performing the present experiments, I think it is scarcely possible but that still further light concerning the composition of water should be obtained, as well as concerning oils, alcohol, acids, &c.; to the investigation of the composition of which, the mode of analysis and synthesis here indicated, may be applied.

§ 3.

The following conclusions appear to me obvious and incontrovertible.

The mere concussion by the electric discharges seems to extricate not only the air dissolved in water, which can be separated from it by boiling and the air pump, but also that which remains in water, notwithstanding these means of extricating it have been employed.

The quantity of this air varies in the same and in different waters, according to circumstances. New River water from the cistern yielded one-fifth of its bulk of air, when placed under the receiver of Mr. CUTHBERTSON'S most powerful air pump; but, in the same situation, New River water taken from a tub exposed to the atmosphere for a long time yielded its own bulk of air. Hence the gaz produced by the first one, two, or even three hundred explosions in water, containing its natural quantity of air, is diminished very little by an electrical spark.

The gaz or air, thus separable from water, like atmospherical air, consists of oxygen and nitrogen or azotic gaz; which may be in exactly the same proportions as in atmospherical air, for the water may retain one kind of gaz more tenaciously than the other; and on this account the air separated may be better or worse than atmospherical air, in different periods of the process for extricating it.

The nature of the gaz, which instantly disappears on passing through it an electric spark, is shown by

(a) This very property of thus diminishing; and by the following properties;

(b) A certain quantity of nitrous gaz instantly disappeared, apparently composing nitrous acid, on being added to the gaz (a) p. 149, H. 154, Exp. iv.; oxygen gaz being added to the residue after saturation with nitrous gaz, and an electric spark being applied to the mixture of gazes, well dried, a considerable diminution immediately took place, and water was produced; p. 154, Exp. iv.

(c) Combustion from hydrogen and oxygen gaz took place, when the tube was about three fourths full of gaz, p. 152, Exp. i. which was confirmed by passing an electrical discharge, under the same circumstances, through a mixture of hydrogen and oxygen gaz, p. 152, Exp. i.

(d) Combustion from hydrogen and oxygen gaz took place, when the points of the compasses were accidentally applied to the part of the tube containing gaz, p. 152, Exp. ii.; which was confirmed by passing a discharge, under the same circumstances, through a mixture of hydrogen and oxygen gaz, while the points of the compasses were applied to the tube; p. 153, Exp. ii.

(e) The observations made of the kindling of gaz in small quantities, from time to time, during the process of obtaining it, particularly while it was ascending in chains of bubbles, or was adhering to the funnel of the tube, p. 153, 154, Exp. iii. confirm the evidence in favour of this gaz being hydrogen and oxygen gaz.

The evidence contained under the heads (a)—(e), considered singly and conjunctively, I apprehend, must be admitted

by the most rigorous reasoner, to be demonstrative that hydrogen and oxygen gas were produced by passing electric discharges through water.

With regard to the origin and mode of production of these two gazes, our present observations and experiments do not afford complete demonstrative evidence; but, although some hypotheses must be admitted, I conceive that the body of evidence we possess can afford a satisfactory interpretation of the phænomena.

EXPLANATION OF THE PLATE (Tab. III.)

Fig. 1, 2, 3, 4. represent the tubes used in producing gaz from water by the interrupted electric discharges.

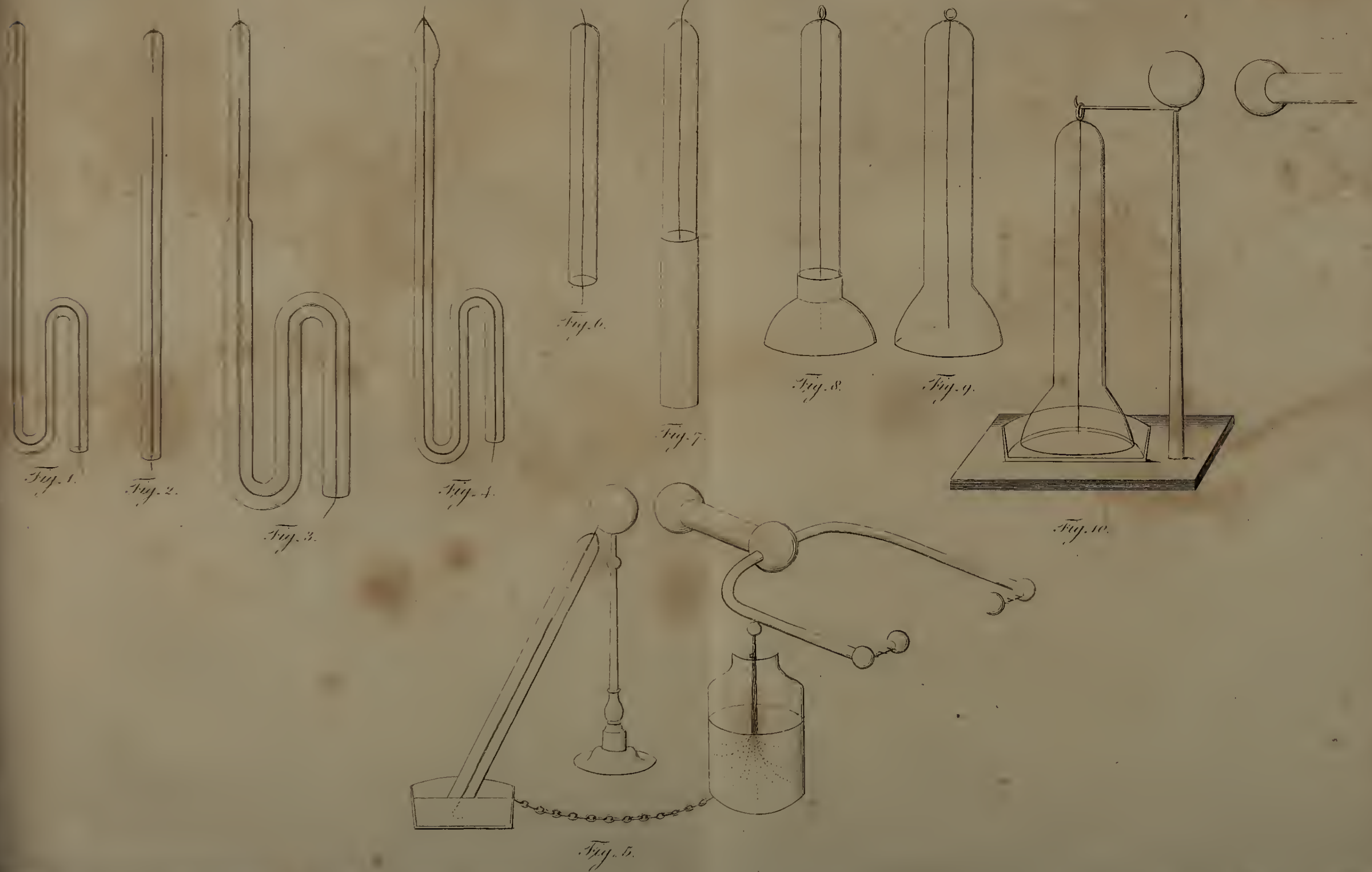
Fig. 5. represents the situation of the above tubes during the process of producing gaz from water.

Fig. 6, 7. represent the tubes employed in producing gaz from water by the first method, with uninterrupted electric discharges.

Fig. 8. shows the figure of the tube mounted with a brass funnel, used in the second method of producing gaz from water by uninterrupted electric discharges.

Fig. 9. represents the tube blown funnel-wise at the end, instead of being mounted with a brass funnel, as in fig. 8.

Fig. 8. represents the situation of the tubes fig. 8. and 9. during the process of producing gaz by the uninterrupted electric discharges.





VIII. *An Experimental Inquiry concerning Animal Impregnation.* By John Haighton, M. D. Communicated by Maxwell Garthshore, M. D. F. R. S.

Read February 2, 1797.

DIFFICILLIMUM aggredior laborem, et exitum vix promitto qui lectori satisfaciat.

This was the sentiment of the justly celebrated Baron HALLER, when he first directed his attention to this subject, when he attempted to produce order and regularity out of chaos, and to show

“ How the dim speck of entity, began

“ T’ extend its recent form, and stretch to man.”

GARTH.

The difficulties which discouraged so able a philosopher, are but ill calculated to inspire me with confidence; but the disappointment from failure will be attended with this solacing reflection, that if I have miscarried, it is in a great undertaking.

The multitude of physiologists who have sought for laurels in this field, can best bear witness to the difficulty of the pursuit; and the penetrating genius of a HARVEY, though adequate to a full exposition of the circulation of the blood, toils in vain in the mysterious researches of generation. His philosophic

mode of scrutiny by experiment, when pointed to one object, conferred immortality on his name; but when directed to another, reduced him to a level with contemporary reputation.

Others, perhaps from possessing a greater propensity to the subject, have laboured with more success: they have penetrated into the interior recesses of nature, and thence brought to view what preceding investigators had deemed inaccessible to research. On this view of the subject, our acknowledgments are particularly due to the labours of STENO, DE GRAAF, HALLER, and others. To STENO and DE GRAAF we are indebted for some important facts on the structure of the ovaries. The supposed analogy to the male's testes is disproved, and the vesicular structure, together with a connexion with the ova, or rudiments of the new formed animal, fully established.

From the experiments of DE GRAAF on rabbits, we learn,

First. That the ovaries are the seat of conception.

Secondly. That one or more of their vesicles become changed.

Thirdly. That the alteration consists in an enlargement of them, together with a loss of transparency in their contained fluid, and a change of it to an opaque and reddish hue.

Fourthly. That the number of vesicles thus altered, corresponds with the number of fœtuses, and from these are formed the true ova.

Fifthly. That these changed vesicles, at a certain period after they have received the stimulus of the male, discharge a substance, which being laid hold of by the fimbriated extremity of the fallopian tube, and conveyed into the uterus, soon assumes a visible vesicular form, and is called an *ovum*.

Sixthly. That these rudiments of the new animal, which for a time manifested no arrangement of parts, afterwards begin to

elaborate and evolve the different organs of which the new animal is composed.

To these facts we may add, that the *calyx* or *capsula* which formed the parietes of the vesicles, thickens, by which the cavity is diminished. This cavity, together with the opening through which the fœtal rudiments escaped becomes obliterated, and from the parietes of these vesicles having acquired a yellowish hue, they are called *corpora lutea*.

But though some important facts are clearly ascertained, there are others still problematical. Physiologists are by no means agreed concerning the *immediate cause* of conception. All admit the necessity of sexual intercourse. They acknowledge too the necessity of some part of the female being affected by the direct contact of a fecundating fluid, but what the precise part is which must receive the stimulus, has hitherto been involved in mystery and doubt. Nor are they more unanimous respecting the state or condition of the substance that passes from the ovaries; whether at the time of its expulsion it has a circumscribed vesicular character, or whether it has no determined figure. DE GRAAF and MALPIGHI, in the last century, and some respectable physiologists of the present day, adopt the first opinion; HALLER and some others favour the last.

The subject of conception involves other problematical points not less interesting; the discussion of which I purpose waving at present, in order the better to direct my attention more closely to the preceding questions.

The intention then of this essay is to explore the *proximate cause* of the impregnation of animals, and to trace with more accuracy the visible effects of it from their first appearance, until the rudiments of the fœtus are lodged in the uterus, and

have assumed the proper characters of an *ovum*. As soon as these rudiments manifest that opaque spot, or "dim speck of entity," which is known to evolve the fœtus by regular and progressive steps; another stage of the inquiry then commences, viz. to trace the visible formation of the new animal through its whole course; but as this belongs rather to the œconomy of the fœtus than the mother, it is not intended to form any part of this paper.

I perceive, however, that I cannot investigate the question of the proximate cause of impregnation in a satisfactory way without first determining what are the evidences or proofs that impregnation has taken place: this then necessarily becomes a preliminary question. I therefore restrict my inquiry to the three following subjects.

First. What are the *evidences* of impregnation?

Second. What is the *proximate cause* of impregnation?

And, *third.* Under what form do the rudiments of the fœtus pass from the ovary to the uterus?

SECTION I.

What are the Evidences of Impregnation?

The investigation of every complicated subject of inquiry comprehends within its range a more or less extended recital of facts, depending in a greater or less degree on each other, but primarily arising from some fundamental proposition.

As this proposition is generally the basis on which this superstructure is raised, or the trunk from which the various ramifications of inquiry proceed, it is essential, to the establishment of the ultimate conclusions, that the antecedent question

be rightly decided. It becomes then indisputably necessary to us in the present subject, to determine *what is the criterion of impregnation.*

That a female is impregnated when a fœtus is sensibly formed, is so obvious to reason that no argument can be necessary to convince us of its truth. But it is important to some conclusions in the sequel of this paper to prove, that a female has conceived before there are any vestiges of a new animal. The test of this condition must then be sought for in the ovaries; and the well conducted experiments of DE GRAAF, in the last century, and of Baron HALLER and others, in the present, bear so forcibly on this point, that the necessity of further investigation is in a great measure precluded.

But, in order that I might bear evidence of its truth, I examined with great attention the ovaries of some full grown virgin rabbits, and found, as DE GRAAF has represented, that there entered into their composition a series of cells containing a transparent colourless fluid. It was indispensably necessary here to be certain, that these rabbits had never been admitted to the male, lest the remains of former impregnations should be confounded with virgin appearances. I therefore observed with care not only the appearance on the surface of these bodies, but likewise examined with great minuteness the interior parts; yet in none of them could I see any of those circumscribed substances, which, from their yellow colour, are called *corpora lutea*. But when similar observations were made on rabbits that had been impregnated at different periods, and the traces of those corpora lutea were more or less evident, according to the interval of time that had elapsed; I may then say that no corpora lutea exist in virgin animals, and that when-

ever they are found, they furnish incontestible proof, that impregnation either does exist, or has preceded.

But a proper distinction between past and existing impregnation can be made only by tracing the phænomena of recent fecundation progressively, and noting the appearances in the different stages. I was therefore under the necessity of repeating with care several of DE GRAAF'S experiments, in order that I might bear testimony to the truth of them, at least as far as the results coincided with my own.

EXPERIMENT.

Having therefore procured several virgin rabbits in a fit state for impregnation, I admitted one of them to the male. Twelve hours afterwards it was killed, and on examining the ovaries several of the vesicles evidently projected; they had lost their transparency, and were become opaque and red. When punctured, a fluid of the same colour escaped. I made sections through some of them; but at this early period the corpora lutea, which are formed by the thickening of the parietes of the vesicles, were not very evident. I therefore determined to examine them in a more advanced state.

EXPERIMENT.

Another rabbit being admitted to the male, I examined it twenty-four hours afterwards. The colour of the fluid contained in the vesicles was similar to that of the last experiment. The vesicles projected more evidently, and their thickened parietes manifesting the commencement of corpora lutea were become more apparent.

EXPERIMENT.

I inspected the ovaries of another rabbit forty-eight hours *post coitum*. At this period the vesicles seemed to be in the very act of bursting, and a semitransparent substance, of a mucus-like consistence, was beginning to protrude from some of them; others indeed were less advanced. The fimbriated extremities of the fallopian tubes were preparing to receive their contents, as appeared by having quitted their usual position, and embraced the ovaries in such a degree, that only a small portion could be seen until the tubes were taken away. Sections being made into the thickened vesicles, the formation of corpora lutea appeared to have made further advances.

From the appearance of an incipient rupture of the vesicles in this experiment, it was but reasonable to expect that their contents would soon have escaped; but as my views were directed to the formation of a corpus luteum, I deferred the next examination to a more distant time.

EXPERIMENT.

In two days and twelve hours after coition, I examined the ovaries of another rabbit. The foetal rudiments had escaped; but the cavity of the ovarian vesicles had suffered but little diminution. Bristles were easily introduced by the ruptured orifices. In this experiment the advances towards the formation of a perfect corpus luteum were such as the period of examination would naturally lead us to expect.

The contents of the vesicles having escaped, it was but reasonable now to look forward to a speedy obliteration of the

cavity. I therefore examined these parts under similar circumstances on the third, fourth, and fifth day. In the last experiment there was but little vestige of cavity, consequently the corpora lutea might be considered as perfectly formed.

I think it not improper to remark here, that though in the relation of the above experiments I have constantly kept in view the formation of corpora lutea; yet I did not altogether neglect the opportunity of making other observations, which in this early stage of the inquiry it would be premature to relate. Besides which, several other rabbits were examined at more distant periods, as well with a view of tracing their progress with accuracy, as to afford further evidence of their connexion with impregnation. But as it would be tedious to state in detail the several experiments made on this single question, by reason of the great similarity of result, I decline trespassing on your patience, and therefore lay before you only the conclusion; which is, that in the great variety of experiments on brute animals which my physiological inquiries have led me to conduct, as well as in the extensive opportunities I have had of observing the ovaries in the human subject, I have never seen a recently formed corpus luteum unattended with some circumstance or other connecting it very evidently with impregnation. I have more than once seen a recently formed corpus luteum in the human subject, *without* a foetus. Nay, even in a subject where there has been a kind of *hymen*: but the uterus in these cases has borne the marks of an early and recent abortion.

SECTION II.

What is the proximate Cause of Impregnation ?

The preliminary question concerning the criterion of fecundation being now answered, we are led by a natural transition to show by what means this test has been produced.

Waving all comment on the peculiar circumstances of sexual intercourse, as being both irrelevant and indelicate, we shall note only one important effect of it, the passage of the fecundating fluid of the male into the generative organs of the female, as being an indispensable requisite in the human female, and in such animals as bear an affinity to it. As this effect of sexual communication is so important, it cannot be indifferent to the design of nature, to what part of the uterine system the semen should be conveyed. It admits of no doubt that it either remains in the vagina, passes into the uterus, or else extends its course along the fallopian tubes to be applied to the surface of the ovaries, which it stimulates, and from which the new animal derives its existence; but whether it be one or other of these, has given birth to more physiological controversy, than perhaps any other operation of a living animal.

Those who have entered the lists have ranged themselves either on the side of application of the semen to the ovaries by means of the tubes; or on that of the inutility of this process. These latter contend for an absorption of this fluid by the vagina, a peculiar excitement of the whole frame as a consequence, of which excitement the changes produced on the ovaries are to be considered the local effects. But though the question has been disputed on both sides with all the zeal of argument

and controversy, the arbiters of science have not yet acknowledged a victor on either side.

The advocates for the first opinion allege, that the semen has been seen both in the uterus and tubes, and quote as their authority the observations of MORGAGNI for the former, and RUYSCH for the latter. When seen in this last situation, some have thought that it was conveyed thither by the muscular power of these parts in the manner of a peristaltic motion, beginning at the uterus and ending at the fimbriated termination of the tube; and when at this last, it was supposed that the semen was applied to the surface of the ovaries, and impregnated them by actual contact.

Though I shall prove that this hypothesis is altogether visionary, yet *prima facie* it is far from carrying with it the characters of absurdity. There is nothing repugnant to reason in contending for what analogy seems to favour, particularly when the subject is thought beyond the reach of demonstration or proof. And the analogy favourable to this opinion has probably been taken from the impregnation of frogs and toads, in which process we are told, on the authority of ROESEL, SWAMMERDAM, and SPALLANZANI, the ova are impregnated by the male as they are passing from the body of the female; and that in water newts the ova are impregnated even without copulation. Now here is an appearance of contact between the fecundating fluid and the ova.

Again, on the other hand, the contact of semen with the ovaries has been thought improbable, from an analogy drawn from the vegetable kingdom; for admitting the Linnæan doctrine to be true, which contends for a necessity of sexual intercourse in vegetables, it would be difficult to demonstrate to

the satisfaction of stern philosophers, that the *pollen* pervades the *pistillum*, and stimulates the contents of the *pericarpium* by contact, to the evolution of the *germen*. Such would deny the contact of semen. The advocates for either opinion then may avail themselves of analogies suited to their own mode of thinking. It may be said, however, and with some colour of truth, that the latter analogy, as being more remote than the former, and as being founded on a principle which some have suspected to be gratuitous, should be received with caution and distrust. Before any deduction can be made from analogy concerning the means by which any important end is to be effected, we cannot examine the instruments performing such actions with an attention too nice or too minute. If we find nature employing different instruments, in different animals, to produce the same ultimate effect, I think it but fair to conclude, that the means used are essentially different; but the closer the resemblance in the instruments or organs, the nearer will the means approach. On this principle no conclusions can be drawn respecting the human species, from observations either on vegetables, or even on frogs, toads, and newts. Birds, as being impregnated by semen conveyed into the body, resemble human impregnation more than the former; but they differ so obviously in the mode of perfecting the *fœtus* from the *ovum*, that I scarcely dare to rest any thing on their general analogy. There is, however, a curious fact respecting them not altogether inapposite to this question, which is, the permanent effect of one *coitus*. I have read in the Abbé SPALLANZANI'S dissertation, and elsewhere, that all the eggs which a hen will lay in twenty days will be impregnated at one *coitus*: and Mr. CLINE tells me, that in Norfolk this matter is reduced to a certainty

with respect to turkeys; and that even to a greater extent. There is certainly some difficulty in reconciling these facts to impregnation by contact of semen; but from the very obvious difference between oviparous and viviparous animals, I shall not press this argument farther. Indeed it should always be impressed on the recollection of those who are labouring in the pursuit of truth, that arguments drawn from analogies, unless from those of the nearest relation, are better adapted to the purpose of illustration than of proof: and though they frequently find advocates in confident closet philosophers, they are received with deserved distrust by the more cautious practical physiologists.

Those who cannot admit the passage of semen by the tubes, do not neglect to take the advantage of some difficulties which their opponents have overlooked. They say, implicit confidence is not due to the observations of MORGAGNI and RUYSCH, and that what appeared to them to be semen in the uterus and tubes, was nothing more than the mucus of the parts. They further invalidate the force of this argument by contrasting these solitary observations, with a numerous train of counterfacts; for in all the experiments made by HARVEY, DE GRAAF, HALLER, and others, it does not appear that semen was found beyond the vagina, except in one of BARON HALLER'S experiments in a sheep, in which he saw semen in the uterus forty-five minutes after coition. But this fact stands almost alone; and when placed in opposition to the many experiments attended with a contrary result, will weigh but little in the balance of impartial decision. Yet, however, he rested much upon this one fact, and adduced it in support of his opinion, that whenever impregnation happened, the semen passed into the uterus,

and was retained; but when it returned from the vagina, then the animal remained unimpregnated. In this latter case, he supposes the semen had never passed beyond the vagina; for if it had, he says it would have been retained. This argument he thinks is unanswerable.

The insufficiency of this reasoning did not escape the penetration of his opponents; and the immense mass of counter-facts poured out against him, like an irresistible torrent, bore away the very foundation of his doctrine. This brings the advocates for the necessity of the contact of semen with the ovaries into a dilemma, from which they attempt to extricate themselves by contending, that fecundation does not require the application of semen to the ovaries in a palpable form; but that there is exhaled from it a subtile fluid in a vaporific state, called *aura seminalis*, and that the contact of this vapour is fully sufficient to impart to the ovaries their due quantity of stimulus.

But the opinion, even thus qualified, has not passed without animadversion. There are some who cannot comprehend how the tubes should perform two motions in contrary directions, which they must do, if they first convey the *aura seminalis* to the surface of the ovaries, and afterwards return the rudiments of the foetus into the uterus. Such a double action they think is repugnant to the œconomy of the part, but assign no reason for their opinion. They might with equal propriety deny the possibility of a peristaltic and inverted peristaltic motion of the intestines, or the opposite actions in the œsophagus of ruminant animals, though I am persuaded very few would acquiesce in their incredulity: but as a minute discussion of this particular

question would be rather extraneous to my investigation, I must decline any further disquisition.

The difficulties which were opposed to the conveyance of the semen by the tubes, were, as we should suspect, intended to prepare the way for a different explanation; therefore physiologists, by a very natural transition of thought, were led to suppose that the presence of semen in the vagina alone was sufficient to account for impregnation.

In order to give support to this opinion, cases were adduced, in which, from some anatomical peculiarities, it seemed almost impossible that the fecundating fluid could be conveyed into the uterus; and yet in several of these cases impregnation had really taken place. It would be digressing too much to state the facts in detail, seeing that in this inquiry I deduce nothing from them; nor would such statement solve the problem before us. The facts are already in the possession of physiologists, but are not admitted as satisfactory proofs. Those who hold the contrary opinion, either cavil at the accuracy of the statement, or draw a different conclusion; therefore to attempt conviction by these materials would be to engage in the service of forlorn hope. It remains then to try whether by a patient experimental investigation, we can make such an accession of new facts to our present stock of knowledge as will enable us to unloose this Gordian knot. This attempt naturally leads us to review the two points of the question, viz. *Is the passage of the semen by the tubes to the ovaries, essential to impregnation? If not, what other means are employed?*

If it be true that the fecundating fluid must pass by the tubes to the ovaries before impregnation can take place, ought it not

to follow, as a consequence, that if, from any cause, both these tubes be obliterated, the animal so affected would be barren? or if the animal be multiparous, would not an obliteration on one side prevent conception in the corresponding ovary?

Now I had some distant apprehensions, even before I made this experiment, that dividing both tubes would produce effects equivalent to an extirpation of both ovaries, which experience has since proved to be well founded; for it not only destroys the power of conception, but even the disposition for using the means.

EXPERIMENT.

Having procured a full grown virgin rabbit, which had betrayed signs of disposition for the male, I made an incision into the posterior part of each flank, exactly upon the part where the tubes are situated. By means of my finger and a bent probe, I drew out a very small portion of the middle of the tube, and cut out about $\frac{1}{8}$ of an inch. The two ends were returned into their former situation, and the wound closed by what surgeons call the quill suture. The same operation was performed on the opposite side, and in a few days both wounds were healed.

As soon as this rabbit appeared in health, it was admitted to the male, but the venereal appetite seemed to be entirely lost. Thinking it possible that its health was not perfectly restored, I kept it a month longer in a state of high feeding, and admitted it to the male a second time, but the same reluctance continued. I began now to suspect that the venereal appetite was irrecoverably gone: but as the season was cold, and of course unfavourable, it appeared proper to persevere in this plan until the

genial influence of returning spring had produced its effect; but instead of discovering signs of the restoration of the female character, it was evidently more averse. It was now killed and examined, the tubes adhered firmly to the loins at the part where they were divided, and at that part their canal was obliterated, so that neither quicksilver nor air could be made to pass. The ovaries were much smaller than they usually are in breeding rabbits; they appeared to have degenerated from their proper character, a circumstance probably the consequence of that destruction of the harmony of action in these parts, which subsists in the healthy state, which is essential to the views and intentions of nature, and for want of which harmony, the sexual indifference, approaching to aversion, was in this instance so remarkable.

In the relation of this experiment, it must be remembered, that a small portion of each tube was cut out, in order to obliterate the canal with greater certainty. It is not altogether indifferent to the present subject to know, whether this apathy depended on the removal of that portion, or whether it would have happened had there been nothing more than a mere division. Nor is it extraneous to inquire, whether a simple division of the tube is sufficient to obliterate it, because less violence is offered to the part, and of course the connection will be less disturbed.

EXPERIMENT.

Being furnished with another rabbit, in high breeding condition, I repeated the experiment, by making only a division of the tubes; in other respects every thing was conducted as before. The venereal appetite declined as evidently in this as

in the former, and notwithstanding many solicitations from a very animated male, during the space of three months, it could never be excited.

On dissection, it appeared that the tubes were as completely obliterated in this experiment as in the last, and the ovaries had equally degenerated.

In the two preceding experiments neither of the rabbits had given any active proofs of fecundity, though they had marks of the venereal heat upon them. I therefore changed my subject for one that had had young ones.

EXPERIMENT.

A healthy rabbit, which had lately been separated from her first litter, was made the subject of a repetition of the experiment. I took the opportunity of feeling for the ovaries, in order to have better evidence respecting their bulk, and by that means to form a juster comparison. The disposition to propagation declined as evidently in this animal as in the two former; and dissection equally evinced a change of the ovaries; for at the expiration of three months, they had lost nearly half their size.

Feeling but little encouragement to persevere in a repetition of these experiments, I determined to change the mode of inquiry, and to try the effect of a division of *one* tube only. From reasoning I was led to think, that if a division of both tubes destroyed the harmony of the generative system, a division of one only might permit that harmony to continue in some degree. I wished likewise, if possible, to have this point determined on a virgin rabbit, the better to guard against any

deception which the remains of a former impregnation might occasion.

EXPERIMENT.

A full grown virgin rabbit had one of the tubes divided at a little distance from the extremity of the cornu uteri. The wound soon healed up, and its health was soon restored, but it betrayed no disposition for the male. I attributed it in part to the coldness of the season, for it was in the middle of December, 1794; but the effects of its inclemency were much moderated by having a fire in the room during the day. I kept her until the first of May; during this interval the male was frequently offered to her, but she always refused, except once in February: it however was unproductive.

From examination after death, it appeared that the divided tube was completely obliterated, but the other was sound: both ovaries were evidently shrunk, proving, in addition to my previous observations, that their actions had been languid.

The result of this experiment disappointed me much; for no reasoning *a priori* had led me to entertain the smallest suspicion that a mutilation of one side only could destroy the harmony of the whole uterine system. But my disappointment originated chiefly from the apprehension that this effect would be uniform, that it was the result of a determined law of the part; and if so, it formed an insuperable obstacle to my research. Its importance to my project was too great to be discouraged from a single obstacle; therefore in justice to my undertaking, I was in some measure compelled to push the inquiry to such an extent, as should enable me to say with precision, whether

it is possible to impregnate an animal in the situation just described.

EXPERIMENT.

Two other rabbits full grown and perfectly healthy were made the subject of a repetition of the last experiment. The male was offered to them several times during the space of three months. They generally refused him, yet received him twice or three times each during this interval; but neither were impregnated. As the signs of degeneracy from their proper sexual character became daily more evident, they were devoted to anatomical inspection, and exhibited appearances in the ovaries like the former, but somewhat less in degree.

The rabbit keeper informing me that those which had already had a litter were more certain of breeding than those which had not; I determined to make a trial of one of this description, with a view to compensate for my former disappointment.

EXPERIMENT.

Being furnished with one of this kind, and from which the young had been taken away three weeks at the age of ten weeks, which, together with the month of gestation, amounted in the whole to four months from the last conception, I made this the subject of the experiment. Now, at this distance of time, it is not very probable that the ovaries should retain very evident vestiges of the preceding conception: but as it was a point of too much importance to be left in doubt, I determined to satisfy myself by ocular examination, which, by a little management, was effected. The traces of corpora lutea were

far from being evident, so that there was no danger of confounding them with any recent mark that might happen. The tube on one side was cut through as before, but to my unspeakable mortification this rabbit was as barren as the former, though tried several times during the space of three months. The generative organs were examined after death, and the appearances corresponded with those of former experiments.

In this case, as well as in a former, I had an opportunity of comparing the shrunk state of the ovaries after death, with the plump and healthy condition before the mutilation; and it affords an additional proof of that sympathetic connexion, or consent, between one part of the generative organs and another; and shows that in the production of a new animal, the co-operation of different parts is necessary; and further, that if the assistance of one part is wanting, the others, as if governed by a principle of intelligence, cease to continue their important work. But I was still in a state of suspense with regard to the end for which these experiments were instituted; and such an uninterrupted succession of failures on a point so essential to my present inquiries, I confess tended but little to animate me in the pursuit. I was beginning to suspect that the barrenness consequent to the division of only one of the tubes, was as determined a law in the œconomy of these parts, as it seemed to be in those cases where both tubes were cut through; and that nothing could prevent this sterility; but my contemplations were directed into another channel by the following experiment.

EXPERIMENT.

Having procured another rabbit, nearly under the same circumstances as the last, I operated precisely in the same mode, and had equal evidence too concerning the condition of the ovary. The result of this experiment was successful; for on admitting the male to her about one month from the operation, she betrayed no reluctance, and became impregnated. Ten days afterwards she was killed, and opened. Both ovaries retained their primitive plumpness, and manifested the evidences of impregnation. These evidences are the presence of corpora lutea, bearing the same precise characters as I have demonstrated in the former part of this essay. Those seated in the ovary of the mutilated side did not differ in any respect from the same bodies on the perfect side: but they were unattended with foetuses; whereas in the perfect side, there were as many foetuses as corpora lutea

As this experiment had succeeded, I examined the divided tube with attention, to satisfy myself whether its canal was obliterated; and of this I had the clearest proof; for it would not allow quicksilver, nor even air to pervade it.

Now here is matter for reasoning. Both ovaries, it seems, bear unequivocal proofs of impregnation, but foetuses are found only on one side.

Now, on what principle shall we explain these phænomena? It is certain that neither semen nor the aura seminalis could have touched the left ovary, and yet it bears the most unequivocal marks of recent impregnation. It must depend on some other cause than the actual contact of semen.

But an important subject for investigation here presents

itself. Why were there no fœtuses on the mutilated side; but only the corpora lutea? Is the application of the semen to the vagina or uterus sufficient to stimulate the ovaries to perform their first procreative operations, without enabling them to achieve any thing more? and does it require the permanent and active energies of this fluid, operating by direct contact on the surface of the ovaries, to produce the full measure of their effects? But as these are queries which cannot be answered from the mere reflexions of the closet, I must engage anew in the business of experimental inquiry. But the first step that ought to be taken in the management of this question, is to give full confirmation to the above fact, by a repetition of the experiment; I therefore engaged a keeper of rabbits to procure me six in high breeding condition, as soon as possible.

EXPERIMENT.

Within the space of a month, I cut through the fallopian tube on one side in six rabbits. The season was warm, and consequently favourable for breeding. As soon as they recovered they were admitted to the male: but out of this number two only were impregnated; and the keeper assured me that one of them had never been impregnated before. When the success in these experiments is compared with that of the former, there was no cause for complaint. Of these two which succeeded, one had three corpora lutea and three fœtuses in the perfect side, with two corpora lutea and *no* fœtuses on the imperfect side. The other, which was the virgin rabbit, had two corpora lutea and two fœtuses on the perfect side, with one corpus luteum and *no* fœtus on the mutilated side.

Having now three indisputable proofs of this important fact,

I consider it a full answer to any objection that can be urged on the ground of accidental appearance; and that what has been stated above, must, under the circumstances described, be considered as a law of the part; viz. *That the ovaries can be affected by the stimulus of impregnation, without the contact either of palpable semen, or of the aura seminalis.*

But I cannot expect that any physiologist, prepossessed with the common notion of the contact of semen, will yield assent to my position, without subjecting it to a severe scrutiny, and exposing every possible objection to which it is liable. It certainly would not be unphilosophic to ask, why fœtuses were not found either in the ovarium, or in the tube between it and the obliterated part, agreeably to the assertion of NUCK, if, as I contend, the ovary was affected by impregnation? Again, a tenacious opponent might further avail himself of this apparent difficulty, by alleging that if the tube had not been obliterated until after coition, the semen or its powers might have affected the ovary by actual contact; and the product of conception might have been more complete. And in support of this idea, he might adduce the result of an experiment said to have been made by NUCK, in which he made an extra-uterine case in a bitch, by tying one of the tubes three days after coition.

These objections have at least speciousness to recommend them to our notice; but it is from experiment alone that we can determine whether they have any solidity.

To the first difficulty I reply, that my experiments were not made under the same circumstances that NUCK's is said to have been; therefore, giving him full credit for what he has advanced, a similarity of result cannot be expected. But it is painful to me to differ from any writer of character in the statement of a

fact, where the truth is equally accessible to us both; and notwithstanding the respect I willingly bear towards a name that has both acquired and deserved considerable reputation, I must confess that it appears to me highly problematical, whether this celebrated experiment be a reality, or only an ingenious device. But some facts, which it will soon be in order to relate, will show (I think very clearly) that I rest my suspicion upon fair grounds. In the mean time I feel it incumbent on me to reply to the general principle of the objection, and to determine by experiment how far it is deserving attention.

Now, if there be any validity in the objection, it should necessarily follow, that if an opportunity was given for the semen to pass by the tubes to the ovaries; we might, by opening an animal at a proper time after coition, detect some disposition in the fimbriated extremities of the tubes to apply the semen, by first approaching, and afterwards embracing the ovaries; and this action ought, according to the common theory, to take place before the usual sign of conception is at all evident on those bodies, which in the rabbit is somewhat apparent in six hours, but unequivocally marked in twelve.

Again, admitting the probability of it, we are led to inquire by what power the semen can be conveyed to such a distant part. It must be either by the male, *vi jaculationis*, or by muscular power in the tubes, analogous to a peristaltic motion. If it were by the first mode, the conveyance would be instantaneous; but in the latter, some little time seems necessary to allow the tubes to be affected by the stimulus preparatory to their peristaltic action. Perhaps this question may receive some light from the sacrifice of a few animals, at different periods between the coitus and the first visible effects of impregnation;

and I considered it by no means inapposite to the subject, to determine whether these conjectures were authorized by any visible changes, either in the condition or situation of the tubes. But the fruits of this inquiry will appear by the following experiments.

EXPERIMENTS.

A female rabbit in high season was admitted to the male, and in a few minutes afterwards the ovaries and tubes were brought into view; but the fimbriæ were in their natural situation.

As soon as proper rabbits could be procured, I repeated this experiment on two others, with precisely the same consequence.

These facts militate strongly against the possibility of the conveyance of the semen to this part *vi jaculationis*, and demonstrably prove, as far as three facts can go, that if the moving power inheres in the female, it is not instantaneously exerted.

But are the powers of the fecundating fluid conveyed *at any time* by the tubes?

This simple question betrayed me into the prosecution of experiments to a greater extent than I at first expected; for the result of several of them was unsatisfactory: but being once engaged in the question, I felt myself compelled to prosecute it, by examining these parts at different periods from the coitus to the manifestation of its effects. But I found from a regular series of observations made on different rabbits, at every hour between the first and the ninth, that the fimbriæ remained nearly in their usual situation; and the only differ-

ence I perceived in the last hours, was a greater turgescency of vessels, as if preparatory to some important action. I desisted from this inquiry at the ninth hour, because the ovaries now bore very evident marks of impregnation; and there appeared to have been no action in the tubes by which the semen could have been conveyed to them.

The impression which these experiments at first made on my mind, was, I must confess, not altogether incongenial to my wish, in as much as they seemed to furnish a satisfactory answer to the question: but reflexions when more at leisure abated my confidence, and in the end convinced me that my proofs did not exceed probability, so that there was still room for the suggestions of scepticism: and indeed it might be said with great propriety, that the tubes might have inclined towards the ovaries in the intervals of the hours above mentioned, and have returned to their former situation, and thus have eluded my research. I think it but candid to acknowledge, that these last experiments do not prepare me to meet that objection.

These reflexions suggested to me the expediency of constructing a plan of inquiry more apposite to the subject; and attended with experiments bearing more directly on the point at issue. Under this impression I determined to obliterate one of the tubes at different periods *post coitum*, and after the lapse of a sufficient length of time, to notice the effect. My particular view in this was to allow sufficient time for the arrival of the semen at the ovaries, supposing it to take place; so that if they were stimulated by an affusion of that fluid, either in a palpable or insensible form, here would be time allowed sufficient to produce its effect; and if in this mode foetuses could

be formed, while by obliterating the tube *ante coitum* nothing more than corpora lutea were seen, it furnished an argument of no inconsiderable force in favour of impregnation by immediate contact; but if on the contrary, corpora lutea ONLY were found, then such experiments would give additional force to the arguments stated in a former part of this section.

EXPERIMENT.

One of the tubes of a rabbit was divided half an hour *post coitum*, and the wound closed as before. She was kept a fortnight, that I might know the result; but there were no marks of impregnation on either side.

Though a failure of impregnation has been very common in experiments connected with the mutilation of these parts, I apprehended that the derangement in the present instance proceeded from some disturbance given to the procreative operations in their commencement, and therefore determined in the next trial to wait a few hours, the better to avoid this.

EXPERIMENT.

I repeated the operation on two other rabbits, in one at four, and in the other at six hours after coition. On inspecting the parts at the end of a fortnight, the first was not impregnated, but the last was. In this there were four corpora lutea in the right side, answering to the same number of foetuses in the cornu uteri of that side; but on the left or imperfect side, there were three corpora lutea without foetuses. The corpora lutea on both sides were cut open, but not the slightest difference could be detected.

Now, if the contact of the semen with the ovaries in any

form be essential to impregnation, here has been an opportunity for such contact during the space of six hours ; but it has not been sufficient to advance the procreative operations further than happened in those experiments where the tube had been divided before coition. Let us then for a moment suppose that the interval be lengthened, in order to allow a better opportunity for producing the full effects of impregnation, by exposing the ovary a longer time to the stimulus of the semen.

EXPERIMENT.

I cut through the left tube of another rabbit twelve hours *post coitum*, and examined the parts on the fifteenth day. There were four corpora lutea with the same number of fœtuses on the right side, and three corpora lutea without fœtuses on the left ; so that twelve hours supposed exposure to semen, had made no sensible advances in the procreative operations on the mutilated side.

EXPERIMENT.

The same operation was repeated twenty-four hours *post coitum*. Corpora lutea were found in both ovaries, but fœtuses only on the perfect side.

Now I observed in one of the experiments related in the former part of this essay, that the vesicles of the ovaries when examined forty-eight hours *post coitum*, were extremely prominent ; they appeared as if going to burst : it is but reasonable then to admit, that at this time they must have received their full measure of stimulus ; and if one of the tubes was divided in this state of things, the result would be more decisive.

EXPERIMENT.

The operation was repeated under the circumstances just described, and in fourteen days the result was ascertained, viz. three corpora lutea and as many foetuses on the perfect side, and two corpora lutea without foetuses on the imperfect one.

Now, what mode of reasoning ought we to adopt here? Has the mutilating process suspended the effect of that stimulus which impregnation had begun? and are those appearances in the ovaries, any thing more than incipient relapses into evanescence? Such really appears to be the state of things, and seems to mark in a decided manner, a sympathetic connexion between one part of the uterine system and another. And were I to adopt the language of a late celebrated physiologist, I should say "that the ovary on the imperfect side, "feeling the inability of the tube to transmit its contents to "the uterus, the proper receptacle, had suspended the usual "operations of these parts, from a consciousness of their in-"utility."

This reasoning will probably appear not perfectly consentaneous to certain well established facts on the subject of extra-uterine foetuses; for dissection has fully evinced the possibility of a foetus being perfectly evolved, and of acquiring considerable bulk, either in the ovary, abdomen, or tube.

I do not hesitate to acknowledge the full force of these facts; but I cannot admit that they subvert the principle I wish to establish from experiment; because I conceive there is an essential difference whether nature spontaneously dispenses with her usual modes, and attempts to effect her ultimate purpose by irregular means; or whether, proceeding in

the ordinary course of her operations, she suffers an impediment which a physiologist may have produced to thwart her designs. In the first case, she may be provided with an expedient; in the last, she will probably be left without resource.

Here again we may notice the experiment mentioned by NUCK, which, though under similar circumstances, was attended with a different result. Some who feel themselves disposed to venerate his authority, will probably oppose his experiment to mine, and think it incumbent on me to account satisfactorily for the difference. I can by no means acknowledge such an obligation; for to confer validity on experiment by reasoning, is to invert the order of inquiry, and support facts by conjectures. It is sufficient for my credit to be able to adduce evidence of the truth of what I advance, and for this evidence I rely on my preparations.

The train of reasoning which I have lately pursued, led me to extend my inquiries into this particular question still further; and as in the last experiments the vesicles were known to be just on the point of bursting before the tube was cut through; the next step in the inquiry appeared to be, to determine the consequences of dividing the tube a short time after the rudiments of the fœtus had passed. Will the procreative operations be suspended, if the tube be cut through after the ovum is deposited in the uterus?

EXPERIMENT.

I repeated the operation on two rabbits, one of which had received the male two days and eighteen hours, the other two days and twelve hours. I knew from my own experiments, as well as those of DE GRAAF, that the vesicles had

discharged their contents before either of these periods. The examination of these at the usual time, proved that the actions of these parts suffer no interruption by a division of the tube made after the rudiments of the fœtus have been conveyed into the uterus ; for there were corporea lutea in both ovaries, and fœtuses in both cornua uteri.

These experiments I think overturn (as far as experiment can) every argument which has hitherto been adduced to support the hypothesis, that the affusion of the semen on the ovaries, either in a sensible form or in that of aura seminalis is essential to impregnation : for if the ovaries were susceptible of their proper excitement only by the contact of semen, by what accident has it happened that the effects of that excitement are not more obvious and further advanced in those experiments, where nothing was done to intercept its course for forty-eight hours, than in those where all communication between the uterus and ovary had been cut off before the means for impregnation had been employed ? We should expect in the one case to find the full effects of impregnation, and in the other no traces of it would be seen ; instead of which, the procreative actions are no further advanced where there has been an opportunity for the passage of the semen, than in those cases where the passage has been impossible. But if we defer the mutilation until the ovary has perfected its work, which it does in a rabbit in something more than fifty hours from the approach of the male, then the generative process is not disturbed, and the evolution of the fœtus goes on in the usual manner ; for now all the different parts of the uterine system being in a condition to act, each performs its peculiar office.

First. The semen by its presence stimulates either the vagina, os uteri, cavity of the uterus, or all of them.

Secondly. The impression made on these is propagated to the ovaries by consent of parts.

Thirdly. One or more of the ovarian vesicles enlarges, projects, bursts, and discharges its contents.

Fourthly. During this process in the ovary, the tube is undergoing a state of preparation for the purpose of embracing the ovary, and receiving the rudiments of the foetus.

Fifthly. This preparation consists in part of an increased turgescence of its vessels, and a consequent enlargement of its fimbriated extremity. When thus prepared, it approaches the ovary.

Sixthly. After the tube has performed its office by a peristaltic motion, commencing at the fimbriæ, and terminating in the uterus, it gradually returns to its former situation and condition.

Seventhly. While these different actions are going on in the appendages of the uterus, others not less important to the design of nature are instituted in the uterus itself: for the tunica decidua, where it is obvious, is formed ready to secure firmness of connexion between the tender ovum and internal surface of the uterus, until a proper attachment by means of placenta can be effected.

Eighthly. By way of guarding with additional security against a premature escape of the ovum, an apparatus, seated in the neck and mouth of the womb, now begins to develop its real structure, and perform its proper action, consisting in the secretion of a mucus-like substance, sufficient in quantity to fill completely the whole length of the neck, and by that

means to seal up the communication between the cavity of the uterus and vagina.

Nintbly. Nor does the care of nature for the preservation of the new animal terminate here ; for while she is by various means forming and perfecting her work, at least as far as comes within the province of the uterine system, she is at the same time making preparation for its nourishment after birth, by instituting the proper secretion of the breasts.

When we take a reflected survey of these successive operations, I think it must appear, on tracing nature's steps through the different stages of this work, *that they are the product of that law in the constitution which is called SYMPATHY, or consent of parts.*

That the semen first stimulates the vagina, os uteri, cavity of the uterus, or all of them.

By *sympathy* the ovarian vesicles enlarge, project, and burst.

By *sympathy* the tubes incline to the ovaries, and having embraced them, convey the rudiments of the foetus into the uterus.

By *sympathy* the uterus makes the necessary preparation for perfecting the formation and growth of the foetus. And,

By *sympathy* the breasts furnish milk for its support after birth.

Having now investigated this intricate question, I hope with some regularity ; the design of this essay next leads me to consider the state or form of that substance which passes from the ovaries in consequence of impregnation.

SECTION III.

What is the Form of that Substance which passes from the Ovaries in consequence of Impregnation ?

No sooner had the researches of the physiologists retraced the existence of the new-born animal to the ovaries, than their curiosity was excited to discover the form it assumed while resident in these bodies, and especially at that particular time when the fœtal primordia are about to escape from them. The analogous phænomena of oviparous animals, and the structure of the ovaries as described by DE GRAAF, concurred to favour an opinion, that in viviparous animals there existed ova in these bodies, and indeed from this very circumstance they received their name. But though several physiologists have concurred in this opinion, there has not been any strict coincidence respecting their state while in the ovary. Some have thought that the vesicles described by DE GRAAF were the true ova, and that these are the bodies that are expelled by impregnation. Others, with greater probability, have considered these vesicles as the apparatus destined by nature, under the influence of the proper stimulus, to form the ovum : and though at all times they contain a glairy kind of fluid, from the stimulus of impregnation this fluid becomes a small vesicle or ovum seated within the larger vesicle, which now becoming thickened, and acquiring a yellow colour, is called the corpus luteum : from this body the interior vesicle or ovum is protruded.

Others again refuse assent to both these opinions, and contend that the substance extruded from the corpora lutea has

no vesicular appearance; and though by some it has been called an ovum, yet that name is not applicable to it from any resemblance of figure, but rather from its agreement with an egg in being the substance in which the rudiments of the future animal are contained,

DE GRAAF contended that the primordia foetus while in the ovary is vesicular, as appears in his work; in which, after describing the enlargement of the proper vesicles usually connected with his name, he says, “*præterea aliquot post coitum diebus tenuiori substantia præditi sunt, et in sui medio limpidum liquorem membranâ inclusum continent, quo unâ cum membranâ foras propulso, exigua solum in iis capacitas superest.*” He is therefore decidedly of opinion, that as soon as the product of conception becomes the subject of notice, it has a vesicular form, and this he thinks takes place at the end of the third day, though the substance passes from the ovaries several hours before this time. He seems rather to assert, that it passes in a vesicular form, than to prove it; for in fifty-two hours after the approach of the male, he found the ovarian vesicles were empty, though he could not now find the new vesicles either in the uterus or the tubes. But in seventy-two hours they were so evident, that he could distinguish with ease the two membranes of which they are formed, viz. the chorion and amnios; so that they cannot be very small at this time. Hence it would follow, that if on a repetition of this experiment on the third day no vesicles should happen to be found, it would not be from minuteness that they would escape observation; therefore should any one be disposed to search for them, he need not bend his sight, as if looking at microscopical objects.

VALISNERI on the contrary searched for these eggs with great industry, accompanied with an ardent wish to find them ; but though his experiments appear to have been judiciously conducted, he never succeeded.

HALLER also maintains, from a regular series of experiments made on sheep (whose term of utero-gestation is five months), that some days elapse between the escape of the substance from the ovaries, and the appearance of a circumscribed body in utero, which can properly be called ovum : and that this does not happen until seventeen days from impregnation. In the mean time, nothing but irregular masses of mucus are found. The circumscribed form at this time acquired seems to depend on the formation of the foetal membranes now bounding the contained mucus-like substance. This apparently homogeneous mass, on the nineteenth day undergoes a change of character ; an opaque spot is seen within it, which subsequent observations prove to be the first evident marks of the evolution or formation of the foetus. From this dim speck of animal existence we may observe a series of regular advances, from an inorganized mucus-like mass to the most beautiful and complicated machine in nature. But to trace her progressive steps through this important work, forms no part of the design of this dissertation.

The chief difference between DE GRAAF and HALLER on this subject, consists in their opinions respecting the form of the substance that is passing from the ovaries, whether it is vesicular at this time or not ; for in the subsequent processes they differ but little. No solution can be given of this question by force of reasoning ; it is from experiment alone that we can receive conviction, notwithstanding the two contrary

opinions that prevail. All that can be expected from an individual in such a case, is to add the result of his own labours to one side or the other, so that in the end the preponderance must depend on the weight of evidence.

The experiments I have made on this simple question do not allow me to incline to the side of DE GRAAF; for in the rabbit I have never found any thing in the uterus which had a regular circumscribed form earlier than the sixth day, and even then the substance was bounded by a covering so very tender, that it scarcely had firmness sufficient to support the figure. Before the sixth day, I have never seen any thing but irregular mucus-like masses in the uterus; but after this time the substance has firmness sufficient to admit of preservation in spirits, a specimen of which I have in my collection of preparations.

This acquisition of figure does not depend so much on a difference of consistence, as on the formation of membranes inclosing this substance. These membranes when in a more advanced state of formation, are known by the names of *chorion* and *amnios*. The product of conception being arrived at this stage, may with some propriety be called an ovum, as it has acquired a determined figure; but the different constituent parts of it are not apparent at this early period; on the tenth day, in the rabbit, an opaque spot is seen in this ovum, which increasing daily in its bulk, progressively manifests the formation of the foetus.

It is a little remarkable that in the rabbit, where the term of utero-gestation does not exceed thirty days, a third part of that time should be required to make that opaque spot obvious to the sight, whilst the remaining two-thirds should suffice to complete the formation of the foetus. It appears as if it

required a more elaborate exertion of the formative powers of these parts to produce what might figuratively be called the nucleus of a foetus, than to go on and complete the work. But this remark applies only to the rabbit; for in the human female, abortions at the third month clearly prove that the evolution of the foetus has been perfected some time before. Such an obvious difference cannot fail to impress our minds with doubts and distrust, whenever we are drawing inferences from analogical reasonings: but to trace the formative process of nature through this work, and to compare her progressive advances in the different periods of utero-gestation, are foreign to the design of this essay.

It remains then for me to beg pardon for having so long trespassed on the patience of this Society.

IX. *Experiments in which, on the third Day after Impregnation, the Ova of Rabbits were found in the fallopian Tubes; and on the fourth Day after Impregnation in the Uterus itself; with the first Appearances of the Fœtus.* By William Cruikshank, Esq. Communicated by Everard Home, Esq. F. R. S.

Read March 23, 1797.

THE ancients imagined that the woman had her testicles, as well as the man, and her own semen. They taught, that in the coitus there was a mixture of the male and female semen in the uterus, and that from a process like fermentation between those two fluids, an embryo was produced. LEWENHOECK said the embryo belonged to the male; and saw, or thought he saw, animalcules in the male semen, resembling the animals to which they belonged. SPALLANZANI says, that the semen of male animals having no animalcules, impregnates as certainly as that of those which have them. This shows that those animalcules are not embryos. STENO, observing that there were round vesicles in the testicles of women, like the eggs of birds, called them ovaria, and said their structure was exactly similar to the ovaria of birds. After this the immortal HARVEY broached the doctrine of "*omnia ab ovo*;" that all animals were produced from ova. "*Nos autem asserimus, animalia omnia, et hominem ipsum, ex quibusdam ovis nasci.*"

The ova in the ovaria of rabbits are particularly described by DE GRAAF, whence HALLER calls them ova Graffiana.

But the ovaria of quadrupeds often contain vesicles of the hydatid kind; and it becomes difficult to distinguish between what are vesicles, and what are ova. The mark with me is this: the ova are inclosed in a capsule highly vascular from arteries and veins, carrying red blood. The hydatid vesicles are not vascular; at least their vessels carry no red blood. The calyx and the ovum, after impregnation, and even before it, in the state in which the quadruped is said to be *hot*, become black as ink, from the greater derivation of blood; and the ova resemble dark spots: they also come nearer the surface of the ovarium, so as to pout or project, at last, like the nipple in a woman's breast. Some hours after impregnation, the calyx and the coverings of the ovaria burst, and the ovum escapes; may fall into the general cavity of the abdomen, and form an extra-uterine fœtus; but almost always falls into the mouth of the fallopian tube, whose fimbriæ, like fingers, grasp the ovarium, exactly at the place where the ovum is to escape. What the appearance of the ovum was, when deprived of its calyx, or when descending the fallopian tube, was not known. DE GRAAF discovered this in the fallopian tubes of rabbits, in the year 1672; and says, "minutissima ova invenimus, quæ licet perexigua, gemina, tamen, tunica, amiciuntur;" and then adds, "hæc quamvis incredibilia, nobis demonstratu facillima sunt."

DE GRAAF had the fate of Cassandra, to be disbelieved even when he spoke the truth! Dr. HUNTER had his doubts; and the great HALLER, of whom I have always spoke in the language of Professor MARRHAR, "cujus auctoritas apud me plus valet, quam auctoritas omnium aliorum anatomicorum simul sumptorum," positively denies their truth. His words are,

“vix liceat admittere”—and afterwards, “denique, quod caput rei est, neque HARTMANNUS cum experimenta GRAFFIANA iteravit; neque VALISNERUS tot et tam variis in bestiis; neque ego in pene centum experimentis; neque nuperiorum anatomorum quispiam, vesiculam, quales sunt in ovariis, post conceptionem, aut in tuba vidimus aut in utero!”

In the beginning of summer 1778, I was conversing with Dr. HUNTER on this subject, and said, “I should like to repeat those experiments, now that lectures are over, and that I have the summer to myself.” “You shall make the experiments,” said he, “and I shall be at all the expence.” Accordingly he carried me to Chelsea, introduced me to a man who kept a rabbit warren, and desired him to let me have as many rabbits as I pleased. I made the experiments; and shall now lay a copy of my journal, then made, before this Society.

EXPERIMENT I.

May 30, 1778. I took a female rabbit, hot, (as the feeders term it) that is, ready to be impregnated, and disposed to receive the male. This they find out, not by exposing her to the male, but by turning up the tail, and inverting part of the vagina: its orifice and internal surface are then as black as ink, from the great derivation of blood to these parts. Having run the point of a double-edged dissecting knife through the spinal marrow, between the atlas and dentata, she instantly expired. I preferred this method of killing her, because when the circulation stopped, the internal parts would be found, respecting vascularity, exactly as in the living body. Upon examination some time after, I found the internal parts of

generation, exactly in the same state as the external; that is, as black as ink: the ovaria had, immediately under their external surfaces, a great number of black, round, bloody spots, somewhat less than mustard seeds. These black spots are the calyces or cups which secrete the ova; they are extremely vascular; the ova themselves are transparent, and carry no visible blood vessels. These calyces, on the expulsion of the ova, enlarge and become yellow, projecting above the external surface of the ovaria, and form the *corpora lutea*; a certain mark of conception in all quadrupeds, and in women themselves, whether the embryo is visible or not. The use of the *corpora lutea* is not yet made out; but the orifice, through which the ovum bursts into the fallopian tube is often extremely manifest, and always has a ragged border, as lacerated parts usually have. The fallopian tubes, independent of their black colour, were twisted like wreathing worms, the peristaltic motion still remaining very vivid; the fimbriæ were also black, and embraced the ovaria (like fingers laying hold of an object) so closely, and so firmly, as to require some force, and even slight laceration, to disengage them.

EXPERIMENT II.

I opened a female rabbit two hours after she received the male: the black bloody spots (just mentioned) now projected much above the surfaces of the ovaria, some of the ruptured orifices were just visible; but in many of these spots there was not the least vestige of an orifice; whence I conclude that they heal very quickly in general. While the animal was yet warm, I injected the arterial system with size coloured with vermilion, whence every thing I had before seen became now

more distinct, and the black spots, which I before conjectured to be congeries of vessels, were now proved to be so.

EXPERIMENT III.

I opened another female rabbit the third day after impregnation: that she was impregnated I could have no doubt, for I never knew impregnation fail if the female was hot, and the male had not been previously exhausted; besides the corpora lutea in the ovaria fully proved it: the appearances were the same as in the last, only the *corpora lutea* were larger; but though I examined the fallopian tubes in the sunshine, and with great care, I could not find any ova, neither in them nor in the horns of the uterus.

EXPERIMENT IV.

I opened another female rabbit the fifth day after conception: the appearances were much the same as in the former animal, only the *corpora lutea* were increased in bulk, but there was not the least vestige of an ovum any where that I could discover. I was now ready to exclaim with HALLER, “vix liceat admittere.”

EXPERIMENT V.

I opened another female rabbit on the eighth day after she had admitted the male: the ova were in the cavity of the uterus, and projected through its substance about the size of a large garden pea; when I cut off the most superior part, and cut into the cavities of the ova, the *liquor amnii* escaped in a proportionate quantity; by their adhesions to the internal surface of the uterus they remained extended, not collapsing in

the smallest degree; the foetus was not visible; but I had often made the chick, in my experiments on the incubated egg, become visible, by dropping on the spot, where I knew it must be, a drop of distilled vinegar; by dropping the vinegar on the bottom of the little cups I had made, by cutting off the tops of the cells, the foetus instantly became visible.

EXPERIMENT VI.

Opened another, ninth day: foetus contained within its amnion, floats in another fluid, between chorion and amnion, which are now at a considerable distance; this fluid jellies in proof spirit. Some *corpora lutea* have cavities, others none, nor the least appearance of orifice. The *corpora lutea* keep increasing as the foetus increases, are of a sand-red colour, and very vascular.

EXPERIMENT VII.

Opened a doe the eleventh day after coitus: ova very little larger than the last, nor the foetus: there were but two ova, though several *corpora lutea*. Some pellucid hydatids appeared hanging on the outside of the fallopian tubes. Could these be ova which had missed the passage? they were vascular: the heart of the foetus was full of blood; the umbilical vessels very distinct, but no chord as yet, contrary to DE GRAAF.

EXPERIMENT VIII.

Opened a doe the fourteenth day: seven *corpora lutea* in one ovarium, and one in the other; only two ova in the horns of the uterus, one in each; that in the horn next one of the ovaria with one *corpus luteum* was blighted, and the foetus invisible, even with distilled vinegar; in the other it was increased pro-

portionable to the time; the umbilical chord now for the first time distinct, and the tail detached from the under surface of the uterus; there was something unintelligible about the head, it was bifid on the side next the mouth, with a hole in each extremity; the intestines were now apparent, at least the rectum, as were the lower extremities.

EXPERIMENT IX.

Opened a doe sixth day complete: found the ova loose in the uterus, as described by DE GRAAF, and corresponding nearly to the *corpora lutea*, six in one horn and four in the other; the ova were transparent and of different sizes; they were double, and contained each an internal vesicle, there was a spot on one side in most of them, which I conceived to be the intended point of adherence between them and the uterus; the internal vesicle was not equally in proportion to the external, but in some larger, in others less; I even suspect I saw something of the foetus: a polypous excrescence in the uterus near the orifice of the fallopian tube, had detained four of the ova at that place; others were scattered in the uterus: just where one of these vesicles had become stationary a white vascular belt was beginning to form, and in the middle of this a cavity where the vesicle lay; the inner membrane I take to be amnion, the outer chorion.

EXPERIMENT X.

Opened a doe the seventh day: the ovaria were shrunk; there were something like three *corpora lutea*, but not distinct; there were two polypi or solid excrescences in the left horn of the uterus, but no ova.

EXPERIMENT XI.

The day after a doe had received the male I made a small opening on the left side of the abdomen, got down upon the uterus just where the fallopian tube goes off, tied the left tube close to the uterus, with a view to intercept the ova. The result of this mentioned afterwards.

EXPERIMENT XII.

Opened a doe the seventh day after coitus: ova all fixed and adhering to the uterus, even making a sensible swell in form of belts at different parts; the amnion appeared in some nearer the chorion than in others; the liquor between amnion and chorion very gelatinous, in many others less so. Saw nothing of foetus.

EXPERIMENT XIII.

Opened a doe eighth day after coitus: there were about ten or eleven ova; foetus distinct in almost every one, but not without the application of distilled vinegar for two or three minutes, and afterwards immersed in proof spirit; in some I found the brain, spinal marrow, and vertebræ, forming two columns at some distance; they afterwards gradually approached; for it was in one of the least forward that this was most evident.

EXPERIMENT XIV.

Opened a doe twenty-first day after the coitus: five vessels were seen going out of the navel in one of the foetuses, besides the urachus; the omphalo-mesenteric artery was very distinct,

and divided into two as it came to the mesentery; could not see the urachus or allantois well, nor the membrane to which the omphalo-mesenteric artery goes.

EXPERIMENT XV.

Opened a doe the fifth day after coitus: found the ova loose in the uterus, to the number of six; even these had a lesser coat in the inside, corresponding to amnion. None in the tubes.

EXPERIMENT XVI.

Opened (fourteen days after the operation) the doe whose fallopian tube I tied. The uterus of the right side was the size of the sixth day; the ovarium and uterus had gone backwards as to the process, and there was no appearance of foetus; though placenta was very evident on the left side, there was no appearance of conception in the uterus; no placenta; the fallopian tube was very large, soft, and tender; the ovarium twice the size of that on the other side, red, and covered with extravasated coagulable lymph; there was an hydatid in the course of the tube, containing a clear fluid, but nothing like foetus. I suspect that tying the tube prevented the ova on this side from coming out of the ovarium, and that though they rather increased in the ovarium, the process soon stopped; that the process went on, however, in the other side for a few days, and then stopped likewise: there was universal inflammation about the uterus and colon of the left side, with great quantities of white extravasated coagulable lymph; there was water in the abdomen, and all the appearance of peritoneal inflammation. This process seems to give but little pain, for the animal at the time she was killed was eating and looking as usual.

EXPERIMENT XVII.

Opened a doe the third day after the coitus: the pouting parts of the *corpora lutea* very transparent before the uterus was touched; but as soon as the spermatic and hypogastric arteries were divided, in order to cut out the uterus, they all, as if struck with some shock like electricity, became opaque. The pouting part I believe is the ovum, and stands upon the top of *corpus luteum*; it is very vascular, particularly at its basis, but as soon as perfect, or ready for expulsion, carries no red blood; it continues to grow of itself in utero, without adhering to the uterus for two or three days, then takes root, and becomes very vascular: nothing in the tubes or uterus.

EXPERIMENT XVIII.

Opened another the fourth day in the morning; but it had not conceived, and was in the state of one hot.

EXPERIMENT XIX.

Opened one in the evening of the fourth day: the appearances were little different from those of the fifth morning; the ova were only less dispersed through the uterus, and all accumulated about the orifice of the tubes; the amnion was likewise closer to the chorion.

EXPERIMENT XX.

Opened another at the end of the third day, or rather on the beginning of the fourth: the ovaria were dark brown; the fallopian tubes and uterus almost black, from the great quantity of blood derived to them at this time; I opened this uterus

on the upper edge and in the body, so that the parts all remained turgid; the spermatics and hypogastrics not cut through; the corpora lutea were very vascular, an artery running across ramified from both sides, but particularly spent itself in the centre; the upper part of the corpus luteum, or centre, was a little concave, like the head of a turned small-pock, but no evident foramen: I believe the ova were gone out, but I could see nothing of them in the tubes nor uterus; the fimbriæ were more vascular than I ever saw them, and wholly covered the ovaria; the peristaltic motion of the tubes was very evident, and greater than ever I had seen it; the inner surface of the horns was graniform, with white spots; this I suppose decidua, or perhaps corpus glandulosum EVERRAHDI. DE GRAAF saw the ova in the tubes this day.

EXPERIMENT XXI.

Opened a rabbit at six days and a half: ova in the horns of the uterus were just begun to fix, but did not adhere by vessels; they were very much enlarged compared with the sixth, and the side next the uterus had a round rough spot in it, now very conspicuous; the chorion and amnion were almost in contact with one another; they were easily turned out of the uterus, which embraced them every where loosely, but at the bottom; the *corpora lutea* now increased exceedingly in vascularity, and nourished by a large vessel running across the tubes; remarkably pale, as having done their duty; the graniform appearance on the uterus internally not observable as in the last.

EXPERIMENT XXII.

Opened a doe the seventh day complete after the coitus: turned out, but with difficulty, one of the ova a little larger than in the last; the substance of the uterus over these ova was become thin and transparent, so at first sight you would imagine it was the ovum naked, neither was this part so vascular as one might have expected, considering the principal change was going on here; the ovum burst the moment it was disengaged from the uterus; a gelatinous coagulable fluid issued out, but no appearance of fœtus even in the microscope.

EXPERIMENT XXIII.

Opened another rabbit at the end of the third day: same appearance as in Exp. xx.: searched in vain for the ova on the right side; at last, by drawing a probe gently over the fallopian tube on the left side, before it was opened, more than an inch on the side next the uterus, I pressed out several ova, which seemed to come from about its middle, as I began the pressure there, and the ova did not appear till the very last; the amnion made a centre spot, and appeared small compared to the chorion; no ova in the uterus.

EXPERIMENT XXIV.

Opened another at three days and a half: ovaria had the appearance as if the ova had not yet gone out; however, many of them were found in the uterus, and many in the tubes; I got about six; others were lost, from the great difficulty in slitting up the fallopian tubes without bruising the ova with the fingers or with the point of scissars; there

were eight or nine *corpora lutea* in one ovarium, and two only in the other; on the side of the two I only found one ovum, but twice as large as those on the other side. I observed that the redness of the uterus, depended on not losing much of the animal's blood; for when they had been so killed that much blood was lost, the fallopian tubes at least and ovaria were always pale.

EXPERIMENT XXV.

Opened another rabbit at two days and a half after the coitus: ovaria impregnated, but found no ova in the tubes, nor orifices in the *corpora lutea*.

EXPERIMENT XXVI.

Opened one, third day complete: found about six or seven ova in the fallopian tubes, near their end, or about an inch within the tube, on the side next the uterus: in the microscope the ovum appeared as having three coats; the middle one perhaps becomes allantois or membrana quarta.

EXPERIMENT XXVII.

Opened again another at two days and half: and though there were a great many *corpora lutea*, I could not discover any ova; they were probably too small to be perceived, for on the third day complete some of the ova were not perceptible, till they were put into a fluid, and viewed in the microscope.

EXPERIMENT XXVIII.

Opened one the third day all but two hours: found six ova in one fallopian tube, and seven in the other, which corres-

ponded exactly to the number of *corpora lutea* in each ovarium; the ova had three membranes as before. The circles in the cicatricula of the hen's egg are perhaps similar to these. The ova seem to enlarge in their way down the tube, as a pea swells in the ground before it begins to take root; even in the uterus, for two days, they are either loose and unconnected by vessels, or the vessels are so small as not to be discovered by the microscope. The *corpora lutea* were flatter on the head than I had ever seen them before.

EXPERIMENT XXIX.

I opened another at eight days and a half: every thing more distinct and more advanced than on the eighth day; the heart now visible, and resembling much the appearance of the incubated egg in the forty-eighth hour. There were seven *corpora lutea* in the right ovarium, and but four ova in the right horn of the uterus; there were also three in the left ovarium, though but two ova in the left horn.

GENERAL CONCLUSIONS.

1st. The ovum is formed in, and comes out of the ovarium after conception.

2dly. It passes down the fallopian tube, and is some days in coming through it.

3dly. It is sometimes detained in the fallopian tube, and prevented from getting into the uterus.

4thly. DE GRAAF saw one ovum only in the fallopian tube, "in oviductus dextri medio unum!" I saw thirteen in one instance, five in another, seven in another, and three in another, in all twenty-eight.

5thly. The ovum comes into the uterus on the fourth day.

6thly. DE GRAAF did not see the foetus till the tenth day; I saw it on the eighth.

7thly. These experiments explain what is seen in the human female. For,

A. I shew a child, at lectures, which remained in the ovaria till it was the size of the fifth month; its fluids were all wasted, and its solids were hard and compressed into an oval form; it had the chorion and amnion, its chord and placenta.

B. I also have in my possession the uterus and ovaria of a young woman who died with the menses upon her; the external membranes of the ovaria are burst at one place, from whence I suspect an ovum escaped, descended through the tube to the uterus, and was washed off by the menstrual blood.

C. The ovum sometimes misses the fallopian tube, falls into the abdomen, and forms the extra-uterine foetus; this sometimes grows to its full size, labour pains come on at the ninth month, the child may then be taken out alive by the Cæsarean section; or, dying and wasting, but not putrefying, may remain without much inconveniency to the mother for many years.

D. The ovum, although it has gone some way down the fallopian tube, may be arrested in its course and become stationary, and form what is called the fallopian tube case. A remarkable case of this kind is given by Dr. HUNTER, in his book on the gravid uterus, where the tube burst, and the mother bled to death.

E. Lastly; the ovum comes into the uterus, where there is room for its enlargement, and a passage for its exit from the body.

P. S. These experiments have been read, and the preparations and engravings shewn, in the lectures on the gravid uterus, given at Windmill-street, every year since the original date of this journal.

EXPLANATION OF THE PLATE (Tab. IV.)

It was not thought necessary to delineate the whole uterus of the rabbit, as it exactly resembles the uterus of other quadrupeds, consisting of a vagina, common to two horns, two fallopian tubes, and two ovaries. Any one who wishes to see this, may see it in DE GRAAF'S little book, tolerably well executed for the age in which he lived: but I am more concerned in his first appearances of the ova, than in his general anatomy of the uterus of the rabbit; and therefore proceed to explain the copy of a plate previously engraved, nineteen years ago.

The figures marked 3d day, are ova of the fallopian tube, found after impregnation on that day. The three first are of the natural size; the three next are magnified, in the simple microscope. In all of them the chorion and amnion are even now distinct, and in some of them the *allantois*, as I suspect.

The figures marked $3\frac{1}{2}$ day, are ova still more advanced; similar to which I found many in the tubes, many in the horns of the uterus. The three first are of the natural size; the two following are magnified also in the simple microscope.

The figures marked 4th day, are more enlarged ova in the horns of the uterus, loose, not adhering, capable of being

moved from one place to another (after these horns are opened) by the gentlest breath blown through a blow-pipe.

The figures marked 5th day, are ova of the fifth day; still loose in utero, and still capable of being blown with the gentlest breath from one part to another: they resemble the last in every thing, only that they are larger. The three first are of the natural size; the three last magnified, as the former ova.

The figures marked 6th day, are ova found in the horns of the uterus on that day; sensibly larger than the preceding; not adhering, even now, to the internal surface of the uterus, but exactly as the last in this respect. The four first are of the natural size, the three last magnified as before; but, as kept some years, the amnion has receded from the chorion to a considerable degree.

The figures marked 7th day, are ova of the seventh day: the first shews the ovum in its cell in the horn of the uterus, laid open; the three next are similar ova, taken out of their cells, and resembling the former; the three last are of the same period, and also removed from the uterus, but magnified by the same microscope as the preceding ova. They are seen after having been kept many years, and the secession of the amnion from the chorion is still more apparent and greater.

The figures marked 8th day: the first shows the foetus now first visible to the naked eye by dropping distilled vinegar on it, in one of the cells of the uterus opened. A little above is seen a cell turgid and unopened; and below a cell half divided. The two next figures, in the same line with the foetus mentioned, are foetuses of the same period from other rabbits,

magnified. They show the rudiments of the *vertebra*, and the first appearance of the *spinal marrow*. The third in the same row is also magnified, it shows also the earlier appearances of the two hemispheres of the brain.

Of the figures marked 9th day, one shows the foetus, now, for the first time, of itself visible to the naked eye, adhering near the tail to the placenta in the closest manner; the navel string as yet too short to be visible, as contrary to DE GRAAF as possible. The second shows the same foetus magnified.

The figure, on the outside of which is No. 10, shows a *fallopian tube*, on one side of the uterus of the rabbit, with its fimbriated orifice opening into abdomen; and its uterine orifice opening into uterus; also the ovarium, and *corpus luteum* in it, projecting above the surface.

3^d day

3¹/₂ day

4th day

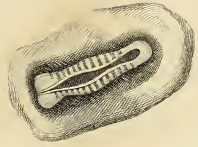
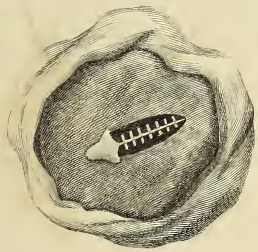
5th day



6th day



7th day



N^o 10.

9th day





X. *Letter from Sir Benjamin Thompson, Knt. Count of Rumford, F. R. S. to the Right Hon. Sir Joseph Banks, Bart. K. B. P. R. S. announcing a Donation to the Royal Society, for the Purpose of instituting a Prize Medal.*

AT the Anniversary of the Royal Society, held the 30th of November, 1796, the President acquainted the Society, that Count RUMFORD had transferred one thousand pounds three *per cent.* consolidated Bank Annuities to the use of the Society, on certain conditions stated in a letter to the President; which was read as follows :

“ SIR,

“ Desirous of contributing efficaciously to the advancement
 “ of a branch of science which has long employed my atten-
 “ tion, and which appears to me to be of the highest importance
 “ to mankind, and wishing at the same time to leave a lasting
 “ testimony of my respect for the Royal Society of London, I
 “ take the liberty to request that the Royal Society would do
 “ me the honour to accept of one thousand pounds stock, in
 “ the three *per cent.* consolidated public funds of this country;
 “ which stock I have actually purchased, and which I beg leave
 “ to transfer to the President, Council, and Fellows of the Royal
 “ Society; to the end that the interest of the same may be by
 “ them, and by their successors, received from time to time
 “ for ever, and the amount of the same applied and given, once
 “ every second year, as a premium to the author of the most

“ important discovery, or useful improvement, which shall be
“ made and published by printing, or in any way made known
“ to the public, in any part of Europe, during the preceding
“ two years, on Heat, or on Light; the preference always being
“ given to such discoveries as shall, in the opinion of the Pre-
“ sident and Council of the Royal Society, tend most to pro-
“ mote the good of mankind.

“ With regard to the formalities to be observed by the Pre-
“ sident and Council of the Royal Society, in their decisions
“ upon the comparative merits of those discoveries, which in
“ the opinion of the President and Council may entitle their
“ authors to be considered as competitors for this biennial pre-
“ mium, the President and Council of the Royal Society will
“ be pleased to adopt such regulations as they in their wisdom
“ may judge to be proper and necessary. But in regard to the
“ form in which this premium is conferred, I take the liberty
“ to request, that it may always be given in two medals, struck
“ in the same die, the one of gold, and the other of silver; and
“ of such dimensions, that both of them together may be just
“ equal in intrinsic value to the amount of the interest of the
“ aforesaid one thousand pounds stock during two years; that
“ is to say, that they may together be of the value of sixty
“ pounds sterling.

“ The President and Council of the Royal Society will be
“ pleased to order such device or inscription to be engraved on
“ the die they shall cause to be prepared for striking these me-
“ dals, as they may judge proper.

“ If, during any term of years, reckoning from the last ad-
“ judication, or from the last period for the adjudication of this
“ premium, by the President and Council of the Royal Society,

“ no new discovery or improvement should be made in any part
“ of Europe, relative to either of the subjects in question (Heat or
“ Light), which, in the opinion of the President and Council of
“ the Royal Society, shall be of sufficient importance to deserve
“ this premium; in that case, it is my desire that the premium
“ may not be given, but that the value of it may be reserved,
“ and being laid out in the purchase of additional stock in the
“ English funds, may be employed to augment the capital of
“ this premium; and that the interest of the same by which
“ the capital may, from time to time, be so augmented, may
“ regularly be given in money with the two medals, and as an
“ addition to the original premium at each such succeeding ad-
“ judication of it. And it is further my particular request, that
“ those additions to the value of the premium, arising from its
“ occasional non-adjudications, may be suffered to increase with-
“ out limitation.

“ With the highest respect for the Royal Society of London,
“ and the most earnest wishes for their success in their labours
“ for the good of mankind,

“ I have the honour to be, &c.

(signed)

“ RUMFORD.”

London, 12th of July, 1796.

To Sir JOSEPH BANKS, Bart. K. B. President
of the Royal Society of London.

The Society hereupon resolved, that they accept of the dona-
tion, and accede to the conditions annexed to it by the Count;

and also directed that a letter be written to the Count, acquainting him of this acceptance; returning him thanks for the liberal donation, and assuring him that the conditions annexed to it will be strictly adhered to.

ERRATA.

Page 158, 3d line from the bottom, *for* fig. 8. *read* fig. 10.

Page 205, l. 14, *for* "there was no appearance of conception in the uterus; no placenta;" *read* "there was no other appearance of conception in the uterus; no other placenta;" &c.

METEOROLOGICAL JOURNAL,

KEPT AT THE APARTMENTS

OF THE

ROYAL SOCIETY,

BY ORDER OF THE

PRESIDENT AND COUNCIL.

METEOROLOGICAL JOURNAL

for January, 1796.

1796	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hy- gro- me- ter.	Rain.	Winds.		Weather.
		H.	M.	o	o	Inches.		Inches.	Points.	Str.	
	o										
Jan. 1	37	8	o	38	54	30,08	80	0,110	SW	2	Fair.
	44	2	o	44	57	30,03	74		SW	1	Fine.
2	38	8	o	39	51	29,95	81		SW	1	Fine.
	48	2	o	47	57	29,72	76		SSW	1	Cloudy.
3	39	8	o	42	53,5	29,82	80	0,109	SW	1	Cloudy.
	48	2	o	44	57	29,83	82		E	1	Rain.
4	42	8	o	46	54	30,15	86		SW	1	Cloudy.
	49	2	o	48	57	30,19	80		WSW	1	Cloudy.
5	46	8	o	46	55	30,20	81		SSW	1	Cloudy.
	48	2	o	46	57	30,13	80		SSW	1	Cloudy.
6	45	8	o	48	55	30,05	83		S	2	Cloudy.
	51	2	o	46	57,5	30,10	81		NW	1	Cloudy.
7	36	8	o	42	54	30,18	82	0,202	SSW	1	Cloudy.
	50	2	o	49	58	30,14	85		SSW	1	Cloudy.
8	45	8	o	46	55	29,98	82		ESE	1	Cloudy.
	49	2	o	46	59	29,88	82		ESE	1	Cloudy.
9	39	8	o	42	56	29,65	84		ESE	1	Fair.
	50	2	o	46	58,5	29,55	80		ESE	1	Fine.
10	39	8	o	41	56	29,46	82		E	1	Cloudy.
	48,5	2	o	48	57	29,45	82		ENE	1	Cloudy.
11	41,5	8	o	45	55	29,51	85		S	2	Cloudy.
	48	2	o	46	58	29,50	84		SSE	2	Rain.
12	43	8	o	47	55	29,85	84	0,071	S	2	Cloudy.
	53	2	o	52,5	58	29,84	85		S	2	Cloudy.
13	51	8	o	51	57,5	30,00	85		S	2	Cloudy.
	55	2	o	55	55,5	29,95	84		S	2	Cloudy.
14	50	8	o	50	57	29,96	83	0,047	SW	2	Cloudy.
	54	2	o	54	60,5	29,94	77		SSW	2	Hazy.
15	48	8	o	48	59	30,00	75		SW	2	Fine.
	55	2	o	53,5	61	30,11	75		SW	2	Fine.
16	49	8	o	51	59	30,23	81		S	2	Cloudy.
	55	2	o	54	62	30,28	78		S b. W	1	Cloudy.

METEOROLOGICAL JOURNAL

for January, 1796.

1796	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.	o	o	Inches.	Inches.	Points.	Str.		
	o										
Jan. 17	45	8	o	46	58	30,32	83		S	1	Fine.
	50	2	o	50	61,5	30,30	81		SSE	1	Fine.
18	42	8	o	46	58	30,14	81		S	1	Fair.
	53	2	o	51	60	30,11	83		S	1	Cloudy.
19	48	8	o	49	58,5	30,08	81		S	2	Fair.
	54	2	o	53	60	29,96	75		S	2	Fair.
20	46	8	o	48	57	29,72	78		S	2	Fair.
	53	2	o	52	60	29,78	75		S	2	Fair.
21	50	8	o	54	58	29,61	77		SSW	2	Cloudy.
	56	2	o	55	60,5	29,68	72		SSW	2	Cloudy.
22	47	8	o	47	58	29,68	74		SE	1	Fair.
	56	2	o	53	62	29,58	69		SE	1	Fine.
23	49	8	o	49	59	29,59	76		S	2	Cloudy.
	52,5	2	o	51,5	60	29,46	73		S	2	Cloudy.
24	47	8	o	47	57	29,26	74	0,049	SSW	2	Fine. [much wind last night.
	50,5	2	o	46	58	29,49	73		W	2	Fair.
25	45	8	o	48	55	29,15	78	0,101	S	3	Cloudy. [much wind last night.
	50,5	2	o	48	58	29,15	77		S	2	Rain.
26	42,5	8	o	44	56	29,30	77	0,070	S	2	Cloudy.
	50	2	o	49	57	29,12	80		S	2	Rain.
27	42	8	o	42	55	29,24	76	0,422	SSW	2	Cloudy.
	48,5	2	o	48,5	58	29,35	73		SSW	2	Fair.
28	42	8	o	42	56	29,24	80	0,202	S	1	Rain.
	47	2	o	45	58	29,18	80		SSW	1	Rain.
29	40	8	o	47	56	29,02	80	0,375	S	2	Cloudy.
	50	2	o	49	58	29,00	75		S	2	Cloudy.
30	41	8	o	42	54	29,03	80	0,155	SSE	1	Fair.
	50	2	o	49,5	58	29,04	76		S	2	Cloudy.
31	44	8	o	44	54	29,09	81	0,215	SSE	1	Cloudy.
	48	2	o	48	57	29,05	80		SE	1	Cloudy.

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for February, 1796.

1796	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom. Inches.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.	o	o			Inches.	Points.	Str.	
	o										
Feb. 1	43	7	o	43	54	29,06	80	0,118	S	2	Rain.
	48	2	o	44	56	29,09	78		S	2	Rain.
2	38	7	o	39	54	29,28	79	0,038	ESE	2	Fair.
	45,5	2	o	44	57	29,27	77		SE	2	Cloudy.
3	33	7	o	33,5	53,5	29,65	81	0,130	SW	1	Fair.
	44	2	o	42	57	29,61	76		S b. E	2	Fair.
4	36	7	o	37,5	54	29,55	81		N	1	Cloudy.
	45	2	o	45	56,5	29,68	75		NNW	1	Fair.
5	36,5	7	o	40	54	29,18	82	0,172	S b. E	2	Rain.
	48	2	o	46	56	29,20	73		SSW	2	Cloudy.
6	39	7	o	39	54	29,15	80		SW	1	Fair.
	46	2	o	46	57	29,27	76		WNW	1	Cloudy.
7	35	7	o	36	54	29,45	80	0,064	SW	1	Cloudy.
	45	2	o	44	55	29,28	76		SE	1	Cloudy.
8	40	7	o	42	53	29,18	80	0,170	S	1	Fair.
	49	2	o	48,5	55,5	29,09	78		S	1	Cloudy.
9	37,5	7	o	38,5	54	29,05	83	0,121	SW	1	Cloudy.
	46,5	2	o	46	56	29,18	77		N	1	Cloudy.
10	39	7	o	39	53	29,70	78	0,020	NE	2	Cloudy.
	41	2	o	41	53	29,92	74		NE	2	Cloudy.
11	32	7	o	33	52	30,23	77		N	1	Fair.
	42	2	o	41	54	30,22	75		SSW	1	Cloudy.
12	40	7	o	44	53	29,93	85	0,102	S	2	Rain.
	52	2	o	52	55	29,70	86		S	2	Cloudy.
13	35	7	o	35	53	29,64	78	0,208	SW	1	Fine.
	44	2	o	40	55	29,54	77		NNE	2	Cloudy.
14	36	7	o	37	53	29,57	78		WNW	1	Fair.
	47	2	o	46,5	55	29,57	71		NW	2	Fair.
15	35	7	o	35,5	52,5	29,81	75		NNW	2	Fine.
	44,5	2	o	44,5	56	29,88	74		N	1	Fair.
16	35	7	o	36	53	30,04	75		W	1	Fair.
	48	2	o	48	55	30,05	73		SW	1	Cloudy.

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for February, 1796.

1796	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.	o	o	Inches.		Inches.	Points.	Str.	
	o										
Feb. 17	42	7	o	43	53	29.97	80		SW	1	Fair.
	50	2	o	50	56	30.00	70		W	1	Cloudy.
18	39.5	7	o	40	54	30.07	80		SSW	1	Cloudy.
	48.5	2	o	48	56	30.05	74		SSW	1	Cloudy.
19	48	7	o	48.5	55.5	30.00	81		WSW	1	Cloudy.
	56	2	o	55.5	58	30.06	76		WNW	1	Cloudy.
20	46	7	o	46	58	30.10	78		WNW	1	Cloudy.
	51.5	2	o	51	58.5	30.10	72		WNW	1	Cloudy.
21	43	7	o	43	57	30.15	76		WSW	1	Cloudy.
	47	2	o	47	58.5	30.15	74		W	1	Cloudy.
22	43	7	o	43	56	30.07	73		E	1	Cloudy.
	44	2	o	44	58	30.05	73		ESE	1	Cloudy.
23	35	7	o	39	56	30.05	79		ENE	1	Cloudy.
	48.5	2	o	47.5	58	30.04	71		ENE	1	Fair.
24	34	7	o	35	55	30.13	78		ENE	1	Fair.
	45	2	o	44.5	57.5	30.16	66		ENE	1	Fine.
25	38	7	o	35	55	30.30	79		E	1	Cloudy.
	43	2	o	42	56	30.31	74		NE	1	Cloudy.
26	38	7	o	38	55	30.30	75		NE	1	Cloudy.
	43	2	o	43	57	30.31	74		NE	1	Cloudy.
27	37	7	o	38	54	30.31	78		NE	1	Cloudy.
	42.5	2	o	41	56.5	30.24	67		NE	2	Cloudy.
28	30	7	o	30.5	52	30.28	71		NE	2	Cloudy.
	37	2	o	36	53.5	30.25	71		NE	1	Cloudy.
29	30	7	o	31	51	30.09	77		E	1	Snow.
	33	2	o	32.5	54	30.14	68		NE	2	Fair.

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for March, 1796.

1796	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.	o	o	Inches.		Inches.	Points.	Str.	
	o										
Mar. 1	27	7	o	30	49	29,88	71		NE	2	Cloudy.
	39	2	o	38	51	29,88	68		NE	2	Cloudy.
2	33	7	o	35	49	29,82	73		NE	2	Cloudy.
	41	2	o	40	51	29,82	67		NE	2	Fair.
3	33	7	o	33	48,5	29,80	70		NE	1	Cloudy.
	37,5	2	o	36	52	29,71	65		E	1	Fair.
4	28,5	7	o	31,5	48,5	29,87	71		NE	1	Cloudy.
	36,5	2	o	36	51	29,95	65		NE	1	Cloudy.
5	26,5	7	o	27	48	30,13	70		E	1	Cloudy.
	38	2	o	37	52	30,19	58		ESE	1	Fine.
6	29	7	o	30	49	30,32	64		SE	1	Cloudy.
	33	2	o	30,5	50	30,35	71		E	1	Cloudy.
7	29	7	o	29	47	30,30	68		NE	1	Cloudy.
	32	2	o	31,5	50	30,29	68		NE	2	Cloudy.
8	27,5	7	o	29	48	30,28	69		ENE	1	Cloudy.
	38	2	o	37,5	50	30,23	68		ENE	1	Fair.
9	33	7	o	34	48	30,06	74		NE	1	Cloudy.
	43	2	o	42	50	30,00	71		E	1	Fair.
10	30	7	o	32	48,5	29,97	80		NE	1	Fair.
	48	2	o	48	53	29,95	67		E	1	Fair.
11	33	7	o	34	50	29,91	79		E	1	Fair.
	45	2	o	45	54	29,90	78		E	1	Fair.
12	40	7	o	42	52	29,98	79		E	1	Cloudy.
	53	2	o	50	55	30,03	76		SSE	2	Cloudy.
13	46	7	o	46	54	30,17	84		SSW	1	Cloudy.
	52	2	o	52	56	30,21	79		S	1	Cloudy.
14	42	7	o	44	54	30,26	83		SSE	1	Fair.
	54	2	o	54	57	30,23	65		S	1	Fair.
15	43	7	o	46	54	30,18	81		S	1	Hazy.
	59	2	o	58	58	30,18	68		SSE	2	Fine.
16	42	7	o	43	56	30,17	76		ENE	1	Fair.
	60	2	o	59	58,5	30,18	66		E	1	Hazy.

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1796	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygro-meter.	Rain.	Winds.		Weather.
		H.	M.	o	o	Inches.		Inches.	Points.	Str.	
	o										
Mar. 17	39	7	o	40	56	30,19	75		ENE	1	Fair.
	56	2	o	56	59	30,18	62		E	1	Fine.
18	37	7	o	38	58	30,12	72		ENE	1	Fine.
	55	2	o	55	60	30,10	63		E	1	Fine.
19	37	7	o	38	58	30,26	74		ENE	1	Fine.
	54	2	o	53	59	30,32	63		E	1	Fine.
20	35	7	o	39	58	30,44	74		NE	1	Cloudy.
	46	2	o	46	59	30,42	66		NE	1	Cloudy.
21	39	7	o	42	55	30,36	72		NE	1	Cloudy.
	51	2	o	51	60	30,36	65		NE	1	Fine.
22	41	7	o	42	58	30,35	70		NE	1	Cloudy.
	49	2	o	49	59	30,30	67		NE	1	Cloudy.
23	41	7	o	42	55	30,22	72		NE	1	Cloudy.
	47	2	o	47	57	30,19	67		NE	1	Cloudy.
24	40	7	o	41	55	29,99	70		W	1	Cloudy.
	48	2	o	48	56	29,90	68		W	1	Cloudy.
25	34	7	o	35	54	29,90	68		N	2	Fine.
	44	2	o	44	56	29,94	59		NE	1	Fine.
26	41	7	o	46	55	29,73	74		NW	1	Cloudy.
	54,5	2	o	53	58	29,72	68		NW	1	Cloudy.
27	33	7	o	33	56	29,50	77		NE	2	Snow.
	39	2	o	38	57	29,63	66		NE	2	Fine.
28	30	7	o	32	54	29,68	75		NNE	2	Cloudy.
	42	2	o	42	56	29,70	68		NE	2	Cloudy.
29	28	7	o	31	53	29,80	72	0,043	W	1	Fine.
	47	2	o	45	54	29,79	69		W	1	Cloudy.
30	36	7	o	40	54	29,66	74		WSW	1	Cloudy.
	51	2	o	48	56	29,56	76		S	1	Cloudy.
31	42	7	o	42	55	29,74	79	0,031	SW	1	Cloudy.
	55	2	o	54	57	29,81	70		SW	1	Cloudy.

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for April, 1796.

1796	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.	o	o	Inches.		Inches.	Points.	Str.	
Apr. 1	o										
	45	7	o	47	56	29,83	82		S	1	Cloudy.
2	56	2	o	56	57	29,88	79		SSE	1	Cloudy.
	46	7	o	46	57	29,93	81		E	1	Cloudy.
3	59	2	o	58	59	29,96	73		ESE	1	Cloudy.
	49,5	7	o	51	58	30,10	82	0,068	E	1	Rain.
4	58	2	o	57,5	60	30,11	78		E	1	Cloudy.
	43	7	o	45	58	30,16	81	0,212	E	1	Hazy.
5	63	2	o	61	61	30,14	76		E	1	Fair.
	42	7	o	45	59	30,15	75		E	1	Fine.
6	56	2	o	56	61	30,15	67		E	1	Fine.
	37	7	o	40	58	30,17	77		E	1	Fine.
7	51	2	o	49	61	30,16	63		E	1	Fine.
	37	7	o	40	58	30,20	78		E	1	Hazy.
8	52	2	o	50,5	59	30,19	62		E	1	Cloudy.
	36	7	o	41	58	30,26	77		ENE	1	Hazy.
9	51,5	2	o	49	58	30,22	66		ENE	1	Cloudy.
	38	7	o	40	57	30,11	75		NE	1	Fair.
10	50	2	o	48,5	58	29,99	64		NE	1	Cloudy.
	37	7	o	42	56	29,96	77		NE	1	Fine.
11	50	2	o	49	57	29,96	68		NE	1	Cloudy.
	39	7	o	41	55	29,95	78		NE	1	Cloudy.
12	50	2	o	49	57	29,92	68		NE	1	Cloudy.
	41,5	7	o	44	55	29,95	75		NE	1	Cloudy.
13	50,5	2	o	49	57	29,98	68		NE	1	Cloudy.
	36	7	o	39	55	30,11	76		NE	1	Cloudy.
14	51	2	o	50,5	57	30,11	66		NE	1	Cloudy.
	39	7	o	42	55	30,13	71		W	1	Cloudy.
15	55,5	2	o	55	58	30,10	63		NW	1	Cloudy.
	45	7	o	48	57	30,20	72		NW	1	Cloudy.
16	63	2	o	62	59,5	30,20	66		NW	1	Fair.
	50	7	o	50	58	30,18	76		W	1	Cloudy.
	61	2	o	58	60	30,14	68		W	1	Cloudy.

METEOROLOGICAL JOURNAL

for April, 1796.

1796	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygro-meter.	Rain.	Winds.		Weather.
		H. M.	°	°	Inches.		Inches.	Points.	Str.	
Apr. 17	47,5	7 0	50	59	30,14	73		WNW	1	Cloudy.
	62	2 0	60	61	30,14	68		NW	1	Cloudy.
18	46	7 0	47	59	30,13	77		W	1	Hazy.
	65	2 0	64	62	30,16	66		W	1	Cloudy.
19	45	7 0	48	60	30,16	77		ESE	1	Cloudy.
	64,5	2 0	64	63	30,13	64		ESE	1	Fine.
20	45	7 0	50	61	30,12	70		E	1	Fine.
	60	2 0	60	63	30,06	61		E	2	Fine.
21	45	7 0	50	61	30,00	71		E	2	Fine.
	65	2 0	64	64	29,99	63		E	2	Fine.
22	48	7 0	52	63	30,10	69		E	1	Fair.
	67	2 0	66	64	30,10	59		E	1	Fine.
23	47	7 0	50	63	30,10	72		E	1	Hazy.
	70	2 0	68,5	64,5	30,06	62		E	1	Cloudy.]
24	50	7 0	53	63	30,04	76		WSW	1	Cloudy.
	60	2 0	59	64	30,08	63		NW	1	Fair.
25	42	7 0	47	62	30,20	70		NE	2	Fine.
	57	2 0	56	63	30,19	64		NE	2	Fair.
26	43	7 0	46	61	30,32	72		NE	2	Fine.
	60	2 0	58	64	30,30	63		NE	1	Fine.
27	41	7 0	42	61	30,30	74		NE	1	Cloudy.
	56	2 0	55	62	30,22	67		SE	1	Hazy.
28	45	7 0	47	61	29,97	73				Fair.
	65	2 0	65	63,5	29,81	59				Fine.
29	45	7 0	48	62	29,50	72		SW	1	Cloudy.
	61,5	2 0	60	63	29,36	62		SSE	1	Fair.
30	45	7 0	47	61	29,14	74		E	2	Cloudy.
	56,5	2 0	51	62	29,08	72	0,022	SE	2	Rain.

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1796	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hy- gro- me- ter.	Rain.	Winds.		Weather.
		H.	M.	o	o	Inches.	Inches.	Points.	Str.		
	o										
May 1	40,5	7	o	44,5	61	29,16	73		SW	1	Fair.
	59	2	o	56	62	29,18	64		ESE	1	Cloudy.
2	42	7	o	48	60	29,35	73		ENE	1	Fair.
	59	2	o	58	62,5	29,43	65		ENE	1	Fair.
3	45	7	o	46	60	29,66	79	0,105	E	1	Rain.
	49	2	o	49	60	29,71	76		E	1	Cloudy.
4	41,5	7	o	44	59	29,81	72	0,102	NE	1	Cloudy.
	50	2	o	50	59	29,81	67		NE	1	Cloudy.
5	39	7	o	44	58	29,84	73		NE	1	Fair.
	53	2	o	52	59	29,84	63		NE	1	Cloudy.
6	39	7	o	45	57	29,90	70		NE	1	Cloudy.
	55	2	o	53	58	29,88	72		NE	1	Cloudy.
7	41	7	o	47	58	29,96	74	0,117	WSW	1	Cloudy.
	60	2	o	58	59	29,91	64		S	1	Fair.
8	46	7	o	51	58	29,53	85	0,370	WNW	1	Rain.
	58	2	o	57	60	29,69	71		W	1	Cloudy.
9	50	7	o	53	59	29,65	75	0,236	SW	2	Fine.
	64	2	o	63	62	29,69	64		WSW	2	Cloudy.
10	50	7	o	52	60	29,66	75	0,064	SW	2	Fair.
	63	2	o	62	62	29,66	65		SW	2	Fair.
11	50	7	o	52	61	29,55	77	0,116	SSW	2	Fair.
	62	2	o	60	62	29,62	66		SSW	2	Fair.
12	49	7	o	52	60	29,72	75	0,120	SSW	2	Fair.
	62	2	o	59	63	29,69	68		SW	2	Fair.
13	45	7	o	47	60	29,41	74	0,101	SSW	2	Fair.
	59	2	o	57	61	29,52	67		SSW	2	Fair.
14	45	7	o	47	60	29,86	74	0,172	NW	1	Cloudy.
	57,5	2	o	57	61	29,91	65		NW	1	Fair.
15	41	7	o	44	60	29,87	77	0,154	NW	1	Fine.
	57	2	o	55	61	29,82	70		NW	1	Cloudy.
16	41	7	o	44	59	29,85	76	0,056	NNW	1	Fair.
	55	2	o	53	60,5	29,95	71		ENE	1	Fair.

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1796	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom. Inches.	Hy-gro-me-ter.	Rain. Inches.	Winds.		Weather.
		H.	M.						Points.	Str.	
	0										
May 17	39	7	0	44	59	30,22	73	0,021	ENE	1	Fine.
	57	2	0	56	60	30,22	64		E	1	Fine.
18	43	7	0	50	58,5	30,12	78		E	1	Fine.
	62,5	2	0	62	61,5	30,04	68		E	2	Fine.
19	49,5	7	0	53	60	29,98	72		E	2	Fine.
	63,5	2	0	62	61	29,94	61		E	2	Fine.
20	47,5	7	0	52	60	29,85	72		ENE	2	Fine.
	65	2	0	63,5	62	29,84	61		E	2	Fine.
21	49	7	0	53	61	29,72	82		NE	1	Cloudy.
	57	2	0	54	61,5	29,71	78		ENE	1	Cloudy.
22	48	7	0	52	61	29,82	74		NW	1	Cloudy.
	63	2	0	61	62	29,85	66		W	1	Fair.
23	48	7	0	50	60	30,00	80	0,040	NW	1	Rain.
	62,5	2	0	61	62	30,05	69		NW	1	Cloudy.
24	48	7	0	49	61	30,10	77		NE	1	Cloudy.
	57,5	2	0	57	62	30,02	71		NE	1	Cloudy.
25	45	7	0	46	60	29,84	80		NE	1	Cloudy.
	64	2	0	63	63	29,76	66		ENE	1	Fine.
26	47	7	0	51	61	29,86	71		WSW	1	Fair.
	69	2	0	67	62	29,79	64		WSW	1	Cloudy.
27	50	7	0	56	61,5	29,78	74		W	1	Cloudy.
	65	2	0	64	63	29,74	67		W	1	Fair.
28	46	7	0	50	61,5	29,86	75		W	1	Hazy.
	62	2	0	59	62	29,80	65		SW	2	Cloudy.
29	47	7	0	50	60	29,55	73	0,058	SSW	2	Fine.
	61	2	0	57	61	29,53	70		SSW	2	Cloudy.
30	49	7	0	51	59	29,00	80	0,164	S	2	Rain.
	58	2	0	57	60	28,94	68		S	2	Cloudy.
31	48	7	0	50	58	29,18	80	0,305	SW	1	Rain.
	60	2	0	59	60	29,34	70		SW	2	Fair.

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1796	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.	o	o	Inches.		Inches.	Points.	Str.	
June 1	o									
	47	7 o	51	60	29,72	72	0,072	SSW	2	Fair.
2	62	2 o	60	60	29,72	67		SSW	2	Fair.
	45	7 o	50	59	29,69	70		SSE	2	Fair.
3	60	2 o	57	59	29,56	67		SE	2	Hazy.
	49	7 o	51	59	29,44	79	0,156	NNE	1	Cloudy.
4	58	2 o	58	59	29,50	70		NW	1	Cloudy.
	47	7 o	48	59	29,62	77	0,076	WNW	1	Cloudy.
5	58	2 o	58	59	29,78	69		NW	1	Fair.
	46	7 o	51	59	30,04	74		WSW	1	Hazy.
6	66	2 o	65	60	30,05	63		S	1	Hazy.
	49	7 o	51,5	59	30,09	79	0,058	NNE	1	Cloudy.
7	64	2 o	62	60	30,12	67		NE	1	Cloudy.
	52	7 o	53	60	30,13	77		SW	1	Fair.
8	72	2 o	70	61	30,07	65		WSW	1	Cloudy.
	57	7 o	58	61,5	29,96	80		SSW	1	Cloudy.
9	68,5	2 o	67	63	29,90	65		WSW	1	Cloudy.
	53	7 o	54,5	61,5	29,80	72		WSW	1	Cloudy.
10	68	2 o	67	62	29,87	62		NW	1	Cloudy.
	48	7 o	51	62	30,10	74		E	1	Fair.
11	68	2 o	67	63	30,00	66		S	2	Fair.
	50	7 o	55	62	29,81	76		SW	1	Hazy.
12	73	2 o	72	63	29,77	67		SSW	1	Hazy.
	51	7 o	57	63	29,77	73		WNW	1	Cloudy.
13	62	2 o	61	63,5	29,86	73		SW	2	Cloudy.
	56	7 o	58	63	30,03	73		SW	2	Cloudy.
14	63	2 o	63	63	30,04	71		SW b.S	2	Cloudy.
	53	7 o	55	62	29,98	71		SSW	2	Cloudy.
15	63	2 o	63	62,5	29,92	69		SSW	2	Cloudy.
	45	7 o	49	61	30,04	74	0,035	WSW	2	Fine.
16	64	2 o	63	62	30,04	61		WSW	2	Fair.
	46	7 o	52	61	30,16	73		SW	2	Fair.
	62	2 o	58	61,5	30,08	79		SSW	2	Cloudy.

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		H.	M.	o.	o.	Inches.		Inches.	Points.	Str.	
June 17	o										
	51	7	o	55	61	30,21	80	0,033	SW	1	Cloudy.
18	67	2	o	66	63	30,17	72	0,056	SSW	1	Cloudy.
	58	7	o	59	62	30,22	77		SW	1	Cloudy.
19	70	2	o	69	63	30,24	64	0,024	WSW	1	Cloudy.
	52	7	o	56	62	30,11	67		E	1	Hazy.
20	72	2	o	72	63,5	29,91	62	0,026	SSE	1	Cloudy.
	53	7	o	55	63	29,67	73		W	1	Cloudy.
21	63	2	o	61	63	29,60	63	0,024	WSW	1	Cloudy.
	46	7	o	53	61	29,77	71		WNW	2	Cloudy.
22	64	2	o	63	63	29,83	63	0,026	WNW	2	Cloudy.
	50	7	o	53	61	29,64	83		S	1	Rain.
23	66	2	o	63	62	29,64	71	0,024	W	1	Cloudy.
	53	7	o	56	62	29,87	74		W	1	Cloudy.
24	68	2	o	67	63	29,96	63	0,024	NW	1	Fair.
	54	7	o	58	63	30,16	74		NW	1	Fair.
25	73	2	o	72	65	30,22	61	0,024	N	1	Fine.
	54	7	o	58	64	30,25	72		SSW	1	Fine.
26	75	2	o	73,5	66	30,19	64	0,024	S b. E	1	Fine.
	54	7	o	59	65	29,96	72		ESE	1	Fine.
27	80	2	o	78	68,5	29,83	65	0,024	S	2	Fine.
	56	7	o	57	66	30,02	65		N	2	Cloudy.
28	65	2	o	61	66	30,03	63	0,024	NE	1	Cloudy.
	53	7	o	57	65	30,09	67		NE	1	Cloudy.
29	65	2	o	61	64	30,09	64	0,024	E	1	Cloudy.
	53	7	o	57	64	30,22	68		E	1	Fair.
30	68	2	o	67	65	30,23	60	0,024	NE	1	Fair.
	49	7	o	53	64	30,31	72		E	1	Fine.
	73	2	o	72	66	30,22	59		1	Fine.	

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1796	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
	H. M.	o	o	Inches.	Inches.	Points.	Str.			
	o									
July 1	53	7 o	57	64	30,18	69		E	1	Cloudy.
	73	2 o	73	66	30,15	67		E	1	Fine.
2	56	7 o	61	64,5	30,04	74		E	1	Cloudy.
	70,5	2 o	68	66	30,00	69		NW	1	Cloudy.
3	53	7 o	56	65	29,81	73	0,031	NW	1	Fair.
	65	2 o	63	66	29,80	65		NE	1	Cloudy.
4	49	7 o	54	64	29,81	71		N	1	Cloudy.
	66	2 o	65	65	29,80	61		W	1	Cloudy.
5	55,5	7 o	58	64	29,52	81		SSW	2	Cloudy.
	68	2 o	67	65	29,40	65		SW	2	Fair.
6	51	7 o	55	64	29,39	76	0,281	SSW	2	Cloudy.
	65	2 o	60,5	64	29,37	67		SSW	2	Cloudy.
7	48	7 o	52	63	29,78	72	0,075	SW	2	Cloudy.
	65	2 o	64	64	29,83	61		W	2	Fair.
8	47,5	7 o	52	62	29,82	74	0,131	SSW	2	Cloudy.
	61	2 o	56	62	29,75	73		SW	2	Cloudy.
9	44,5	7 o	50	61	29,94	77	0,218	SW	2	Fine.
	64	2 o	61	62	29,94	73		SW	2	Cloudy.
10	54	7 o	56	61	29,65	86	0,350	SW	2	Rain.
	62	2 o	61,5	62	29,71	68		SW	2	Cloudy.
11	49	7 o	52	61	29,83	75		WNW	2	Fine.
	65	2 o	64	62	29,82	64		WNW	2	Cloudy.
12	46,5	7 o	51	60	29,94	76	0,167	WNW	1	Fine.
	68,5	2 o	67,5	62	29,96	61		WNW	2	Fair.
13	55	7 o	57	61	29,74	81		SW	1	Cloudy.
	72	2 o	71	63	29,78	63		SW	1	Hazy.
14	58	7 o	62	62	29,81	82	0,018	SSW	2	Cloudy.
	75	2 o	74,5	64	29,87	66		SW	2	Fair.
15	60	7 o	63	64	29,94	76		S	2	Fair.
	77,5	2 o	76,5	66	29,90	65		SSE	2	Fine.
16	63	7 o	63,5	67	29,62	74		S	2	Fair.
	72	2 o	71	67	29,62	67		S	2	Cloudy.

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1796	Six's. Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hy- gro- me- ter.	Rain.	Winds.		Weather.
		H.	M.	o	o	Inches.	Inches.	Points.	Str.		
	o										
July 17	56	7	o	58	66	29,75	73		SSW	2	Fine.
	69	2	o	67	67	29,81	65		SSW	2	Fair.
18	54	7	o	58	63	29,92	77		S	2	Cloudy.
	70	2	o	68	65	29,87	63		SSE	2	Cloudy.
19	56	7	o	56	65	29,65	79	0,173	S	2	Cloudy.
	63	2	o	63	65	29,78	70		SW	2	Cloudy.
20	50	7	o	54	64	30,11	77		WSW	1	Fine.
	70	2	o	68,5	65	30,11	64		SW	1	Cloudy.
21	54	7	o	59	64	29,87	75		SSW	1	Cloudy.
	70	2	o	69	65	29,88	61		SW	2	Fair.
22	53	7	o	55	64	29,85	73		SW	1	Fair.
	67	2	o	66	65	29,81	62		SW	1	Fair.
23	52	7	o	55	64	29,72	74	0,030	SW	1	Fine.
	68	2	o	66	64	29,71	64		WNW	1	Cloudy.
24	53	7	o	56	63	29,54	81	0,186	SW	2	Rain.
	72	2	o	69	65	29,59	65		W	2	Fair.
25	58	7	o	60	64	29,56	80	0,025	W	2	Fair.
	71	2	o	71	66,5	29,56	66		SSW	2	Fair.
26	57	7	o	58	65	29,55	77	0,031	S	2	Fair.
	67	2	o	66	65	29,54	67		S	2	Fair.
27	55	7	o	57	64	29,67	76	0,030	S	1	Fair.
	69	2	o	67	66	29,67	64		SSW	1	Cloudy.
28	53	7	o	56	64	29,84	80	0,080	SSW	2	Fine.
	66	2	o	62	64	29,85	74		S	2	Cloudy.
29	57,5	7	o	58	64	29,77	80	0,078	SSW	2	Cloudy.
	73,5	2	o	72	66	29,84	67		SW	2	Fair.
30	56	7	o	59	65	29,93	75		NE	1	Hazy.
	71	2	o	71	66	29,93	66		E	1	Hazy.
31	56	7	o	58	65	29,93	82		ESE	1	Cloudy.
	74	2	o	73	66	29,91	67		SSE	2	Fair.

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1796	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hy- gro- me- ter.	Rain.	Winds.		Weather.
		H.	M.	o	o	Inches.	Inches.	Points.	Str.		
Aug. 1	o										
	60	7	o	62	66	29,79	83		SSW	1	Cloudy.
	68	2	o	64	66	29,78	81		SSW	1	Rain.
2	60,5	7	o	62	66	29,80	78	0,093	SSW	2	Cloudy.
	68	2	o	66	66	29,80	75		SSW	2	Cloudy.
3	56,5	7	o	56,5	65,5	29,73	82	0,210	W	1	Cloudy.
	66	2	o	64	66	29,86	68		NW	1	Cloudy.
4	48,5	7	o	52	64,5	30,01	77		SW	1	Fine.
	66,5	2	o	65	64	30,02	63		SW	1	Cloudy.
5	51	7	o	53	64	30,14	78		SW	1	Fine.
	69,5	2	o	68	66	30,11	64		SW	2	Fair.
6	54	7	o	58	64	30,06	76		SW	2	Cloudy.
	69	2	o	66	65	30,01	70		SW	2	Cloudy.
7	50	7	o	53	64	30,22	76		WNW	1	Fine.
	72	2	o	71	65	30,19	62		WNW	1	Fine.
8	52	7	o	57	64	30,12	73		SW	1	Fine.
	74	2	o	74	66	30,01	63		S	1	Fine.
9	56	7	o	58	65	29,93	75		SW	1	Cloudy.
	73	2	o	72	67	29,85	68		S	1	Fine.
10	58	7	o	60	66	29,71	79		W	1	Cloudy.
	72	2	o	71	68	29,82	72		WNW	1	Fair.
11	52	7	o	56	66	30,14	76		WSW	1	Hazy.
	71	2	o	71	67	30,14	62		NE	1	Hazy.
12	52	7	o	58	66	30,08	76		E	1	Hazy.
	72	2	o	72	68	30,03	63		E	1	Fine.
13	55,5	7	o	57,5	66	30,15	75		ENE	1	Hazy.
	77	2	o	74	68	30,17	62		NE	1	Fine.
14	57	7	o	62	67	30,20	75		NE	1	Cloudy.
	77	2	o	77	68	30,19	64		E	1	Cloudy.
15	58	7	o	61	68	30,29	74		ENE	1	Fine.
	73	2	o	72	70	30,31	69		E	1	Fine.
16	52	7	o	57	67,5	30,41	78		NE	1	Fair.
	71	2	o	71	69	30,39	62		E	1	Fine.

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		H.	M.	o	o	Inches.		Inches.	Points.	Str.	
	o										
Aug. 17	49	7	o	55	67	30,37	72		NE	1	Cloudy.
	68,5	2	o	68	68	30,32	63		E	1	Fine.
18	56	7	o	59	67	30,22	67		NE	1	Cloudy.
	70	2	o	69	69	30,15	64		E	1	Fine.
19	53	7	o	58	68	30,10	80		NE	1	Cloudy.
	71	2	o	71	69	30,06	65		E	1	Fine.
20	55	7	o	60	68	30,05	83		E	1	Cloudy.
	74	2	o	73	70	30,02	59		E	1	Fine.
21	57	7	o	60	69	30,00	80		E	1	Hazy.
	78	2	o	78	72	30,00	63		E	1	Fair.
22	58	7	o	63	70	30,10	72		E	1	Fair.
	80	2	o	80	72	30,11	59		NE	1	Fine.
23	56,5	7	o	59	70	30,17	80		NE	1	Cloudy.
	74,5	2	o	74	71,5	30,14	67		NE	1	Fine.
24	56	7	o	58	70	30,14	60		ENE	1	Cloudy.
	76	2	o	74	72	30,09	65		E	1	Fine.
25	57	7	o	58	68	30,08	77		NE	1	Cloudy.
	74,5	2	o	73	71	30,03	65		NE	1	Fair.
26	56	7	o	58	68	30,03	79		SW	1	Cloudy.
	73	2	o	73	70	29,98	67		WSW	1	Cloudy.
27	54	7	o	57	69	29,90	77	0,098	W	1	Fair.
	65	2	o	63	69	29,98	64		NW	1	Fair.
28	48	7	o	53	68	30,14	75		SW	1	Fine.
	65	2	o	64	67	30,11	62		WNW	2	Fair.
29	51,5	7	o	55	67	29,98	77		NW	2	Cloudy.
	64	2	o	61	68	29,98	73		NW	2	Cloudy.
30	52,5	7	o	56	65	30,04	78	0,071	NE	2	Cloudy.
	64	2	o	60	66	30,04	80		NNE	1	Cloudy.
31	54	7	o	56	66	29,98	80	0,057	NE	1	Cloudy.
	61	2	o	61	66	29,97	83		N	1	Cloudy.

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	o	H. M.	o	o	Inches.	Inches.	Points.	Str.		
Sept. 1	o									
	53,5	7 0	54	65	29,97	79	0,038	N	1	Cloudy.
2	63	2 0	62	65	29,92	65		N	1	Cloudy.
	51	7 0	53	64	29,97	77		NE	1	Fine.
3	62	2 0	62	64	30,01	65		N	1	Cloudy.
	50	7 0	53	64	30,08	79		NE	1	Cloudy.
4	65,5	2 0	65	64,5	30,03	65		W	1	Fair.
	53	7 0	56	63	29,88	85		W	1	Fair.
5	67	2 0	66	64	29,88	82		WSW	1	Cloudy.
	53	7 0	56	64	30,02	75	0,169	WSW	1	Fair.
6	65	2 0	64	64	30,06	69		W	1	Cloudy.
	52	7 0	56	63,5	30,05	81		SW	1	Cloudy.
7	68	2 0	68	65	29,96	68		SW	1	Fair.
	59	7 0	61	65	29,79	73		SW	2	Cloudy.
8	68,5	2 0	67	65,5	29,82	77		W	2	Cloudy.
	52,5	7 0	56	65	29,95	82		SW	1	Fine.
9	72	2 0	71	65	29,99	66		WNW	1	Cloudy.
	57	7 0	59	66	30,05	80		SW	1	Fair.
10	72,5	2 0	71,5	67	30,06	69		S	1	Cloudy.
	59	7 0	60	65,5	30,01	78		NE	1	Fair.
11	76	2 0	75	67	30,01	67		S	1	Fair.
	61	7 0	63	67	30,06	81		S	1	Cloudy.
12	73	2 0	73	68	30,06	72		SW	1	Fair.
	53	7 0	55	67	30,21	81		SSW	1	Cloudy.
13	71	2 0	70	67,5	30,22	73		S	1	Fair.
	53	7 0	56	66,5	30,16	81		SW	1	Cloudy.
14	74	2 0	73,5	68	30,09	67		S	2	Fine.
	55	7 0	57	67	30,12	81		SSW	1	Cloudy.
15	74,5	2 0	74	68	30,08	69		SW	1	Fine.
	61	7 0	63	68	30,00	80		SW	2	Fair.
16	74	2 0	74	70	30,01	67		SW	2	Fair.
	61,5	7 0	62	68,5	30,10	80		SSW	2	Fair.
	71,5	2 0	71	71	30,10	68		SSW	2	Fine.

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		H.	M.	o	o			Inches.	Points.	Str.	
Sept. 17	o										
	56	7	o	58	69	30,04	81		SSW	1	Cloudy.
18	79	2	o	78	71	30,01	72		S	1	Fine.
	58	7	o	61	70	29,94	79		SW	1	Fine.
19	75,5	2	o	73	72	29,91	68		WNW	1	Fine.
	60	7	o	61	70,5	29,83	85	0,380	ENE	1	Cloudy.
20	62	2	o	62	70	29,78	83		NE	1	Rain.
	60	7	o	62	69	29,57	85	0,062	SE	1	Cloudy.
21	70	2	o	68	70	29,49	77		ESE	2	Cloudy.
	59	7	o	59	69	29,46	83	0,316	S	1	Rain.
22	68	2	o	67	69	29,57	73		S	1	Fair.
	52	7	o	55	68	29,72	82	0,122			Foggy.
23	64	2	o	63	68	29,73	76		E	1	Fair.
	52	7	o	52	66,5	29,86	81	0,116	NE	1	Cloudy.
24	59	2	o	58	66	29,87	75		NE	1	Cloudy.
	51	7	o	52	65	29,92	78	0,018	NE	1	Cloudy.
25	61	2	o	61	65	29,92	77		NE	1	Cloudy.
	50	7	o	56	64	29,85	85		NE	1	Cloudy.
26	61	2	o	57	64	29,79	84		NE	1	Rain.
	54,5	7	o	56	64	29,81	88	0,320	NE	1	Cloudy.
27	59	2	o	58	64	29,86	86		NE	1	Cloudy.
	54	7	o	54	63	30,01	85		NE	1	Cloudy.
28	61	2	o	61	64	30,05	73		NE	1	Fair.
	52	7	o	54,5	63	30,08	81		NE	1	Cloudy.
29	61	2	o	60	63	30,10	80		NE	1	Cloudy.
	49	7	o	50	62	30,14	80		NE	1	Fine.
30	60	2	o	59	63,5	30,18	69		NE	1	Cloudy.
	45	7	o	46	61	30,28	78		NE	1	Fine.
	57	2	o	56	62	30,28	66		NE	1	Fair.

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		H.	M.	o	o	Inches.		Inches.	Points.	Str.	
	o										
Oct. 1	40	7	o	42	60	30,21	77		WSW	1	Cloudy.
	56	2	o	55	60	30,17	72		NW	1	Cloudy.
2	49,5	7	o	50	60	30,22	77		W	1	Cloudy.
	59	2	o	59	60	30,28	71		WNW	1	Cloudy.
3	50	7	o	53	59,5	30,29	75		W	1	Cloudy.
	58	2	o	58	60	30,32	69		WNW	1	Cloudy.
4	53	7	o	53	59	30,24	73		W	1	Cloudy.
	57	2	o	57	59,5	30,17	70		W	1	Cloudy.
5	53	7	o	53	59	29,82	72		SSW	2	Fair.
	58	2	o	56	59	29,71	75		SSW	2	Cloudy.
6	51	7	o	52	59	29,40	85	0,210	S	1	Cloudy.
	58	2	o	57	59,5	29,44	69		S	2	Cloudy.
7	45	7	o	46	58	29,38	76	0,061	S	2	Fair.
	53	2	o	52	58	29,31	78		SSE	2	Fair.
8	42	7	o	43	57	29,48	81	0,092	SSW	1	Fair.
	55	2	o	54	58	29,50	68		W	1	Fair.
9	48	7	o	50	57	29,38	86	0,220	S	1	Rain.
	57,5	2	o	56	58	29,17	85		WSW	1	Rain.
10	43,5	7	o	45	56	29,65	73	0,095	W	2	Fine.
	54,5	2	o	54	57	29,70	65		WNW	2	Fair.
11	42	7	o	43	56	29,50	80		WNW	2	Fine.
	53	2	o	48	56	29,38	76		WNW	2	Fair.
12	38,5	7	o	40	54,5	29,48	78	0,036	W	1	Fine.
	55	2	o	54	56,5	29,49	69		W	2	Fine.
13	46	7	o	46	55	29,42	80	0,120	S	1	Rain.
	51	2	o	51	57	29,46	74		NW	1	Fair.
14	42	7	o	43	56	29,67	81	0,110	SW	1	Fine.
	55	2	o	55	59	29,64	76		S b. E	1	Cloudy.
15	45	7	o	46,5	57	29,61	85	0,530	W	1	Cloudy.
	49	2	o	48,5	58	29,68	82		NE	1	Cloudy.
16	39	7	o	40	56	29,95	84		NE	1	Fine.
	51	2	o	50,5	59	30,02	74		NE	1	Fair.

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		H.	M.	o	o	Inches.	Inches.	Points.	Str.		
	o										
Oct. 17	38	7	o	39	56	30,18	85		ENE	1	Fair.
	53	2	o	53	59	30,05	76		NE	1	Fair.
18	44	7	o	46	57	29,73	83	0,080	NE	1	Rain.
	50,5	2	o	50	58	29,72	76		NW	1	Cloudy.
19	41	7	o	45	56	29,85	83	0,120	W	1	Cloudy.
	51	2	o	51	58	30,01	73		N	1	Cloudy.
20	46,5	7	o	46,5	57	30,24	79		WNW	1	Cloudy.
	55	2	o	54,5	59	30,26	75		NNW	1	Cloudy.
21	50	7	o	50	58	30,27	84		WNW	1	Cloudy.
	54	2	o	54	60	30,25	79		SW	1	Cloudy.
22	50	7	o	50	59	30,21	85		SW	1	Cloudy.
	56	2	o	55	60	30,16	78		SW	1	Cloudy.
23	51	7	o	51	59	30,18	80		SW	1	Cloudy.
	56	2	o	54	61	30,03	82		WSW	1	Cloudy.
24	36,5	7	o	38	58	30,16	77	0,031	W	1	Fair.
	46	2	o	45	59	30,26	69		NW	1	Fair.
25	30	7	o	32	56	30,50	75		NW	1	Fair.
	44,5	2	o	44	57	30,55	66		NW	1	Fine.
26	35	7	o	36	55	30,55	80		NE	1	Fine.
	49,5	2	o	48	57	30,47	80		NE	1	Cloudy.
27	41	7	o	41	55	30,38	84	0,098	NE	1	Fine.
	50	2	o	49	58	30,36	71		NE	1	Fair.
28	46	7	o	46	56	30,21	80		NE	1	Cloudy.
	52	2	o	50	58	30,14	77		NE	1	Cloudy.
29	46	7	o	46	57	30,08	82		E	1	Cloudy.
	50	2	o	50	58	30,05	79		E	1	Cloudy.
30	47	7	o	47	57	29,98	83		E	1	Cloudy.
	49	2	o	49	58	29,98	79		NE	1	Cloudy.
31	45	7	o	46	56	30,08	79		W	1	Cloudy.
	52,5	2	o	52,5	58	30,11	72		NW	1	Cloudy.

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		H	M.	°	°	Inches.		Inches.	Points.	Str.	
	o										
Nov. 1	45	7	o	47	56	30,19	82		W	1	Cloudy.
	56	2	o	56	58	30,13	80		WSW	1	Cloudy.
2	52	7	o	52	57	30,00	84		SW	1	Cloudy.
	57	2	o	57	59	29,93	80		WSW	1	Cloudy.
3	52	7	o	52	58	29,78	85		WNW	1	Cloudy.
	54	2	o	54	60	29,75	68		WNW	1	Cloudy.
4	42	7	o	42,5	57	29,57	80		W	1	Fine.
	49	2	o	45	56	29,55	76		NW	1	Fine.
5	39	7	o	42	56	29,86	79		NNW	1	Cloudy.
	45	2	o	45	58	30,03	69		NNE	1	Fine.
6	30	7	o	30	55	30,12	76		SW	1	Fine.
	47	2	o	47	57	29,96	74		SW	1	Fair.
7	40	7	o	42	54	29,69	85		SW	1	Cloudy.
	49,5	2	o	49,5	57	29,60	74		S	1	Hazy.
8	37	7	o	38	55	29,50	81		W	1	Cloudy.
	47	2	o	47	57	29,53	77		NE	1	Fair.
9	41	7	o	41	55	29,78	84		NE	1	Cloudy.
	47,5	2	o	47	56	29,99	80		NE	1	Cloudy.
10	42	7	o	43	54	30,00	84		NE	1	Cloudy.
	45	2	o	45	56	29,98	80		ENE	1	Cloudy.
11	37	7	o	37	54	29,96	82		E	1	Cloudy.
	44	2	o	44	55	29,94	79		E	1	Cloudy.
12	39	7	o	39	53	29,84	83		NE	1	Cloudy.
	46	2	o	46	54	29,77	82		NE	1	Cloudy.
13	39	7	o	39	53	29,68	81		NE	1	Cloudy.
	44	2	o	44	53	29,71	73		NE	1	Fair.
14	38	7	o	38	52	29,74	75		E	1	Cloudy.
	40	2	o	40	53	29,70	72		E	1	Cloudy.
15	36	7	o	36	52	29,70	82		W	1	Cloudy.
	41	2	o	41	54	29,84	71		NW	1	Cloudy.
16	33	7	o	34	52	29,91	80		S	1	Cloudy.
	45	2	o	44	52	29,63	82		SSE	2	Rain.

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		H.	M.	o	o	Inches.	Inches.	Points.	Str.		
	o										
Nov. 17	40	7	o	43	52	29,18	83	0,192	SSW	1	Cloudy.
	43	2	o	43	53	29,30	83		WNW	1	Cloudy.
18	33,5	7	o	36	52	29,34	79	0,153	NW	1	Cloudy.
	42	2	o	42	53	29,33	82		NW	1	Cloudy.
19	39	7	o	39	52	29,38	84	0,110	NW	1	Cloudy.
	42,5	2	o	42	53	29,35	82		NE	1	Cloudy.
20	36	7	o	36	52	29,46	85		NE	1	Cloudy.
	42	2	o	42	53	29,51	83		NE	1	Cloudy.
21	31	7	o	32	51,5	29,64	84	0,087		1	Foggy.
	42	2	o	35	53	29,66	85		NE	1	Cloudy.
22	42	7	o	42	52	29,65	88	0,387	NE	1	Rain.
	49	2	o	47,5	53,5	29,69	87		SE	1	Cloudy.
23	44	7	o	44	52	29,83	87	0,280	E	1	Cloudy.
	47	2	o	46	55	29,88	86		E	1	Cloudy.
24	42	7	o	43	53	29,98	85		E	1	Cloudy.
	47	2	o	47	55	30,00	80		NE	1	Cloudy.
25	42	7	o	43	54	30,18	80		E	1	Cloudy.
	43	2	o	43	55	30,25	82		E	1	Cloudy.
26	41	7	o	41	53	30,28	79		NE	1	Cloudy.
	43	2	o	43	55	30,28	76		NE	1	Cloudy.
27	42	7	o	42	53	30,27	80		NE	1	Cloudy.
	44,5	2	o	44	55	30,22	85		NE	1	Cloudy.
28	40	7	o	40	53	30,29	81		NE	1	Cloudy.
	42	2	o	42	54	30,27	77		NE	1	Cloudy.
29	34	7	o	35	52	30,27	78		W	1	Cloudy.
	39	2	o	39	54	30,16	75		WNW	1	Fair.
30	29	7	o	29	50	30,00	74		NW	1	Fine.
	30	2	o	30	48	30,00	68		NW	1	Fine.

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		H.	M.	o	o			luches.	luches.	Points.	
Dec. 1	o										
	24	8	o	24	47	29,98	75		NW	1	Fine.
2	32	2	o	32	49	29,99	73		NW	1	Fine.
	21,5	8	o	21,5	47	30,05	77		W	1	Fine.
3	34	2	o	34	48	30,07	76		SW	1	Fine.
	22	8	o	25	46	29,90	77		W	1	Cloudy.
4	35	2	o	34	47	29,77	78		SW	1	Fair.
	35	8	o	35	47	29,57	84		SW	1	Cloudy.
5	36	2	o	32	47	29,69	78		NW	1	Fair.
	23	8	o	25	44	29,74	81		W	1	Fine.
6	36	2	o	36	47	29,62	76		SW	1	Fine.
	24	8	o	25	44	29,82	80	0,262	NW	1	Fair.
7	35	2	o	32	48	29,99	74		NW	1	Fine.
	23	8	o	24	44	30,13	82		NW	1	Fair.
8	37	2	o	35	45	30,03	80		NW	1	Cloudy.
	26	8	o	28	45	30,00	75		NW	1	Fine.
9	37	2	o	37	47,5	30,04	73		NW	1	Fair.
	27	8	o	26	46	30,33	81		NW	1	Fine.
10	35	2	o	33	48	30,34	78		NW	1	Cloudy.
	23	8	o	24	45	30,51	81		E	1	Cloudy.
11	30	2	o	29	48	30,50	78		SW	1	Fine.
	31	8	o	27	46	30,27	85		SSW	1	Cloudy.
12	34	2	o	34	45	30,30	86		WNVV	1	Cloudy.
	33,5	8	o	34	44	30,34	85	0,034	N	1	Rain.
13	39	2	o	39	48	30,35	85		NNE	1	Cloudy.
	35	8	o	35	46	30,30	87		NE	1	Cloudy.
14	37	2	o	37	49	30,26	85		NE	1	Cloudy.
	33,5	8	o	37	47	30,07	83		NE	1	Cloudy.
15	41	2	o	41	50	30,07	84		NE	1	Cloudy.
	35	8	o	35	48	30,14	83		NE	1	Cloudy.
16	36	2	o	34	50	30,18	84		NE	1	Cloudy.
	33	8	o	33	48	30,16	84		ENE	1	Cloudy.
	35	2	o	35	50	30,20	82		E	1	Cloudy.

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	H. M.	o	o	Inches.	Inches.	Points.	Str.			
	o									
Dec. 17	31	8 o	31	48	30,15	80		E	1	Cloudy.
	32	2 o	32	50	30,05	80		E	1	Cloudy.
18	30	8 o	32	48	29,61	85	0,138	E	1	Rain.
	35	2 o	35	50	29,42	86		E	1	Rain.
19	35	8 o	38	48	29,28	88	0,435			Foggy.
	48	2 o	46,5	53	29,24	89		SSW	1	Cloudy.
20	40	8 o	40	50	29,27	89	0,096	SW	1	Rain.
	42	2 o	39	53	29,41	84		NE	1	Cloudy.
21	28	8 o	28	49	29,61	85		SW	1	Fine.
	33	2 o	33	51	29,65	82		SSW	1	Cloudy.
22	28	8 o	28	49	29,58	80		NE	1	Cloudy.
	31	2 o	31	50	29,61	74		NE	1	Fair.
23	25	8 o	26	48	29,29	87		NE	1	Snow.
	32	2 o	32	50	29,36	83		NE	1	Cloudy.
24	19	8 o	20	47	29,63	80		N	1	Fair.
	23	2 o	23	49	29,68	76		N	1	Fair.
25	4	8 o	5	43	29,73	80				Foggy.
	23	2 o	16	46	29,72	80		NE	1	Fair.
26	16	8 o	23	43	29,62	82		E	2	Fair.
	29	2 o	29	45	29,59	79		E	2	Cloudy.
27	26,5	8 o	26,5	43	29,57	80		E	2	Cloudy.
	29	2 o	29	47	29,63	80		E b. S	2	Cloudy.
28	26,5	8 o	30	43	29,40	85	0,200	E b. S	2	Rain.
	37	2 o	35	47	29,44	88		E	1	Cloudy.
29	35	8 o	36	45	29,47	90	0,068			Foggy.
	45	2 o	45	48	29,47	90		SE	1	Cloudy.
30	44	8 o	46	48	29,54	90	0,041	S b. W	2	Rain.
	51,5	2 o	49	52	29,54	84		S b. W	2	Rain.
31	46	8 o	46	51	29,53	83	0,035	S	2	Cloudy.
	48	2 o	48	54	29,53	86		S	2	Cloudy.

1796.	Six's Therm. without.			Thermometer without.			Thermometer within.			Barometer.*			Hygrometer.			Rain.	
	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Deg.	Inches.
January	56	36	47.3	55	38	47.5	62	51	57.2	30.32	29.00	29.72	86	73	79.3	2.128	
February	56	30	41.7	55.5	30.5	41.7	58.5	51	55.0	30.31	29.05	29.81	86	66	76.3	1.143	
March	60	26.5	41.0	59	27	41.4	60	47	54.0	30.44	29.50	30.03	84	58	70.7	0.074	
April	70	36	50.9	68.5	39	51.4	64.5	55	59.8	30.32	29.08	30.04	82	59	70.4	0.302	
May	65	39	52.7	64	44	54.0	63	57	60.4	30.22	28.94	29.73	85	63	71.4	2.301	
June	80	45	58.8	78	49	59.8	68.5	59	62.2	30.31	29.44	29.96	83	59	69.7	0.536	
July	77.5	44.5	61.2	76.5	50	62.0	67	60	64.1	30.18	29.37	29.79	86	61	71.2	1.904	
August	80	48.5	62.5	80	52	63.7	72	64	67.2	30.41	29.71	30.06	83	59	71.5	0.529	
September	79.8	45	61.9	78	46	61.4	72	61	66.1	30.28	29.46	29.96	88	65	75.1	1.541	
October	59	30	48.7	59	32	48.9	61	54.5	57.8	30.55	29.17	29.94	86	65	77.2	1.803	
November	57	29	42.2	57	29	42.2	60	50	54.3	30.29	29.18	29.83	88	68	80.0	1.209	
December	51.5	4	32.1	49	5	32.1	53	43	47.5	30.51	29.24	29.83	90	73	81.9	1.309	
Whole year			50.1			50.5			58.8			29.89			74.6	14.779	

* The quicksilver in the basin of the barometer is 81 feet above the level of low water spring tides at Somerset-house.

PHILOSOPHICAL
TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCXCVII.

PART II.



LONDON,

SOLD BY PETER ELMSLY,
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MDCCXCVII.

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PHILOSOPHICAL
TRANSACTIONS.

XI. *On the Action of Nitre upon Gold and Platina.* By
Smithson Tennant, Esq. F. R. S.

Read March 23, 1797.

GOLD, which cannot be calcined by exposure to heat and air, has been also considered as incapable of being affected by nitre. But in the course of some experiments on the diamond, an account of which has been communicated to this Society, I observed, that when nitre was heated in a tube of gold, and the diamond was not in sufficient quantity to supply the alkali of the nitre with fixed air, a part of the gold was dissolved. From this observation I was induced to examine more particularly the action of nitre upon gold, as well as to inquire whether it would produce any effect upon silver and platina.

With this intention I put some thin pieces of gold into the tube together with nitre, and exposed them to a strong red heat for two or three hours. After the tube was taken from the fire the part of the nitre which remained, consisting of caustic alkali, and of nitre partially decomposed, weighed

140 grains; and 60 grains of the gold were found to have been dissolved. Upon the addition of water about 50 grains of the gold were precipitated, in the form of a black powder. The gold which was thus precipitated was principally in its metallic state, the greater portion of it being insoluble in marine acid. The remaining gold, about 10 grains in weight, communicated to the alkaline solution, in which it was retained, a light yellow colour. By dropping into this solution diluted vitriolic or nitrous acid, it became at first of a deeper yellow, but if viewed by the transmitted light, it soon appeared green, and afterwards blue. This alteration of the colour from yellow to blue arises from the gradual precipitation of the gold in its metallic form, which by the transmitted light is of a blue colour. Though the gold is precipitated from this solution in its metallic form, yet there seems to be no doubt that while it remains dissolved it is entirely in the state of calx. Its precipitation in the metallic state is occasioned by the nitre contained in the solution, which having lost part of its oxygen by heat, appears to be capable of attracting it from the calx of gold; for I found that if the calx of gold is dissolved by being boiled in caustic alkali, and a sufficient quantity of nitre which has lost some of its air by heat is mixed with it, the gold is precipitated by an acid in its metallic state.*

* As the precipitation of gold in its metallic form, by nitre which has lost some of its oxygen has not, I believe, been noticed, it may not be improper to mention some of those facts relating to it which seem most entitled to attention. Nitre which has been heated some time precipitates gold in its metallic state from a solution in aqua regia, if it is diluted with water. If a solution of gold in nitrous acid is dropped into pure water, the calx of gold is separated, which is of a yellow colour; but if the water contains a very small proportion of nitre which has lost some of its air by heat (as one grain in six ounces), the gold is deprived of its oxygen, and becomes blue. The

Having found that nitre would dissolve gold, I tried whether it would produce any effect upon platina.

It has been formerly observed that the grains of platina, in the impure state in which it is originally found, might, by being long heated in a crucible with nitre, be reduced to powder. LEWIS, from his own experiments and those of MARGRAAF, thought that the iron only which is contained in the grains of platina was corroded by the nitre. But by heating nitre with some thin pieces of pure platina in a cup of the same metal, I found that the platina was easily dissolved, the cup being much corroded, and the thin pieces entirely destroyed. By dissolving the saline matter in water, the greater part of the platina was precipitated in the form of a brown powder. This powder, which was entirely soluble in marine acid, consisted of the calx of platina, combined with a portion of alkali, which could not be separated by being boiled in water. The platina which was retained by the alkaline solution communicated to it a brown-yellow colour. By adding an acid to it a precipitate was formed, which consisted of the calx of platina, of alkali, and of the acid which was employed.

Silver, I found to be a little corroded by nitre. But as its action upon that metal was very inconsiderable, it did not appear to be deserving of a more particular examination.

alkali of the nitre does not assist in producing this effect. Nitrous acid alone, which does not contain its full proportion of oxygen, occasions the same precipitation, unless it is very strong; and if a mixture of such strong nitrous acid, and of a solution of gold in nitrous acid, is dropped into water, the gold is deprived of its oxygen, and is precipitated of a blue colour. Two causes contribute to produce this effect upon the addition of water. The adhesion of the calx of gold to nitrous acid is by that means weakened, and the oxygen is attracted more strongly to the imperfect nitrous acid, in consequence of their attraction for water when they are united.

XII. *Experiments to determine the Force of fired Gunpowder.*
 By Benjamin Count of Rumford, F. R. S. M. R. I. A.

Read May 4, 1797.

NO human invention of which we have any authentic records, except, perhaps, the art of printing, has produced such important changes in civil society as the invention of gunpowder. Yet, notwithstanding the uses to which this wonderful agent is applied are so extensive, and though its operations are as surprising as they are important, it seems not to have hitherto been examined with that care and perseverance which it deserves. The explosion of gunpowder is certainly one of the most surprising phænomena we are acquainted with, and I am persuaded it would much oftener have been the subject of the investigations of speculative philosophers, as well as of professional men, in this age of inquiry, were it not for the danger attending the experiments: but the force of gunpowder is so great, and its effects so sudden and so terrible, that, notwithstanding all the precautions possible, there is ever a considerable degree of danger attending the management of it, as I have more than once found to my cost.

Several eminent philosophers and mathematicians, it is true, have, from time to time, employed their attention upon this curious subject; and the modern improvements in chemistry have given us a considerable insight into the cause, and the

nature of the explosion which takes place in the inflammation of gunpowder; and the nature and properties of the elastic fluids generated in its combustion. But the great desideratum, the real measure of the initial expansive force of inflamed gunpowder, so far from being known, has hitherto been rather guessed at than determined; and no argument can be more convincing to show our total ignorance upon that subject, than the difference in the opinions of the greatest mathematicians of the age, who have undertaken its investigation.

The ingenious Mr. ROBINS, who made a great number of very curious experiments upon gunpowder, and who, I believe, has done more towards perfecting the art of gunnery than any other individual, concluded, as the result of all his inquiries and computations, that the force of the elastic fluid generated in the combustion of gunpowder is 1000 times greater than the mean pressure of the atmosphere. But the celebrated mathematician DANIEL BERNOULLI determines its force to be not less than 10,000 times that pressure, or ten times greater than Mr. ROBINS made it.

Struck with this great difference in the results of the computations of these two able mathematicians, as well as with the subject itself, which appeared to me to be both curious and important, I many years ago set about making experiments upon gunpowder, with a view principally of determining the point in question, namely, *its initial expansive force* when fired; and I have ever since, occasionally, from time to time, as I have found leisure and convenient opportunities, continued these inquiries.

In a paper printed in the year 1781, in the LXXI. Volume of the Philosophical Transactions, I gave an account of an

experiment (No. 92.) by which it appeared that, calculating even upon Mr. ROBINS'S own principles, the force of gunpowder, instead of being 1000 times, must at least be 1308 times greater than the mean pressure of the atmosphere. However, not only that experiment, but many others, mentioned in the same paper, had given me abundant reason to conclude that the principles assumed by Mr. ROBINS, in his treatise upon gunnery, were erroneous; and I saw no possibility of ever being able to determine the initial force of gunpowder by the methods he had proposed, and which I had till then followed in my experiments. Unwilling to abandon a pursuit which had already cost me much pains, I came to a resolution to strike out a new road, and to endeavour to ascertain the force of gunpowder by *actual measurement*, in a direct and decisive experiment.

I shall not here give a detail of the numerous difficulties and disappointments I met with in the course of these dangerous pursuits; it will be sufficient briefly to mention the plan of operations I formed, in order to obtain the end I proposed, and to give a cursory view of the train of unsuccessful experiments by which I was at length led to the discovery of the truly astonishing force of gunpowder;—a force at least *fifty thousand* times greater than the mean pressure of the atmosphere!

My first attempts were to fire gunpowder in a confined space, thinking, that when I had accomplished this, I should find means, without much difficulty, to measure its elastic force. To this end, I caused a short gun-barrel to be made, of the best wrought iron, and of uncommon strength; the diameter of its bore was $\frac{3}{4}$ of an inch, its length 5 inches, and the thickness of the metal was equal to the diameter of the bore, so that its external diameter was $2\frac{1}{4}$ inches. It was closed at both

ends, by two long screws, like the breech-pin of a musket; each of which entered 2 inches into the bore, leaving only a vacuity of 1 inch in length for the charge. The powder was introduced into this cavity by taking out one of the screws, or breech-pins; which being afterwards screwed into its place again, and both ends of the barrel closed up, fire was communicated to the powder by a very narrow vent, made in the axis of one of the breech-pins for that purpose. The chamber, which was 1 inch in length, and $\frac{3}{4}$ of an inch in diameter, being about half filled with powder, I expected that when the powder should be fired, the generated elastic fluid being obliged to issue out at so small an opening as the vent, which was no more than $\frac{1}{20}$ of an inch in diameter, instead of giving a smart report, would come out with something like a hissing noise; and I intended, in a future experiment, to confine the generated elastic fluid entirely, by adding a valve to the vent, as I had done in some of my experiments mentioned in my paper published in the LXXI. Volume of the Philosophical Transactions. But when I set fire to the charge (which I took the precaution to do by means of a train), instead of a hissing noise, I was surprised by a very sharp and a very loud report; and, upon examining the barrel, I found the vent augmented to at least four times its former dimensions, and both the screws loosened.

Finding, by the result of this experiment, that I had to do with an agent much more troublesome to manage than I had imagined, I redoubled my precautions. As the barrel was not essentially injured, its ends were now closed up by two new screws, which were firmly fixed in their places by solder, and a new vent was opened in the barrel itself. As both ends of

the barrel were now closed up, it was necessary, in order to introduce the powder into the chamber, to make it pass through the vent, or to convey it through some other aperture made for that purpose. The method I employed was as follows: a hole being made in the barrel, about $\frac{2}{10}$ of an inch in diameter, a plug of steel was screwed into this hole; and it was in the centre or axis of the plug that the vent was made. To introduce the powder into the chamber the plug was taken away. The vent was made conical, its largest diameter being inwards, or opening into the chamber; and a conical pin, of hardened steel, was fitted into it; which pin was intended to serve as a valve for closing up the vent, as soon as the powder in the chamber should be inflamed. To give a passage to the fire through the vent in entering the chamber, this pin was pushed a little inwards, so as to leave a small vacuity between its surface and the concave surface of the bore of the vent. But notwithstanding all possible care was taken in the construction of this instrument, to render it perfect in all its parts, the experiment was as unsuccessful as the former: upon firing the powder in the chamber, (though it did not fill more than half its cavity), the generated elastic fluid not only forced its way through the vent, notwithstanding the valve (which appeared not to have had time to close), but it issued with such an astonishing velocity from this small aperture, that instead of coming out with a hissing noise, it gave a report nearly as sharp and as loud as a common musket. Upon examining the vent-plug and the pin, they were both found to be much corroded and damaged; though I had taken the precaution to harden them both before I made the experiment.

I afterwards repeated the experiment with a simple vent,

made very narrow, and lined with gold to prevent its being corroded by the acid vapour generated in the combustion of the gunpowder; but this vent was found, upon trial, to be as little able to withstand the amazing force of the inflamed gunpowder as the others. It was so much, and so irregularly corroded, by the explosion in the first experiment, as to be rendered quite unserviceable; and what is still more extraordinary, the barrel itself, notwithstanding its amazing strength, was blown out into the form of a cask; and though it was cracked, it was not burst quite asunder, nor did it appear that any of the generated elastic fluid had escaped through the crack. The barrel, in the state it was found after this experiment, is still in my possession.

These unsuccessful attempts, and many others of a similar nature, of which it is not necessary to give a particular account, as they all tended to shew that the force of fired gunpowder is in fact much greater than has generally been imagined, instead of discouraging me from pursuing these inquiries, served only to excite my curiosity still more, and to stimulate me to further exertions.

These researches did not by any means appear to me as being merely speculative; on the contrary, I considered the determination of the real force of the elastic fluid generated in the combustion of gunpowder as a matter of great importance.

The use of gunpowder is become so extensive, that very important mechanical improvements can hardly fail to result from any new discoveries relative to its force, and the law of its action. Most of the computations that have hitherto been made relative to the action of gunpowder, have been founded upon the supposition that the elasticity of the generated fluid is as its

density; but if this supposition should prove false, all those computations, with all the practical rules founded on them, must necessarily be erroneous; and the influence of these errors must be as extensive as the uses to which gunpowder is applied.

Having found by experience how difficult it is to confine the elastic vapour generated in the combustion of gunpowder, when the smallest opening is left by which any part of it can escape, it occurred to me, that I might perhaps succeed better by closing up the powder entirely, in such a manner as to leave no opening whatever, by which it could communicate with the external air; and by setting the powder on fire, by causing the heat employed for that purpose to pass through the solid substance of the iron barrel used for confining it. In order to make this experiment, I caused a new barrel to be constructed for that purpose: its length was 3.45 inches, and the diameter of its bore $\frac{7}{10}$ of an inch; its ends were closed up by two screws, each one inch in length, which were firmly and immoveably fixed in their places by solder; a vacuity being left between them in the barrel 1.45 inch in length, which constituted the chamber of the piece; and whose capacity was nearly $\frac{6}{10}$ of a cubic inch. An hole, 0.37 of an inch in diameter, being bored through both sides of the barrel, through the centre of the chamber, and at right angles to its axis, two tubes of iron, 0.37 of an inch in diameter, the diameter of whose bore was $\frac{1}{10}$ of an inch, were firmly fixed in this hole with solder, in such a manner that while their internal openings were exactly opposite to each other, and on opposite sides of the chamber, the axes of their bores were in the same right line. The shortest of these tubes, which pro-

jected 1.3 inch beyond the external surface of the barrel, was closed at its projecting end, or rather it was not bored quite through its whole length, $\frac{3}{10}$ of an inch of solid metal being left at its end, which was rounded off in the form of a blunt point. The longer tube, which projected 2.7 inches beyond the surface of the barrel on the other side, and which served for introducing the powder into the chamber, was open; but it could occasionally be closed by a strong screw, furnished with a collar of oiled leather, which was provided for that purpose. The method of making use of this instrument was as follows. The barrel being laid down, or held, in a horizontal position, with the long tube upwards, the charge, which was of the very best fine-grained glazed powder, was poured through this tube into the chamber. In doing this, care must be taken that the cavity of the short tube be completely filled with powder, and this can best be done by pouring in only a small quantity of powder at first, and then, by striking the barrel with a hammer, cause the powder to descend into the short tube. When, by introducing a priming-wire through the long tube, it is found that the short tube is full, it ought to be gently pressed together, or rammed down, by means of the priming-wire, in order to prevent its falling back into the chamber upon moving the barrel out of the horizontal position. The short tube being properly filled, the rest of the charge may be introduced into the chamber, and the end of the long tube closed up by its screw.

More effectually to prevent the elastic fluid generated in the combustion of the charge from finding a passage to escape by this opening, after the charge was introduced into the

chamber, the cavity of the long tube was filled up with cold tallow, and the screw that closed up its end (which was $\frac{1}{2}$ an inch long, and but a little more than $\frac{1}{10}$ of an inch in diameter) was pressed down against its leather collar with the utmost force. The manner of setting fire to the charge was as follows: a block of wrought iron, about $1\frac{1}{2}$ inch square, with a hole in it, capable of receiving nearly the whole of that part of the *short tube* which projects beyond the barrel, being heated red hot, the end of the short tube was introduced into this hole, where it was suffered to remain till the heat, having penetrated the tube, set fire to the powder it contained, and the inflammation was *from thence* communicated to the powder in the chamber.

The result of this experiment fully answered my expectations. The generated elastic fluid was so completely confined that no part of it could make its escape. The report of the explosion was so very feeble, as hardly to be audible: indeed it did not by any means deserve the name of a report, and certainly could not have been heard at the distance of twenty paces; it resembled the noise which is occasioned by the breaking of a very small glass tube.

I imagined at first that the powder had not all taken fire, but the heat of the barrel soon convinced me that the explosion must have taken place, and after waiting near half an hour, upon loosening the screw which closed the end of the long vent tube, the confined elastic vapours rushed out with considerable force, and with a noise like that attending the discharge of an air-gun. The quantity of powder made use of in the experiment was indeed very small, not amounting to

more than $\frac{1}{8}$ part of what the chamber was capable of containing; but having so often had my machinery destroyed in experiments of this sort, I began now to be more cautious.

Having found means to confine the elastic vapour generated in the combustion of gunpowder, my next attempts were to measure its force; but here again I met with new and almost insurmountable difficulties. To measure the expansive force of the vapour, it was necessary to bring it to act upon a moveable body of known dimensions, and whose resistance to the efforts of the fluid could be accurately determined; but this was found to be extremely difficult. I attempted it in various ways, but without success. I caused a hole to be bored in the axis of one of the screws, or breech-pins, which closed up the ends of the barrel just described, and fitting a piston of hardened steel into this hole (which was $\frac{2}{10}$ of an inch in diameter), and causing the end of the piston which projected beyond the end of the barrel to act upon a heavy weight, suspended as a pendulum to a long iron rod, I hoped, by knowing the velocity acquired by the weight, from the length of the arc described by it in its ascent, to be able to calculate the pressure of the elastic vapour by which it was put in motion; but this contrivance was not found to answer, nor did any of the various alterations and improvements I afterwards made in the machinery render the results of the experiment at all satisfactory. It was not only found almost impossible to prevent the escape of the elastic fluid by the sides of the piston, but the results of apparently similar experiments were so very different, and so uncertain, that I was often totally at a loss to account for these extraordinary variations. I was however at length led to suspect, what I afterwards found abundant

reason to conclude was the real cause of these variations, and of all the principal difficulties which attended the ascertaining the force of fired gunpowder by the methods I had hitherto pursued.

It has generally been believed, after Mr. ROBINS, that the force of fired gunpowder consists in the action of a permanently elastic fluid, similar in many respects to common atmospheric air; which being generated from the powder in combustion, in great abundance, and being moreover in a very compressed state, and its elasticity being much augmented by the heat (which is likewise generated in the combustion), it escapes with great violence, by every avenue; and produces that loud report, and all those terrible effects, which attend the explosion of gunpowder.

But though this theory is very plausible, and seems upon a cursory view of the subject to account in a satisfactory manner for all the phænomena, yet a more careful examination will shew it to be defective. There is no doubt but the permanently elastic fluids, generated in the combustion of gunpowder, *assist* in producing those effects which result from its explosion; but it will be found, I believe, upon ascertaining the real expansive force of fired gunpowder, that this cause, alone, is quite inadequate to the effects actually produced; and that, therefore, the agency of some other power must necessarily be called in to its assistance.

Mr. ROBINS has shewn, that if all the permanently elastic fluid generated in the combustion of gunpowder be compressed in the space originally occupied by the powder, and if this fluid so compressed be supposed to be heated to the intense heat of red-hot iron, its elastic force *in that case* will be 1000

times greater than the mean pressure of the atmosphere; and this, according to his theory, is the real measure of the force of gunpowder, *fired in a cavity which it exactly fills.*

But what will become of this theory, and of all the suppositions upon which it is founded, if I shall be able to prove, as I hope to do in the most satisfactory manner, that the force of fired gunpowder, instead of being 1000 times, is at least 50,000 greater than the mean pressure of the atmosphere?

For my part, I know of no way of accounting for this enormous force, but by supposing it to arise principally from the elasticity of the *aqueous vapour* generated from the powder in its combustion. The brilliant discoveries of modern chemists have taught us, that both the constituent parts of which water is composed, and even water itself, exist in the materials which are combined to make gunpowder; and there is much reason to believe that water is actually formed, as well as disengaged, in its combustion. M. LAVOISIER, I know, imagined that the force of fired gunpowder, depends in a great measure upon the expansive force of uncombined *caloric*, supposed to be let loose in great abundance during the combustion or deflagration of the powder: but it is not only dangerous to admit the action of an agent whose existence is not yet clearly demonstrated, but it appears to me that this supposition is quite unnecessary; the elastic force of the heated aqueous vapour, whose existence can hardly be doubted, being quite sufficient to account for all the phænomena. It is well known that the elasticity of aqueous vapour is incomparably more augmented by any given augmentation of temperature, than that of any permanently elastic fluid whatever; and those who are acquainted with the amazing force of steam, when heated only to a few degrees

above the boiling point, can easily perceive that its elasticity must be almost infinite when greatly condensed and heated to the temperature of red-hot iron; and this heat it must certainly acquire in the explosion of gunpowder. But if the force of fired gunpowder arises *principally* from the elastic force of heated aqueous vapour, a cannon is nothing more than a *steam-engine* upon a peculiar construction; and upon determining the ratio of the elasticity of this vapour to its density, and to its temperature, a law will be found to obtain, very different from that assumed by Mr. ROBINS, in his Treatise on Gunnery. What this law really is, I do not pretend to have determined with that degree of precision which I wished; but the experiments of which I am about to give an account will, I think, demonstrate in the most satisfactory manner, not only that the force of fired gunpowder is in fact much greater than has been imagined, but also that its force consists principally in the temporary action of a fluid not permanently elastic, and consequently that all the theories hitherto proposed for the elucidation of this subject, must be essentially erroneous.

The first step towards acquiring knowledge is undoubtedly that which leads us to a discovery of the falsehood of received opinions. To a diligent inquirer every common operation, performed in the usual course of practice, is an experiment, from which he endeavours to discover some new fact, or to confirm the result of former inquiries.

Having been engaged many years in the investigation of the force of gunpowder, I occasionally found many opportunities of observing, under a variety of circumstances, the various effects produced by its explosion; and as a long habit of meditating upon this subject rendered every thing relating

to it highly interesting to me ; I seized these opportunities with avidity, and examined all the various phænomena with steady and indefatigable attention.

During a cruise which I made as a volunteer in the *Victory*, with the British fleet, under the command of my late worthy friend Sir CHARLES HARDY, in the year 1779, I had many opportunities of attending to the firing of heavy cannon : for though we were not fortunate enough to come to a general action with the enemy, as is well known, yet, as the men were frequently exercised at the great guns, and in firing at marks, and as some of my friends in the fleet, then captains, (since made admirals) as the Honourable KEITH STEWART, who commanded the *Berwick* of 74 guns—Sir CHARLES DOUGLAS, who commanded the *Duke* of 98 guns—and Admiral MACBRIDE, who was then captain of the *Bienfaisant* of 64 guns, were kind enough, at my request, to make a number of experiments, and particularly by firing a greater number of bullets at once from their heavy guns than ever had been done before, and observing the distances at which they fell in the sea ; I had opportunities of making several very interesting observations, which gave me much new light relative to the action of fired gunpowder. And afterwards, when I went out to America, to command a regiment of cavalry which I had raised in that country for the King's service, his Majesty having been graciously pleased to permit me to take out with me from England four pieces of light artillery, constructed under the direction of the late Lieutenant General DESAGULIERS, with a large proportion of ammunition, I made a great number of interesting experiments with these guns, and also with the

ship guns on board the ships of war in which I made my passage to and from America.

It would take up too much time, and draw out this paper to too great a length, to give an account in detail of all these experiments, and of the various observations I have had opportunities of making from time to time, relative to this subject. I shall, therefore, only observe at present, that the result of all my inquiries tended to confirm me more and more in the opinion, that the theory generally adopted relative to the explosion of gunpowder was extremely erroneous, and that its force is in fact much greater than is generally imagined. That the position of Mr. ROBINS, which supposes the inflammation and combustion of gunpowder to be so instantaneous "that the whole of the charge of a piece of ordnance is actually inflamed and converted into an elastic vapour before the bullet is sensibly moved from its place," is very far from being true; and that the ratio of the elasticity of the generated fluid, to its density, or to the space it occupies as it expands, is very different from that assumed by Mr. ROBINS.

The rules laid down by Mr. ROBINS for computing the velocities of bullets from their weight, the known dimensions of the gun, and the quantities of powder made use of for the charge, may, and certainly do, very often give the velocities very near the truth; but this is no proof that the principles upon which these computations are made are just; for it may easily happen, that a complication of erroneous suppositions may be so balanced, that the result of a calculation founded on them may, nevertheless, be very near the truth; and this is never so likely to happen as when, from known effects, the

action of the powers which produce them are computed. For it is not in general very difficult to assume such principles as, when taken together, may in the most common known cases answer completely all the conditions required. But in such cases, if the truth be discovered with regard to any one of the assumed principles, and it be substituted in the place of the erroneous supposition, the fallacy of the whole hypothesis will immediately become evident.

As I have mentioned the experiments made with heavy artillery, as having been led by their results to form important conjectures relative to the nature of the expansion of the fluid generated in the combustion of gunpowder; it may perhaps be asked, and indeed with some appearance of reason, what the circumstances were which attended the experiments in question, which could justify so important a conclusion as that of the fallacy of the commonly received theory relative to that subject. To this I answer briefly, that in regard to the supposed instantaneous inflammation of the powder, upon which the whole fabric of this theory is built, or rather of all the computations which are grounded upon it, a careful attention to the phenomena which take place upon firing off cannon, led me to suspect, or rather confirmed me in my former suspicions, that however rapid the inflammation of gunpowder may be, its *total combustion* is by no means so sudden as this theory supposes. When a heavy cannon is fired in the common way, that is, when the vent is filled with loose powder, and the piece is fired off with a match, the time employed in the passage of the inflammation through the vent into the chamber of the piece is perfectly sensible, and this time is evidently shorter after the piece has been heated by

repeated firing. With the same charge, the recoil of a gun, (and consequently the velocity of its bullet), is greater after the gun has been heated by repeated firing than when it is cold. The velocity of the bullet is considerably greater when the cannon is fired off with a vent tube, or by firing a pistol charged with powder into the open vent, than when the vent is filled with loose powder. The velocity of two, three, or more fit bullets discharged at once from a piece of ordnance, compared to the velocity of one single bullet discharged by the same quantity of powder, from the same cannon, is greater than it ought to be according to the theory. Considerable quantities of powder are frequently driven out of cannon and other fire-arms *unconsumed*. The manner in which the smoke of gunpowder rises in the air, and is gradually dissolved and rendered invisible, shews it to partake of the nature of steam. But not to take up too much time with these general observations, I shall proceed to give an account of experiments the results of which will be considered as more conclusive.

Having found it impossible to measure the elastic force of fired gunpowder with any degree of precision by any of the methods before mentioned, I totally changed my plan of operations, and instead of endeavouring to determine its force by causing the generated elastic fluid to act upon a moveable body through a determined space, I set about contriving an apparatus in which this fluid should be made to act, by a determined surface, against a weight, which by being increased at pleasure should at last be such as would just be able to confine it, and which in that case would just counterbalance and consequently *measure* its elastic force.

The idea of this method of determining the force of fired

gunpowder occurred to me many years ago; but a very expensive and troublesome apparatus being necessary in order to put it in execution, it was not till the year 1792, when, being charged with the arrangement of the army of his most Serene Highness the ELECTOR PALATINE, reigning Duke of Bavaria, and having all the resources of the military arsenal, and a number of very ingenious workmen at my command, with the permission and approbation of his most Serene Electoral Highness, I set about making the experiments which I shall now describe: and as they are not only important in themselves, and in their results, but as they are, I believe, the first of the kind that have been made, I shall be very particular in my account of them, and of the apparatus used in making them.

One difficulty being got over, that of setting fire to the powder without any communication with the external air, by causing the heat employed for that purpose to pass through the solid substance of the barrel, it only remained to apply such a weight to an opening made in the barrel as the whole force of the generated elastic fluid should not be able to lift, or displace; but in doing this many precautions were necessary. For, first, as the force of gunpowder is so very great, it was necessary to employ an enormous weight to confine it; for, though by diminishing the size of the opening, the weight would be lessened in the same proportion, yet it was necessary to make this opening of a certain size, otherwise the experiments would not have been satisfactory; and it was necessary to make the support or base upon which the barrel was placed very massy and solid, to prevent the errors which would unavoidably have arisen from its want of solidity, or from its elasticity.

The annexed drawings (Tab. V.) will give a complete idea

of the whole apparatus made use of in these experiments. A. (fig. 1.) is a solid block of very hard stone, 4 feet 4 inches square, placed upon a bed of solid masonry, which descended 6 feet below the surface of the earth. Upon this block of stone, which served as a base to the whole machinery, was placed the barrel B of hammered iron, upon its support C, which is of cast brass, or rather of gun-metal; which support was again placed upon a circular plate of hammered iron D, 8 inches in diameter, and $\frac{3}{4}$ of an inch thick, which last rested upon the block of stone. The opening of the bore of the barrel (which was placed in a vertical position, and which was just $\frac{1}{4}$ of an inch in diameter) was closed by a solid hemisphere E of hardened steel, whose diameter was 1.16 inch; and upon this hemisphere the weight F, made use of for confining the elastic fluid generated from the powder in its combustion, reposed. This weight, (which in some of the most interesting experiments was a cannon of metal, a heavy twenty-four pounder, placed vertically upon its cascabel) being fixed to the timbers G G which formed a kind of carriage for it, was moveable up and down; the ends of these timbers being moveable in grooves cut in the vertical timbers K K, which being fixed below in holes made to receive them in the block of stone, and above by a cross piece L, were supported by braces and iron clamps made fast to the thick walls of building of the arsenal. This weight was occasionally raised and lowered in the course of the experiments (in placing and removing the barrel), by means of a very strong lever, which is omitted in the drawing to make it less complicated. The barrel, a section of which is represented in fig. 2. of its natural size, is 2.78 inches long, and 2.82 inches in diameter, at its lower extremity, where it

reposes upon its supporter, but something less above, being somewhat diminished, and rounded off at its upper extremity. Its bore, which, as I have already observed, is $\frac{1}{4}$ of an inch in diameter, is 2.13 inches long, and it ends in a very narrow opening below, not more than 0.07 of an inch in diameter, and 1.715 inch long, which forms the vent (if I may be permitted to apply that name to a passage which is not open at both ends), by which the fire is communicated to the charge. From the centre of the bottom of the barrel there is a projection of about 0.45 of an inch in diameter, and 1.3 inch long, which forms the vent tube V. Fig. 3. is a view of an iron ball W, which being heated red-hot, and being applied to the vent tube by means of an hole O made in it for that purpose, fire is communicated through the solid substance of the vent tube to the powder it contains, and from thence to the charge.

Fig. 4. which is drawn on a scale of two inches to the inch, or half the real size of the machinery, shews how the barrel B was placed upon its support C; how this last was placed upon its circular plate of iron D, and how the red-hot iron ball W was applied to the vent tube V. This ball is managed by means of a long handle *b* of iron, and being introduced through a circular opening *g* in the support, and applied to the vent tube V, is kept in its place by means of a wedge, or rather lever *l*, whose external end is represented in the drawing as being broken off, to save room. The circular opening in front of the support is seen in front, and consequently more distinctly, in the drawing, fig. 1. In this drawing the end of the vent tube may be likewise discovered through this opening; but as it was necessary, in order to introduce all the parts of this machinery, to make the drawing upon a very small scale, it was not possible

to express all the smaller parts with that distinctness which I wished. The other figures which are added, in which the parts are expressed separately, and upon a larger scale, will, it is hoped, supply this defect.

The stand, or support as I have called it, upon which the barrel was placed, is circular, and in order that it might be united more firmly to the plate of iron upon which it reposes, this plate is furnished with a cylindrical projection *p*, 1 inch long and $1\frac{1}{2}$ in diameter, which enters a hole made in the bottom of the stand to receive it.

Fig. 5. is a view of the barrel from above, in which the projecting screws, or rather cylinders, are seen, by which the hemisphere *E*, fig. 2. which closed the end of the barrel, was kept in its place. Two of these screws 1, 2, are seen in the figures 2 and 4. The smaller circle *ab*, fig. 5. shews the diameter of a circular plate of gold, which was let into the end of the barrel, being firmly fixed to the iron solder; and the larger circle *cd* represents a circular piece of oiled leather, which was placed between the end of the barrel and the hemisphere which rested upon it.

The end of the barrel was covered with gold, in order to prevent as much as possible its being corroded by the elastic vapour which, when the weight is not heavy enough to confine it, escapes between the end of the barrel and the flat surface of the hemisphere; but even this precaution was not found to be sufficient to defend the apparatus from injury. The sharp edge of the barrel at the mouth of the bore was worn away almost immediately, and even the flat surface of the hemisphere, notwithstanding it was of hardened steel and very highly polished, was sensibly corroded. This corrosion of the mouth of the

bore, by which the dimensions of the surface upon which the generated elastic fluid acted were rendered very uncertain, would alone have been sufficient to have rendered all my attempts to determine the force of fired gunpowder abortive, had I not found means to remedy the evil. The method I pursued for this purpose was as follows. Having provided some pieces of very good compact sole-leather, I caused them to be beaten upon an anvil with a heavy hammer, to render them still more compact; and then, by means of a machine made for that purpose, cylindric stoppers, of the same diameter precisely as the bore of the barrel, and 0.13 of an inch in length (that is to say, the thickness of the leather), were formed of it; and one of these stoppers, which had previously been greased with tallow, being put into the mouth of the piece after the powder had been introduced, and being forced into the bore till its upper end coincided with the end of the barrel, upon the explosion taking place, this stopper (being pressed on the one side by the generated elastic fluid, and on the other by the hemisphere, loaded with the whole weight employed to confine the powder), so completely closed the bore, that when the force of the powder was not sufficient to raise the weight to such a height that the stopper was actually blown out of the piece, not a particle of the elastic fluid could make its escape. And in those cases in which the weight was actually raised, and the generated elastic fluid made its escape, as it did not corrode the barrel in any other part but just *at the very extremity of the bore*, the experiment by which the weight was ascertained, which was just able to counterbalance the pressure of the generated elastic fluid, was in nowise vitiated, either by the increased diameter of the bore at its extremity, or by any

corrosion of the hemisphere itself; for as long as the bore retained its form and its dimensions, in that part to which the efforts of the elastic fluid were confined, that is, in that part of the bore immediately in contact with the lower part of the stopper, the experiment could not be affected by any imperfection of the bore either above or below.

In the figures 2. and 4. this stopper is represented in its place, and fig. 6. shews the plan, and fig. 7. the profile of one of these stoppers of its full size. Fig. 8. shews a small but very useful instrument, employed in introducing these stoppers into the bore, and more especially in occasionally extracting them: it resembles a common cork-screw, only it is much smaller. In the figure (where it is shewn in its full size), it is represented screwed into a stopper. Fig. 9. shews the plan, and fig. 10. a side view, of the hemisphere of hardened steel, by which the end of the barrel was closed. In the figures 2. and 4. the barrel is represented as being about half filled with powder.

Presuming that what has been already said, together with the assistance of the annexed drawings, will be sufficient to give a perfect idea of all the different parts of this apparatus, I shall now proceed to give an account of the experiments which from time to time have been made with it. And in order to render these details as intelligible as possible, and to shew the results of all these inquiries in a clear and satisfactory manner, I shall first give a brief account of the manner in which the experiments were made; of the various precautions used; and the particular appearances which were observed in the prosecution of them.

The powder made use of in these experiments was of the best quality, being that kind called *poudre de chasse* by the

French, and very fine grained : and it was all taken from the same parcel. Care was taken to dry it very thoroughly, and the air of the room in which it was weighed out for use was very dry. The weights employed for weighing the powder were German apothecary's grains, 104.8 of which make 100 grains Troy. I have reduced the weights employed to confine the elastic vapour generated in the combustion of the powder from Bavarian pounds, in which they were originally expressed, to pounds avoirdupois. The measures of length were all taken in English feet and inches. The experiments were all made in the open air, in the court-yard of the arsenal at Munich; and they were all made in fair weather, and between the hours of nine and twelve in the forenoon, and two and five in the afternoon; but the barrel was always charged, and the extremity of the bore closed by its leather stopper, in the room where the powder was weighed. In placing the barrel upon the block of stone, great care was taken to put it exactly under the centre of gravity of the weight employed to confine the generated elastic vapour. Upon applying the red-hot ball to the vent tube, and fixing it in its place by its lever which supported it, the explosion very soon followed.

When the force of the generated elastic vapour was sufficient to raise the weight, the explosion was attended by a very sharp and surprisingly loud report; but when the weight was not raised, as also when it was only a little moved, but not sufficiently to permit the leather stopper to be driven quite out of the bore, and the elastic fluid to make its escape, the report was scarcely audible at the distance of a few paces, and did not at all resemble the report which commonly attends the explosion of gunpowder. It was more like the noise

which attends the breaking of a small glass tube than any thing else to which I can compare it. In many of the experiments in which the elastic vapour was confined, this feeble report attending the explosion of the powder was immediately followed by another noise, totally different from it, which appeared to be occasioned by the falling back of the weight upon the end of the barrel, after it had been a little raised, but not sufficiently to permit the leather stopper to be driven quite out of the bore. In some of these experiments, a very small part only of the generated elastic fluid made its escape: in these cases the report was of a peculiar kind, and though perfectly audible at some considerable distance, yet not at all resembling the report of a musket. It was rather a very strong, sudden hissing, than a clear, distinct, and sharp report.

Though it could be determined with the utmost certainty by the report of the explosion, whether any part of the generated elastic fluid had made its escape, yet for still greater precaution, a light collar of very clean cotton wool was placed round the edge of the steel hemisphere, where it reposed upon the end of the barrel, which could not fail to indicate by the black colour it acquired, the escape of the elastic fluid, whenever it was strong enough to raise the weight by which it was confined sufficiently to force its way out of the barrel.

Though the end of the barrel at the mouth of the bore was covered with a circular plate of gold, in order the better to defend the mouth of the bore against the effects of the corrosive vapour, yet this plate being damaged in the course of the experiments (a piece of it being blown away), the remainder of it was removed; and it was never after thought necessary to replace it by another. When this plate of gold was taken

away, the length of the barrel was of course diminished as much as the thickness of this plate amounted to, which was about $\frac{1}{400}$ part of an inch; but in order that even this small diminution of the length of the barrel might have no effect on the results of the experiments, its bore was deepened $\frac{1}{400}$ of an inch when this plate was removed, so that the *capacity* of the bore remained the same as before.

After making use of a great variety of expedients, the best and most convenient method of closing the end of the bore, and defending the flat surface of the steel hemisphere from the corroding vapours, was found to be this; first, to cover the end of the bore with a circular plate of thin oiled leather, then to lay upon this a very thin circular plate of hammered brass, and upon this brass plate the flat surface of the hemisphere. When the elastic fluid made its escape, a part of the leather was constantly found to have been torn away, but never in more places than one; that is to say, always on one side only.

What was very remarkable in all those experiments in which the generated elastic vapour was completely confined, was the small degree of expansive force which this vapour appeared to possess after it had been suffered to remain a few minutes, or even only a few seconds, confined in the barrel; for, upon raising the weight by means of its lever, and suffering this vapour to escape, instead of escaping with a loud report, it rushed out with a hissing noise hardly so loud or so sharp as the report of a common air-gun; and its efforts against the leathern stopper, by which it assisted in raising the weight, were so very feeble as not to be sensible. Upon examining the barrel, however, this diminution of the force of the generated elastic fluid was easily explained; for what was undoubtedly in the

moment of the explosion in the form of an elastic fluid, was now found transformed into a *solid body* as hard as a stone! It may easily be imagined how much this unexpected appearance excited my curiosity; but, intent on the prosecution of the main design of these experiments, the ascertaining the force of fired gunpowder, I was determined not to permit myself to be enticed away from it by any extraordinary or unexpected appearances, or accidental discoveries, however alluring they might be; and faithful to this resolution, I postponed the examination of this curious phænomenon to a future period; and since that time I have not found leisure to engage in it. I think it right, however, to mention in this place such cursory observations as I was able, in the midst of my other pursuits, to make upon this subject; and it will afford me sincere pleasure, if what I have to offer should so far excite the curiosity of philosophers, as to induce some one who has leisure, and the means of pursuing such inquiries with effect, to precede me in the investigation of this interesting phænomenon; and as the subject is certainly not only extremely curious in itself, but bids fair to lead to other and very important discoveries, I cannot help flattering myself that some attention will be paid to it. I have said that the solid substance into which the elastic vapour generated in the combustion of gunpowder was transformed, was *as hard as a stone*. This I am sensible is but a vague expression; but the fact is, that it was very hard, and so firmly attached to the inside of the barrel, and particularly to the inside of the upper part of the vent tube, that it was always necessary, in order to remove it, to make use of a drill, and frequently to apply a considerable degree of force. This substance, which was of a black colour, or rather of a

dirty grey, which changed to black upon being exposed to the air, had a pungent, acrid, alkaline taste, and smelt like liver of sulphur. It attracted moisture from the air with great avidity. Being moistened with water, and spirit of nitre being poured upon it, a strong effervescence ensued, attended by a very offensive and penetrating smell. Nearly the whole quantity of matter of which the powder was composed, seemed to have been transformed into this substance; for the quantity of elastic fluid which escaped upon removing the weight, was very inconsiderable; but this substance was *no longer gunpowder*; it was not even inflammable. What change had it undergone? what could it have lost? It is very certain the barrel was considerably heated in these experiments. Was this occasioned by the *caloric*, disengaged from the powder in its combustion, making its escape through the iron? And is this a proof of the existence of *caloric*, considered as a fluid *sui generis*; and that it actually enters into the composition of inflammable bodies, or of pure air, and is necessary to their combustion? I dare not take upon me to decide upon such important questions. I once thought that the heat acquired by a piece of ordnance in being fired, arose from the vibration or friction of its parts, occasioned by the violent blow it received in the explosion of the powder; but I acknowledge fairly, that it does not seem to be possible to account in a satisfactory manner for the very considerable degree of heat which the barrel acquired in these experiments, merely on that supposition.

That this hard substance, found in the barrel after an experiment in which the generated elastic vapour had been completely confined, was actually in a fluid or elastic state in the moment of the explosion, is evident from hence, that in all

those cases in which the weight was raised, and the stopper blown out of the bore, nothing was found remaining in the barrel. It was very remarkable that this hard substance was not found distributed about in all parts of the barrel indifferently, but there was always found to be more of it near the middle of the length of the bore, than at either of its extremities; and the upper part of the vent tube in particular was always found quite filled with it. It should seem from hence, that it attached itself to those parts of the barrel which were soonest cooled; and hence the reason, most probably, why none of it was ever found in the lower part of the vent tube, where it was kept hot by the red-hot ball by which the powder was set on fire.

I found by a particular experiment, that the gunpowder made use of, when it was well shaken together, occupied rather less space in any given measure, than the same weight of water; consequently when gunpowder is fired in a confined space which it fills, the density of the generated elastic fluid must be at least equal to the density of water. The real specific gravity of the solid grains of gunpowder, determined by weighing them in air and water, is to the specific gravity of water, as 1.868 to 1.000. But if a measure, whose capacity is one cubic foot, hold 1000 ounces of water, the same measure will hold just 1077 ounces of fine grained gunpowder, such as I made use of in my experiments; that is to say, when it is well shaken together. When it was moderately shaken together, I found its weight to be exactly equal to that of an equal volume, or rather measure, of water. But it is evident that the weight of any given measure of gunpowder, must depend much upon the forms and sizes of its grains. I shall add only one

observation more, relative to the particular appearances which attended the experiments in which the elastic vapour generated in the combustion of gunpowder was confined, and that is, with regard to a curious effect produced upon the inferior flat surface of the leathern stopper, where it was in contact with the generated elastic vapour. Upon removing the stopper, its lower flat surface appeared entirely covered with an extremely white powder, resembling very light white ashes, but which almost instantaneously changed to the most perfect black colour upon being exposed to the air.

The sudden change of colour in this substance upon its being exposed to the air, has led me to suspect that the solid matter found in the barrel was not originally black, but that it became black merely in consequence of its being exposed to the air. The dirty grey colour it appeared to have immediately on being drilled out of the cavity of the bore, where it had fixed itself, seems to confirm this suspicion. An experiment made with a very strong glass barrel would not only decide this question, but would most probably render the experiment peculiarly beautiful and interesting on other accounts; and I have no doubt but a barrel of glass might be made sufficiently strong to withstand the force of the explosion. Whether it would be able to withstand the sudden effects of the heat, I own I am more doubtful; but as the subject is so very interesting, I think it would be worth while to try the experiment. Perhaps the apparatus might be so contrived as to set fire to the powder by the solar rays, by means of a common burning glass; but even if that method should fail, there are others equally unexceptionable, which might certainly be employed with success; and it is hardly possible to imagine any thing

more curious than an experiment of this kind would be, if it were successful.

But to proceed to the experiments by which I endeavoured to ascertain the force of fired gunpowder. All the parts of the apparatus being ready, it was in the autumn of the year 1792 that the first experiment was made.

The barrel being charged with 10 grains of powder (its contents when quite full amounting to about 28 grains), and the end of the barrel being covered by a circular piece of oiled leather, and the flat side of the hemisphere being laid down upon this leather, and a heavy cannon, a twenty-four pounder, weighing 8081 lbs. avoirdupois, being placed upon its cascabel in a vertical position upon this hemisphere, in order to confine by its weight the generated elastic fluid, the heated iron ball was applied to the end of the vent tube; and I had waited but a very few moments in anxious expectation of the event, when I had the satisfaction of observing that the experiment had succeeded. The report of the explosion was extremely feeble, and so little resembling the usual report of the explosion of gunpowder, that the by-standers could not be persuaded that it was any thing more than a cracking of the barrel, occasioned merely by its being heated by the red-hot ball: yet, as I had been taught by the result of former experiments not to expect any other report, and as I found upon putting my hand upon the barrel that it began to be sensibly warm, I was soon convinced that the powder must have taken fire; and after waiting four or five minutes, upon causing the weight which rested upon the hemisphere to be raised, the confined elastic vapour rushed out of the barrel. Upon removing the barrel and examining it, its bore was found to be choaked up by the solid

substance which I have already described, and from which it was with some difficulty that it was freed, and rendered fit for another experiment. The extreme feebleness of the report of the explosion, and the small degree of force with which the generated elastic fluid rushed out of the barrel upon removing the weight which had confined it, had inspired my assistants with no very favourable idea of the importance of these experiments. I had seen, indeed, from the beginning by their looks, that they thought the precautions I took to confine so inconsiderable a quantity of gunpowder as the barrel could contain, perfectly ridiculous; but the result of the following experiment taught them more respect for an agent, of whose real force they had conceived so very inadequate an idea.

In this second experiment, instead of 10 grains of powder, the former charge, the barrel was now quite filled with powder, and the steel hemisphere, with its oiled leather under it, was pressed down upon the end of the barrel by the same weight as was employed for that purpose in the first experiment, namely, a cannon weighing 8081 lbs. In order to give a more perfect idea of the result of this important experiment, it may not be amiss to describe more particularly one of the principal parts of the apparatus employed in it, I mean the barrel. This barrel (which though similar to it in all respects was not the same that has already been described,) was made of the best hammered iron, and was of uncommon strength. Its length was $2\frac{3}{4}$ inches; and though its diameter was also $2\frac{3}{4}$ inches, the diameter of its bore was no more than $\frac{1}{4}$ of an inch, or less than the diameter of a common goose quill. The length of its bore was 2.15 inches. Its diameter being $2\frac{3}{4}$ inches, and the diameter of its bore only $\frac{1}{4}$ of an inch, the thickness of the

metal was $1\frac{1}{4}$ inch; or, it was 5 times as thick as the diameter of its bore. The charge of powder was extremely small, amounting to but little more than $\frac{1}{10}$ of a cubic inch: not so much as would be required to load a small pocket pistol, and not *one-tenth part* of the quantity frequently made use of for the charge of a common musket. I should be afraid to relate the result of this experiment, had I not the most indisputable evidence to produce in support of the facts. This inconsiderable quantity of gunpowder, when it was set on fire by the application of the red-hot ball to the vent tube, exploded with such inconceivable force as to burst the barrel asunder in which it was confined, notwithstanding its enormous strength; and with such a loud report as to alarm the whole neighbourhood. It is impossible to describe the surprise of those who were spectators of this phænomenon. They literally turned pale with affright and astonishment, and it was some time before they could recover themselves. The barrel was not only completely burst asunder, but the two halves of it were thrown upon the ground in different directions: one of them fell close by my feet, as I was standing near the machinery to observe more accurately the result of the experiment. Though I thought it possible that the weight might be raised, and that the generated elastic vapour would make its escape, yet the bursting of the barrel was totally unexpected by me. It was a new lesson to teach me caution in these dangerous pursuits.

It affords me peculiar satisfaction in laying these accounts before the Royal Society, to be able to produce the most respectable testimony of their authenticity.

My friend Sir CHARLES BLAGDEN, one of the worthy Secretaries of the Society, visited Munich in the summer of the year

1793, in his return from Italy; and though I was then absent (travelling for the recovery of my health), yet, by my directions, he was not only shewn every part of the apparatus made use of in these experiments, but several experiments were actually repeated in his presence; and he was kind enough to take with him to England one half of the barrel which was burst in the experiment just mentioned, which at my request he has deposited in the Museum of the Society, and which I flatter myself will be looked upon as the most unequivocal proof of my discoveries relative to the amazing force of the elastic vapour generated in the combustion of gunpowder.

When the amazing strength of this barrel is considered, and when we consider the smallness of the capacity of its bore, it appears almost incredible, that so small a quantity of powder as that which was employed in the experiment could burst it asunder.

But without insisting on the testimony of several persons of respectable character, who were eye witnesses of the fact, and from whom Sir CHARLES BLAGDEN received a verbal account, in detail, of all the circumstances attending the experiment, I fancy I may very safely rest my reputation upon the silent testimony which this broken instrument will bear in my favour; much doubting whether it be in the power of art to burst asunder such a mass of solid iron, by any other means than those I employed.

Before I proceed to give an account of my subsequent experiments upon this subject, I shall stop here for a moment to make an estimate, from the known strength of iron, and the area of the fracture of the barrel, of the real force employed by the elastic vapour to burst it. In a course of experiments upon

the strength of various bodies which I began many years ago, and an account of which I intend at some future period to lay before the Royal Society,* I found, by taking the mean of the results of several experiments, that a cylinder of good tough hammered iron, the area of whose transverse section was only $\frac{3}{1600}$ of an inch, was able to sustain a weight of 119 lbs. avoirdupois, without breaking. This gives 63,466 lbs. for the weight which a cylinder of the same iron whose transverse section is one inch, would be able to sustain without being broken. The area of the fracture of the barrel before mentioned was measured with the greatest care, and was found to measure very exactly $6\frac{1}{2}$ superficial inches. If now we suppose the iron of which this barrel was formed, to be as strong as that whose strength I determined (and I have no reason to suspect it to be of an inferior quality), in that case, the force actually employed in bursting the barrel must have been equal to the pressure of a weight of 412529 lbs. For the resistance or cohesion of one inch, is to 63466 lbs. as that of $6\frac{1}{2}$ inches to 412529 lbs. ; and this force, so astonishingly great, was exerted by a body which weighed less than 26 grains Troy, and which acted in a space that hardly amounted to $\frac{1}{10}$ of a cubic inch.

To compare this force exerted by the elastic vapour gene-

* Since writing the above, I have met with a misfortune which has put it out of my power to fulfil my promise to the Royal Society. On my return to England from Germany in October, 1795, after an absence of eleven years, I was stopped in my post-chaise in St. Paul's churchyard, in London, at six o'clock in the evening, and robbed of a trunk which was behind my carriage, containing all my private papers and my original notes and observations on philosophical subjects. By this cruel accident I have been deprived of the fruits of the labours of my whole life ; and have lost all that I held most valuable. This most severe blow has left an impression on my mind, which I feel that nothing will ever be able entirely to remove.

rated in the combustion of gunpowder, and by which the barrel was burst, to the pressure of the atmosphere, it is necessary to determine the area of a longitudinal section of the bore of the piece. Now the diameter of the bore being $\frac{1}{4}$ of an inch, and its length (after deducting 0.15 of an inch for the length of the leathern stoppers) 2 inches, the area of its longitudinal section turns out to have been $\frac{1}{2}$ an inch. And if now we assume the mean pressure of the atmosphere = 15 lbs. avoirdupois for each superficial inch, this will give $7\frac{1}{2}$ for that upon a surface = $\frac{1}{2}$ inch, equal to the area of a longitudinal section of the bore of the barrel.

But we have just found that the force actually exerted by the elastic vapour in bursting the barrel, amounted to 412529 lbs.; this force was therefore 55004 times greater than the mean pressure of the atmosphere! For it is as $7\frac{1}{2}$ lbs. to 1 atmosphere, so 412529 lbs. to 55004 atmospheres.

Thinking it might perhaps be more satisfactory to know the real strength of the identical iron of which the barrel used in the before mentioned experiment was constructed, rather than to rest the determination of the strength of the barrel upon the decision of the strength of iron taken from another parcel, and which very possibly might be of a different quality, since writing the above, I have taken the trouble to ascertain the strength of the iron of which the barrel was made, which was done in the following manner. Having the one half of the barrel still in my possession, I caused small pieces, 2 inches long, and about $\frac{1}{8}$ of an inch square, to be cut out of the solid block, in the direction of its length, with a fine saw; and these pieces being first made round in their middle by filing, and then by turning in a lathe with a very sharp instrument, were

reduced to such a size as was necessary, in order to their being pulled asunder in my machine for measuring the strength of bodies. In this machine the body to be pulled asunder is held fast by two strong vices, the one fastened to the floor, and the other suspended to the short arm of a Roman balance, or common steel-yard; and in order that the bodies so suspended may not be injured by the jaws of the vices, so as to be weakened and to vitiate the experiments, they are not made cylindrical, but they are made larger at their two ends where they are held by the vices, and from thence their diameters were gradually diminished towards the middle of their lengths, where their measures were taken, and where they never failed to break.

As I had found by the results of many experiments which I had before made upon the strength of the various metals, that iron, as well as all other metals, is rendered much stronger by hammering, I caused those pieces of the barrel which were prepared for these experiments to be separated from the solid block of metal, and reduced to their proper sizes, by sawing, filing, and turning, and without ever receiving a single blow of a hammer; so that there is every reason to believe that the strength of the iron, as determined by the experiments, may safely be depended on. The results of the experiments were as follows:

Experiments	Diameter of the Cylinder at the Fracture.	Area of a transverse section of the Cylinder at the Fracture.	Weight required to break it. lbs. avoirdupois.	Weight required to break 1 inch of this iron. lbs. avoirdupois.
1.	$\frac{50}{1000}$	$\frac{1}{509,29}$	123.18	62737.
2.	$\frac{60}{1000}$	$\frac{1}{353,68}$	182.	64366.
3.	$\frac{66}{1000}$	$\frac{1}{292,3}$	220.75	64526.
4.	$\frac{76}{1000}$	$\frac{1}{220,7}$	277.01	61063.
Number of Experiments = 4.)				252692.
Mean				63173.

If now we take the strength of the iron of which the barrel was composed as here determined by actual experiments, and compute the force required to burst the barrel, it will be found equal to the pressure of a weight of $410624\frac{1}{2}$ lbs. instead of 436800 as before determined. For it is the resistance or force of cohesion of 1 inch of this iron to 63173 lbs., as that of $6\frac{1}{2}$ inches (the area of the fracture of the barrel) to $410624\frac{1}{2}$ lbs. And this weight turned into atmospheres, in the manner above described, gives 54750 atmospheres for the measure of the force which must have been exerted by the elastic fluid in bursting the barrel. But this force, enormous as it may appear, must still fall short of the real initial force of the elastic fluid generated in the combustion of gunpowder, before it has begun to expand; for it is more than probable that the barrel was in fact burst before the generated elastic fluid had exerted all its force, or that this fluid would have been able to have burst a barrel still stronger than that used in the experiment.—But I wave these speculations in order to hasten to more interesting and more satisfactory investigations. Passing over in silence a consider-

able number of promiscuous experiments, which having nothing particularly remarkable in their results, could throw no new light upon the subject, I shall proceed immediately to give an account of a regular set of experiments, undertaken with a view to the discovery of certain determined facts, and prosecuted with unremitting perseverance.

These experiments were made by my directions under the immediate care of Mr. REICHENBACH, commandant of the corps of artificers in the Elector's military service, and of Count SPRETI, first lieutenant in the regiment of artillery.

Though I was prevented by ill health from being actually present at all these experiments, yet being at hand, and having every day, and almost every hour, regular reports of the progress that was made in them, and of every thing extraordinary that happened, the experiments may be said with great truth to have been made under my immediate direction; and as the two gentlemen by whom I was assisted, were not only every way qualified for such an undertaking, but had been present, and had assisted me in a number of similar experiments which I had myself made, they had acquired all that readiness and dexterity in the various manipulations which are so useful and necessary in experimental inquiries; and I think I can safely venture to say that the experiments may be depended upon. It would have afforded me great satisfaction to have been able to say that the experiments were all made by myself; and I had resolved to repeat them before I made them public, particularly as there appear to have been some very extraordinary and quite unaccountable differences in the results of those made in different seasons of the year; but having hitherto been prevented by ill health, and by other avocations, from engag-

ing again in these laborious researches, I have thought it right not to delay any longer the publication of facts, which appear to me to be both new and interesting, as their publication may perhaps excite others to engage in their farther investigation.

The principal objects I had in view in the following set of experiments were, first, to determine the expansive force of the elastic vapour generated in the combustion of gunpowder in its various states of condensation, and to ascertain the ratio of its elasticity to its density: and secondly, to measure, by one decisive experiment, the utmost force of this fluid in its most dense state; that is to say, when the powder completely fills the space in which it is fired, and in which the generated fluid is confined. As these experiments were very numerous, and as it will be more satisfactory to be able to see all their results at one cursory view, I have brought them into the form of a general table.

In this table, which does not stand in need of any particular explanation, may be seen the results of all these investigations.

The dimensions of the barrel made use of in the experiments mentioned in this table, were as follows.

Diameter of the bore at its muzzle = 0.25 of an inch.

Joint capacities of the bore, and of its vent tube, exclusive of the space occupied by the leathern stopper, = 0.08974 of a cubic inch.

Quantity of powder contained by the barrel and its vent tube when both were quite full, (exclusive of the space occupied by the leathern stopper,) 25.641 German apothecary's grains, = $24\frac{1}{2}$ grains Troy.

The capacities of the barrel and of its vent tube were deter-

mined by filling them with mercury, and then weighing in air and in water the quantity of mercury required to fill them; and the quantity of powder required to fill the barrel and its vent tube was determined by computation, from the known joint capacities of the barrel and its vent tube, in parts of a cubic inch, and from the known specific gravity of the powder used in the experiments.

Thus the contents of the barrel and its vent tube having been found to amount to 0.08974 of a cubic inch, and it having been found that 1 cubic inch of the gunpowder in question, well shaken together, weighed just 272.68 grains Troy, this gives 24.47 grains Troy (= 25.641 grains, German apothecary's weight) for the contents of the barrel and its vent tube.

The numbers expressing the charges of powder in *thousandth parts* of the joint capacities of the barrel and of its vent tube, were determined from the known quantities of powder used in the different experiments, expressed in German apothecary's grains, and the relation of these quantities to the quantity required to fill the barrel and its vent tube completely.

Thus, as the barrel and its vent tube were capable of containing 25.641 apothecary's grains of powder, if we suppose this quantity to be divided into 1000 equal parts, this will give 39 of those parts for 1 grain; 78 parts for 2 grains; 390 for 10 grains, &c. For it is 25.641 to 1000, as 1 to 39 very nearly.

As this method of expressing the quantities of powder shows at the same time the relative density of the generated elastic fluid, it is the more satisfactory on that account: it will also

considerably facilitate the computations necessary in order to ascertain the ratio of the elasticity of this fluid to its density.

The elastic force of the fluid generated in the combustion of the charge of powder, is measured by the weight by which it was confined, or rather by that which it was just able to move, but which it could not raise sufficiently to blow the leathern stopper quite out of the mouth of the bore of the barrel.

This weight in all the experiments, except those which were made with very small charges of powder, was a piece of ordnance, of greater or less dimensions, or greater or less weight, according to the force of the charge; placed vertically upon its cascabel, upon the steel hemisphere which closed the end of the barrel; and the same piece of ordnance, by having its bore filled with a greater or smaller number of bullets, as the occasion required, was made to serve for several experiments.

The weight employed for confining the generated elastic fluid, is expressed in the following table in *pounds avoirdupois*; but in order that a clearer and more perfect idea may be formed of the real force of its elastic fluid, I have added a column in which its force, answering to each charge of powder, is expressed in *atmospheres*.

The numbers in this column were computed in the following manner. The diameter of the bore of the barrel at its muzzle being just $\frac{1}{4}$ of an inch, the area of its transverse section is 0.049088 of a superficial inch; and assuming the mean pressure of the atmosphere upon 1 superficial inch equal to 15lbs. avoirdupois, this will give 0.73631 of a pound avoirdupois for that pressure upon 0.049088 of a superficial inch, or upon a surface equal to the area by which the generated

elastic fluid acted on the weight employed to confine it; consequently the weight expressed in *pounds avoirdupois*, which measured the force of the generated elastic fluid in any given experiment, being divided by 0.73631, will show how many times the pressure exerted by the fluid was greater than the mean pressure of the atmosphere. Thus in the experiment, No. 6, where the weight which measured the elastic force of the generated fluid was = 504.8lbs. avoirdupois, it is $\frac{504.8}{0.73631} = 685.6$ atmospheres. And so of the rest.

I have said that the diameter of the bore of the barrel, made use of in the following experiments, was just $\frac{1}{4}$ of an inch *at its muzzle*, and this is strictly true, as I found upon measuring it with the greatest care; but its diameter is not perfectly the same throughout its whole length, being rather narrower towards its lower end: yet the *capacity* of the barrel being known, and also *the diameter of the bore of its muzzle*, any small inequalities of the bore in any other part can in no wise affect the results of the experiments, as will be evident to those who will take the trouble to consider the matter for a moment with attention. I should not indeed have thought it necessary to mention this circumstance, had I not been afraid that some one who should calculate the joint capacities of the bore and of the vent tube from their lengths and diameters, finding their calculation not to agree with my determination of those capacities, as ascertained by filling them with mercury, might suspect me of having committed an error. The mean diameter of the bore of the barrel, as determined from its length and its capacity, turns out to be just 0.2281 of an inch; the diameter of the vent tube being taken equal to 0.07 of an inch, and its length 1.715 inch.

Table I. Experiments on the Force of fired Gunpowder.

No. of the Experiment.	Time when the Experiment was made. 1793.		State of the Atmosphere.		The charge of Powder.		Weight employed to confine the elastic Fluid.		General Remarks.
			Thermom.	Barometer.	In Apoth. grs.	In 1000 parts of the capacity of the bore.	In lbs. avoirdupois.	In atmospheres.	
No 1	h. m.	F.	Engl. In.	grs	Parts.	lbs.			{ The generated elastic fluid was completely confined, the weight not being raised Ditto. Ditto, weight not raised. Ditto, ditto. Weight just moved. { In these three experiments the weight was raised with a report as loud as that of a pistol. { But just raised, report much weaker. 77.86 Weight hardly moved. Not raised. Raised with a loud report. Ditto, the report weaker. Ditto, the report still weaker. 182.3 Weight but just moved. Raised with a loud report. Ditto, ditto. Ditto, report weaker. Raised, report weaker. 288.2 { The weight hardly moved, no report. Raised with a loud report. Ditto, ditto. Ditto, report less loud. 382.4 { Weight hardly moved, and no report. Raised, loud report. Ditto, ditto. Ditto, ditto. Not raised. Not raised. Not raised. Not raised.
2	23d Feb. 9 0	31°	28.58	1	39	504.8			
3	9 30	—	—	2	78	—			
4	25th 9 0	37°	28.56	3	117	—			
5	10 15	—	—	4	156	—			
6	10 30	—	—	5	195	—			
7	11 0	—	—	6	234	—	685.6		
8	3 0 PM	57°	28.37	1	39	14.16			
9	3 15	—	—	—	—	26.5			
10	3 30	—	—	—	—	38.9			
11	3 45	—	—	—	—	51.3			
12	4 0	—	—	—	—	57.4	77.86		
13	26th 9 0	34°	28.1	2	78	163.5			
14	9 15	—	—	—	—	124			
15	9 30	—	—	—	—	130.5			
16	9 45	—	—	—	—	133			
17	10 0	—	—	—	—	134.2	182.3		
18	3 0	48°	28.31	3	117	186.3			
19	3 15	—	—	—	—	198.7			
20	3 30	—	—	—	—	204.8			
21	3 45	—	—	—	—	208.5			
22	4 0	—	—	—	—	212.24	288.2		
23	27th 3 0	50°	28.36	4	156	269.2			
24	3 15	—	—	—	—	274.13			
25	3 30	—	—	—	—	277.9			
26	3 45	—	—	—	—	281.57	382.4		
27	28th 9 0	34°	28.32	5	195	319.68			
28	9 15	—	—	—	—	351.37			
29	9 30	—	—	—	—	400.9			
30	10 0	—	—	—	—	475.2			
31	3 0	48°	28.35	—	—	443.5			
32	3 15	—	—	—	—	425.65			
33	3 30	—	—	—	—	419.46			

Table I. Experiments on the Force of fired Gunpowder.

No. of the Experiment.	Time when the Experiment was made. 1793.		State of the Atmosphere.		The charge of Powder.		Weight employed to confine the elastic Fluid.		General Remarks.
			Thermom.	Barometer.	In Apoth. grs.	In 1000 parts of the capacity of the bore.	In lbs. avoirdupois.	In atmospheres.	
N ^o	h.	m.	F.	Eng. In.	grs.	Parts.	lbs.		
33	28th Feb.	3 45	48°	28.35	5	195	413.27	561.2	Weight but just moved.
34	1st Mar.	9 0	34°	28.35	7	273	535.79		Raised with a loud report.
35		9 15	—	—	—	—	548.14		Ditto, ditto.
36		9 30	—	—	—	—	560.52		Ditto, ditto.
37		3 0	59°	28.34	—	—	572.9		Ditto, ditto.
38		3 15	—	—	—	—	585.28		Ditto, report weaker.
39		3 30	—	—	—	—	597.66	811.7	{ Weight but just moved, no report.
40		3 45	—	—	8	312	690.52		Raised, report very loud.
41		4 0	—	—	—	—	752.42		Ditto, ditto.
42		4 15	—	—	—	—	783.37		Ditto, ditto.
43	2d	9 0	50°	28.32	—	—	876.22		Not raised.
44		9 15	—	—	—	—	845.19		But just raised, report weak.
45		9 30	—	—	—	—	857.64	1164.8	{ Weight but just moved, and no report.
46		9 45	—	—	9	351	961.65		Raised with a loud report.
47		10 0	—	—	—	—	1209.4		Not raised.
48		10 30	—	—	—	—	1142.3	1551.3	{ Weight just moved, no report.
49		3 0	52°	28.33	10	390	1456.8		Not raised.
50		3 30	—	—	—	—	1329.9		Raised, loud report.
51	5th	9 0	32°	28.2	—	—	1387.5	1884.3	{ Weight but just moved, and no report.
52		9 15	—	—	11	429	1708.2		Not raised.
53		9 45	—	—	—	—	1646.2		Not raised.
54		10 15	—	—	—	—	1615.2		Raised, with a weak report.
55		10 45	—	—	—	—	1634	2219	{ Weight but just moved, and no report.
56	6th	9 0	36°	28.34	12	468	1943.3		Not raised.
57		9 30	—	—	—	—	1932.2		Not raised.
58		10 30	—	—	—	—	1907.4		Weight not raised.
59		11 0	—	—	—	—	1878.4		Raised with a loud report.
60		11 30	—	—	—	—	1895.1	2573.7	{ Weight but just moved, and no report.
61		3 0	42°	28.3	13	507	2142.7		Raised with a loud report.
62		3 15	—	—	—	—	2204.6		Ditto, ditto.

Table I. Experiments on the Force of fired Gunpowder.

No. of the Experiment.	Time when the Experiment was made. 1793.		State of the atmosphere.		The charge of Powder.		Weight employed to confine the elastic Fluid.		General Remarks.
			Thermom.	Barometer.	Apoth. grs.	In 1000 parts of the capacity of the bore.	In lbs. avoirdupois.	In atmospheres.	
N ^o 63	6th Mar.	h. m. 3 30	F. 42°	Eng. In. 28.3	grs. 13	Parts. 507	lbs. 2266.5		Raised with a loud report.
64		3 45	—	—	—	—	2390.3		Raised, report weaker.
65		4 0	—	—	—	—	2422	3288.3	{ Weight just moved, no report.
66	9th	9 0	43°	28.3	14	546	3213		Not raised.
67		9 30	—	—	—	—	3093		Not raised.
68		10 0	—	—	—	—	2968		Not raised.
69		10 30	—	—	—	—	2846		Raised, with a loud report.
70		10 45	—	—	—	—	2908		Raised, report weaker.
71		11 0	—	—	—	—	2939		Ditto, report still weaker.
72		11 15	—	—	—	—	2951	4008	{ Weight but just moved, no report.
73		11 30	—	—	15	585	3750		Not raised.
74		11 45	—	—	—	—	3508		Not raised.
75		12 15	—	—	—	—	3477	4722.5	{ Weight but just moved, and no report.
76	11th	9 0	43°	28.3	16	624	4037		{ The weight was raised with a loud report.
77		9 15	—	—	—	—	4284		Raised, loud report.
78		9 30	—	—	—	—	4532		Ditto, ditto.
79	4th Apr.	3 0	70°	28.2	—	—	5027		Ditto, ditto.
80		3 15	—	—	—	—	5138		Raised, report weaker.
81		3 30	—	—	—	—	5262		Not raised.
82		3 45	—	—	—	—	5220	7090	{ Weight just moved, but no report.
83	5th	3 0	68°	28.3	17	663	8081		Not raised.
84		3 30	—	—	18	702	8081	10977	{ The weight was raised with a very sharp report, louder than that of a well loaded musket.
85		4 0	—	—	—	—	8700		{ The vent tube of the barrel was burst, the explosion being attended with a very loud report.

The barrel being rendered unfit for further service, by the bursting of its vent tube, an end was put to this set of experiments.

In order that a clear and satisfactory idea may be formed of the results of these experiments I have drawn the figure (Tab. VI.), in which the given densities of the generated elastic fluid, or (which amounts to the same thing) the quantities of powder used for the charge, being taken on the line *AB*, from *A* towards *B*, the corresponding elasticities, as found by the experiments, are represented by lines perpendicular to the line *AB*, at the points where the measures of the densities end.

As the irregularities of the dotted line *AC* are owing, no doubt, merely to the errors committed in making the experiments, these irregularities being removed, by drawing the line *AD* in such a manner as to balance the errors of the experiments, this line *AD*, which must necessarily be regular, will, by bare inspection, give us a considerable degree of insight into the nature of the equation which must be formed to express the relation of the densities to the elasticities; one principal object of these experimental inquiries.

Putting the density = x , and the elasticity = y , the line *AD* will be the locus of the equation expressing the relation of x to y ; and had Mr. ROBINS's supposition, that the elasticity is as the density, been true, x would have been found to be to y in a constant (simple) ratio, *AD* would have been a straight line, and *AE* would have been the position of this line, had Mr. ROBINS's determination of the force of fired gunpowder been accurate.

But *AD* is a curve, and this shows that the ratio of x to y

is variable; and moreover it is a curve *convex towards the line* AB, on which x is taken; and this circumstance proves that the ratio of y to x is continually increasing.

Though these experiments all tend to show that the ratio of y to x increases as x is increased, yet when we consider the subject with attention, we shall, I think, find reason to conclude that the exponent of that ratio can never be less than *unity*; and farther, that it must of necessity have *that value precisely*, when, the density being taken infinitely small, or $= 0$, x and y vanish together.

Supposing this to be the case, namely, that the exponent of the ultimate ratio of y to x is $= 1$, let the densities or successive values of x be expressed by a series of natural numbers,

0, 1, 2, 3, 4, &c. to 1000,

the last term $= 1000$ answering to the greatest density; or when the powder completely fills the space in which it is confined; then, by putting $z =$ the variable part of the exponent of the ratio of y to x ,

To each of the successive values of $x =$ } 0, 1, 2, 3, 4, &c.

The corresponding value of y will be accurately expressed by the equations } $0^{1+z}, 1^{1+z}, 2^{1+z}, 3^{1+z}, 4^{1+z}, \&c.$

For, as the variable part (z), of this exponent may be taken of *any dimensions*, it may be so taken at each given term of the series, (or for each particular value of x), that the equation $x^{1+z} = y$, may always correspond with the result of the experiments; and when this is done, the value of z , and the law of its increase as x increases, will be known; and this will show the relation of x to y , or of the elasticities of the ge-

nerated fluid to their corresponding densities, in a clear and satisfactory manner.

Without increasing the length of this paper still more (it being perhaps already too voluminous), by giving an account in detail of all the various computations I made, in order, from the results of the experiments in the foregoing table, to ascertain the real value of z , and the rate at which it increases as x is increased, I shall content myself with merely giving the general results of these investigations, and referring for farther information to the following table II, where the agreement of the law founded on them, with the results of the foregoing experiments, may be seen.

Having from the results of the experiments in table I. computed the different values of z , corresponding to all the different densities, or different charges of powder, from 1 grain, or 39 *thousandth parts*, to 18 grains, or 702 *thousandth parts* of the capacity of the barrel, I found that while the density of the elastic fluid = x , expressed in *thousandth parts*, is increased from 0 to 1000 (or till the powder completely fills the space in which it is confined), the variable part z of the exponent of x , $(1 + z)$ is increased from 0 to $\frac{4}{10}$. And though some of the experiments, and particularly those which were made with large charges of powder, seemed to indicate that while x is increased with an equable or uniform motion, z increases with a motion continually accelerated; *yet*, as the results of by far the greatest number of the other experiments showed the velocity of the increase of z to be *equable*, this circumstance, added to some other reasons drawn from the nature of the subject, have induced me to assume the ratio of the increase of z to the increase of x as constant.

But if, while x increases with an equable velocity from 0 to 1000, z is increased with an *equable velocity* from 0 to $\frac{4}{10}$, then it is every where z to x as $\frac{4}{10}$ to 1000; or $1000z = \frac{4}{10}x$, and consequently $z = \frac{4x}{10000}$; and when x is = 1, it is $z = \frac{4}{10000} = 0.0004$; and when x is greater or less than 1, it is $z = 0.0004x$; and z being expunged, the general equation expressing the relation of x to y becomes $x^{1+0.0004x} = y$; and this is the equation which was made use of in computing the values of y , as expressed in the following table.

In order that the elasticities might be expressed in atmospheres, the values of y , as determined by this equation, were multiplied by 1.841.

If it be required to express the elasticity in *pounds avoirdupois*, then the value of y , as determined by the foregoing equation, being multiplied by 27.615, will show how many pounds avoirdupois, pressing upon a superficial inch, will be equal to the pressure exerted by the elastic fluid in the case in question:

Table II. General Results of the Experiments in Table I. on the Force of fired Gunpowder.

The Charge of Powder.		Value of the Exponent $1+0.0004x$.	Computed Elasticity of the generated Fluid, or Value of y , according to the Theorem $x^{1+0.0004x} = y$.		Actual Elasticity, as shown by the Experiments.	Difference of the computed and the actual Elasticities.
In Grains.	In equal Parts.		In equal Parts.	In Atmospheres.	In Atmospheres.	
1	39	1.0156	41.294	76.822	77.86	+ 1.838
2	78	1.0312	89.357	164.506	182.30	+ 17.794
3	117	1.0468	146.210	269.173	228.2	- 40.973
4	156	1.0624	213.784	393.577	382.4	- 11.177
5	195	1.0780	294.209	541.640	561.2	+ 19.560
6	234	1.0936	389.919	717.841	685.6	- 32.241
7	273	1.1092	503.723	927.353	811.7	- 115.653
8	312	1.1248	638.889	1176.19	1164.8	- 12.390
9	351	1.1404	799.223	1471.37	1551.3	+ 79.930
10	390	1.1560	989.169	1821.06	1884.3	+ 63.240
11	429	1.1716	1213.91	2234.81	2219.	- 15.810
12	468	1.1872	1479.50	2723.77	2573.7	- 150.07
13	507	1.2028	1793.	3300.91	3283.3	- 17.61
14	546	1.2184	2162.69	3980.52	4008.	+ 27.48
15	585	1.2340	2598.18	4783.26	4722.5	- 60.76
16	624	1.2496	3110.73	5726.83	7090.	+ 1363.17
17	663	1.2652	3713.46	6836.46		
18	702	1.2808	4421.69	8140.34	10977.	+ 2836.66
19	741	1.2964	5253.3	9671.33		
20	780	1.3120	6229.14	11467.8		
25.641	1000	1.4000	15848.9	29177.9		

The agreement of the elasticities computed from the theorem $x^{1+0.0004x} = y$, with the actual elasticities as they were measured in the experiments, may be seen in the foregoing table; but this agreement may be seen in a much more striking manner by a bare inspection of the figure (Tab. VI.); for the line AD in this figure having been drawn from the computed elasticities, its general coincidence with the line AC shows how nearly the computed and the actual elasticities approach each other. And

when the irregularities of the line AC (which, as had already been observed, must be attributed to the unavoidable errors of the experiments), are corrected, these two curves will be found to coincide with much precision throughout a considerable part of the range of the experiments; but towards the end of the set of experiments, when the charges of powder were considerably increased, the elasticities seem to have increased faster than, according to the assumed law, they ought to have done. From this circumstance, and from the immense force the charge must have exerted in the experiment, when the barrel was burst, I was led to suspect that the elastic force of the fluid generated in the combustion of gunpowder, when its density is great, is still much greater than these experiments seem to indicate; and a farther investigation of the subject served to confirm me in this opinion.

It has been shown that the force exerted by the charge in the experiment in which the barrel was burst could not have been less than the pressure of 54,752 atmospheres; but the greatest force of the generated elastic fluid, when, the powder filling the space in which it is confined, its density is = 1000, on computing its elasticity by the theorem $x^{1+0.0004x} = y$, turns out to be only equal to 29,178 atmospheres.

In this computation the mean of the results of all the experiments in the foregoing set is taken as a standard to ascertain the value, expressed in atmospheres, of y , and it is $y \times 1.841 = 29,178$.

But if, instead of taking the mean of the whole set of experiments as a standard, we select that experiment in which the force exerted by the powder appears to have been the greatest,

yet in this case even the initial force of fired gunpowder, computed by the above rule, would be much too small.

In the experiment No. 84, when the charge consisted of 18 grains of powder, and the density or value of x was 702, a weight equal to the pressure of 10,977 atmospheres was raised. Here the value of y ($= x^{1+0.0004x}$) is found to be $(702^{1.2808})$, $= 4421.7$; and to express this value of y in atmospheres, and at the same time to accommodate it to the actual result of the experiment, it must be multiplied by 2.4826; for it is 4421.7 (the value of y expressed in equal parts) to 10,977 (its value in atmospheres, as shown by the experiment), as 1 to 2.4826, and consequently $4421.7 \times 2.4826 = 10,977$.

If now the value of y be computed on the same principles, when x is put $= 1000$, it will turn out to be $y = 1000^{1+0.4} = 15,849$; and this number expressed in atmospheres, by multiplying it by 2.4826, gives the value of $y = 39,346$ atmospheres.

This however falls still far short of 54,752 atmospheres, the force the powder was actually found to exert when the charge filled the space in which it was confined. But in the 84th experiment, when 18 grains of powder were used, as the weight (8081 lbs. avoirdupois) was raised with a *very loud report*, it is more than probable that the force of the generated elastic fluid was in fact considerably greater than that at which it was estimated, namely, greater than the pressure of 10,977 atmospheres.

But, without wasting time in fruitless endeavours to reconcile anomalous experiments, which, probably, never can be made to agree, I shall hasten to give an account of another

set of experiments; the results of which, it must be confessed, were still more various, extraordinary, and inexplicable.

The machinery having been repaired and put in order, the experiments were recommenced in July, 1793, the weather at that time being very hot.

The principal part of the apparatus, *the barrel*, had undergone a trifling alteration: upon refitting and cleaning it, the diameter of its bore at the muzzle was found to be a little increased, so that a weight equal to 8081 lbs. avoirdupois, instead of being equal to 10977 atmospheres (as was the case in the former experiments), was now just equal to the pressure of 9431 atmospheres.

Though I was not at Munich when this last set of experiments was made, they however were undertaken at my request, and under my direction, and I have no reason to doubt of their having been executed with all possible care. They were all made by the same persons who were employed in making the first set; and as these experimenters may be supposed to have grown expert in practice, and as they could not possibly have had any interest in deceiving me, I cannot suspect the accuracy of their reports.

Table III. Experiments on the Force of fired Gun powder.

No. of the Experiment.	Time when the Experiment was made. 1793.		State of the Atmosphere.		The Charge of Powder.		Weight employed to confine the elastic Fluid.		General Remarks.
			Thermom.	Barometer.	In Apoth. grs.	In 1000 parts of the capacity of the bore.	In lbs. avoirdupois.	In atmospheres.	
N ^o 86	1st July	h. m.	F.	Eng. In.	grs.	Parts.	lbs.		
		4 0	88°	28.37	17	663	808I	943I	{ The weight was raised with an astonishing loud report. In these three experiments the weight was raised with a very loud report.. Weight not raised.
87		4 30	—	—	—	—	—		
88		4 45	—	—	16	624	—		
89		5 0	—	—	15	585	—		{ Weight but just raised, report very weak. Raised, loud report.
90		5 30	—	—	12	468	—		
91		6 0	—	—	13	507	—	943I	{ Raised, report very feeble. Just moved, no report.
92	2d	9 0	71°	28.38	—	—	—		
93		9 30	—	—	12	468	—		{ Not raised. Not raised.
94		10 0	—	—	—	—	—	943I	
95		10 30	80°	—	11 $\frac{7}{8}$	—	—		{ Not raised. Not raised.
96	3d	10 0	70°	28.55	12	468	—		
97		10 30	—	—	13	507	—		{ Just raised, feeble report. Not raised.
98		11 0	75°	—	14	546	—	943I	
99	4th	9 0	70°	28.56	14	546	—		{ Not raised. Not raised.
100		9 30	—	—	—	—	—		
101		10 0	72°	—	15	585	—		{ The weight was raised, the report not very loud. Nearly as above.
102		10 30	—	—	15 $\frac{1}{2}$	—	—		
103	8th	9 0	74°	28.42	—	—	—		{ Raised, and with an uncommonly loud report. Raised, report very loud.
104		9 30	—	—	13	507	—		
105		10 45	85°	—	12	468	—	943I	{ But just raised, the report very feeble. Nearly as above.
106	17th	9 0	75°	28.4	—	—	—		
107		9 45	—	—	—	—	—		{ Not raised. Just moved, no report.
108		10 30	—	—	11 $\frac{1}{4}$	—	—		
109		11 0	—	—	—	—	—		The same as above.

It appears from the foregoing table, that in the afternoon of the 1st of July, the weight (which was a heavy brass cannon, a 24 pounder, weighing 8081 lbs. avoirdupois), was not raised by 12 grains of powder, but that 13 grains raised it with an audible though weak report. That the next morning, July 2d, at 10 o'clock, it was raised twice by charges of 12 grains. That in the morning of the 3d of July, it was not raised by 12 grains, nor by 13 grains; but that 14 grains just raised it. That in the afternoon of the same day, two experiments were made with 14 grains of powder, in neither of which the weight was raised; but that in another experiment, in which 15 grains of powder were used, it was raised with a moderate report. That in the morning of the 8th July, in two experiments, one with 15 grains, and the other with 13 grains of powder, the weight was raised with a *loud report*; and in an experiment with 12 grains, it was raised with a *feeble report*. And lastly, that in three successive experiments, made in the morning of the 17th of July, the weight was raised by charges of 12 grains.

Hence it appears, that under circumstances the most favourable to the developement of the force of gunpowder, a charge (= 12 grains) filling $\frac{468}{1000}$ of the cavity in which it is confined, on being fired, exerts a force against the sides of the containing vessel equal to the pressure of 9431 atmospheres; which pressure amounts to 141465 lbs. avoirdupois on each superficial inch.

Mr. ROBINS makes the initial, or greatest force of the fluid generated in the combustion of gunpowder, (namely when the charge completely fills the space in which it is confined), to

be only equal to the pressure of 1000 atmospheres. It appears, however, from the result of these experiments, that even admitting the elasticities to be as the densities, as Mr. ROBINS supposes them to be, the initial force of this generated elastic fluid must be at least twenty times greater than Mr. ROBINS determined it; for $\frac{468}{1000}$, the density of the elastic fluid in the experiments in question, is to 1, its density when the powder quite fills the space in which it is confined, as 9431 atmospheres, the measure of its elastic force in the experiments in question, to 20108 atmospheres; which, according to Mr. ROBINS's theory respecting the ratio of the elasticities to the densities, would be the measure of its initial force.

But all my experiments tend uniformly to prove, that the elasticities increase *faster* than in the simple ratio of the corresponding densities; consequently the initial force of the generated elastic fluid *must necessarily* be greater than the pressure of 20108 atmospheres.

In one of my experiments which I have often had occasion to mention, the force actually exerted by the fluid must have been at least equal to the pressure of 54752 atmospheres. The other experiments ought, no doubt, to show, at least, that it is *possible* that such an enormous force may have been exerted by the charge made use of; and this, I think, they actually indicate.

In the first set of experiments, which were made when the weather was cold, though the results of them uniformly showed the force of the powder to be much less than it appeared to be in all the subsequent experiments, made with greater charges, and in warm weather, yet they all show that the ratio

of the elasticity of the generated fluid to its density is very different from that which Mr. ROBINS's theory supposes; and that this ratio increases as the density of the fluid is increased.

Supposing (what on many accounts appears to be extremely probable) that this ratio increases uniformly, or with an equable celerity, while the density is uniformly augmented; and supposing farther, that the velocity and limit of its increase have been rightly determined from the result of the set of experiments, table I. which were made with that view; then, from the result of the experiments of which we have just been giving an account, (in which 12 grains of powder exerted a force equal to 9431 atmospheres), taking these experiments as a standard, we can with the help of the theorem ($x^{1+0.0004x} = y$) deduced from the former set of experiments, compute the initial force of fired gunpowder, thus:

The density of the elastic fluid, when 12 grains of powder are used for the charge, being = 468, it is $468^{1.1872} = y = 1479.5$; and in order that this value of y may correspond with the result of the experiment, and be expressed in atmospheres, it must be multiplied by a certain coefficient, which will be found by dividing the value of y expressed in atmospheres, as shown by the experiment, by the number here found indicating its value, as determined by computation.

It is therefore $\frac{9431}{1479.5} = 6.3744$ for the value of this coefficient, and this multiplied into the number 1479.5 gives 9431 for the value of y in atmospheres.

Again, the density being supposed = 1000 (or, that the charge of powder completely fills the cavity in which it is confined), in that case it will be $1000^{1+0.4} = y = 15849$; and this number being turned into atmospheres by being multiplied by

the coefficient above found ($= 6.3744$), gives 101021 atmospheres for the measure of the initial force of the elastic fluid generated in the combustion of gunpowder.

Enormous as this force appears, I do not think it over-rated; for nothing much short of such an inconceivable force can, in my opinion, ever explain in a satisfactory manner the bursting of the barrel so often mentioned; and to this we may add, that, as in 7 different experiments, all made with charges of 12 grains of powder, there were no less than 5 in which the weight was *raised with a report*, and as the same weight was *moved* in 3 different experiments in which the charge consisted of less than 12 grains, there does not appear to be any reason whatever for doubt with regard to the principal fact on which the above computation is founded.

There is an objection, however, that may be made to these decisions respecting the force of gunpowder, which, on the first view, appears of considerable importance; but on a more careful examination it will be found to have no weight.

If the force of fired gunpowder is so very great, how does it happen that fire-arms and artillery of all kinds, which certainly are not calculated to withstand so enormous a force, are not always burst when they are used? I might answer this question by another, by asking how it happened that the barrel used in my experiments, and which was more than ten times stronger in proportion to the size of its bore than ever a piece of ordnance was formed, could be burst by the force of gunpowder, if its force is not in fact much greater than it has ever been supposed to be? But it is not necessary to have recourse to such a shift to get out of this difficulty: there is nothing more to do than to show, which may easily be done, that the combustion of

gunpowder is less rapid than it has hitherto been supposed to be, and the objection in question falls to the ground.

Mr. ROBINS's theory supposes that all the powder of which a charge consists is not only set on fire, but that it is actually consumed and "*converted into an elastic fluid before the bullet is sensibly moved from its place.*" I have already in the former part of this paper offered several reasons which appeared to me to prove that, though the *inflammation* of gunpowder is very rapid, yet the progress of the combustion is by no means so *instantaneous* as has been imagined. I shall now give an account of some experiments which put that matter out of all doubt.

It is a fact well known that on the discharge of fire-arms of all kinds, cannon and mortars as well as muskets, there is always a considerable quantity of unconsumed grains of gunpowder blown out of them; and, what is very remarkable, and as it leads directly to a discovery of the cause of this effect is highly deserving of consideration, these unconsumed grains are not merely blown out of the *muzzles* of fire-arms; they come out also by their vents or touch-holes, *where the fire enters to inflame the charge*; as many persons who have had the misfortune to stand with their faces near the touch-hole of a musket, when it has been discharged, have found to their cost.

Now it appears to me to be extremely improbable, if not absolutely impossible, that a grain of gunpowder actually in the chamber of the piece, and completely surrounded by flame, should, by the action of that very flame, be blown out of it, without being at the same time set on fire. But if these grains of powder are *actually on fire* when they come out of the piece, and are afterwards found at a distance from it *unconsumed*,

this is, in my opinion, a most decisive proof, not only that the combustion of gunpowder is by no means so rapid as it has generally been thought to be, but also (what will doubtless appear quite incredible), that if a grain of gunpowder, actually on fire, and burning with the utmost violence over the whole extent of its surface, be projected with *a very great velocity* into a cold atmosphere, the fire will be extinguished, and the remains of the grain will fall to the ground unchanged, and as inflammable as before.

This extraordinary fact was ascertained beyond all possibility of doubt by the following experiments. Having procured from a powder-mill in the neighbourhood of the city of Munich a quantity of gunpowder, all of the same mass, but formed into grains of very different sizes, some as small as the grains of the finest Battel powder, and the largest of them nearly as big as large pease, I placed a number of vertical screens of very thin paper, one behind another, at the distance of 12 inches from each other; and loading a common musket repeatedly with this powder, sometimes without, and sometimes with a wad, I fired it against the foremost screen, and observed the quantity and effects of the unconsumed grains of powder which impinged against it.

The screens were so contrived, by means of double frames united by hinges, that the paper could be changed with very little trouble, and it was actually changed after every experiment.

The distance from the muzzle of the gun to the first screen was not always the same; in some of the experiments it was only 8 feet, in others it was 10, and in some 12 feet.

The charge of powder was varied in a great number of dif-

ferent ways, but the most interesting experiments were made with one single large grain of powder, propelled by smaller and larger charges of very fine-grained powder.

These large grains never failed to reach the screen; and though they sometimes appeared to have been broken into several pieces, by the force of the explosion, yet they frequently reached the first screen entire; and sometimes passed through all the screens (five in number), without being broken.

When they were propelled by large charges, and consequently with great velocity, they were seldom on fire when they arrived at the first screen, which was evident not only from their not setting fire to the paper (which they sometimes did), but also from their being found sticking in a soft board, against which they struck, after having passed through all the five screens; or leaving visible marks of their having impinged against it, and being broken to pieces and dispersed by the blow. These pieces were often found lying on the ground; and from their forms and dimensions, as well as from other appearances, it was often quite evident that the little globe of powder had been on fire, and that its diameter had been diminished by the combustion, before the fire was put out on the globe being projected into the cold atmosphere. The holes made in the screen by the little globe in its passage through them, seemed also to indicate that its diameter had been diminished.

That these globes or large grains of powder were always set on fire by the combustion of the charge can hardly be doubted. This certainly happened in many of the experiments, for they arrived at the screens on fire, and set fire to the paper; and in the experiments in which they were projected with small

velocities, they were often seen to pass through the air on fire; and when this was the case no vestige was to be found.

They sometimes passed, on fire, through several of the foremost screens without setting them on fire, and set fire to one or more of the hindmost, and then went on and impinged against the board, which was placed at the distance of 12 inches behind the last screen.

It is hardly necessary for me to observe, that all these experiments prove that the combustion of gunpowder is very far from being so instantaneous as has generally been imagined. I will just mention one experiment more, in which this was shown in a manner still more striking, and not less conclusive. A small piece of red-hot iron being dropped down into the chamber of a common horse pistol, and the pistol being elevated to an angle of about 45 degrees, upon dropping down into its barrel one of the small globes of powder (of the size of a pea), it took fire, and was projected into the atmosphere by the elastic fluid generated in its own combustion, leaving a very beautiful train of light behind it, and disappearing all at once, like a falling star.

This amusing experiment was repeated very often, and with globes of different sizes. When very small ones were used singly, they were commonly consumed entirely before they came out of the barrel of the pistol; but when several of them were used together, some, if not all of them were commonly projected into the atmosphere on fire.

I shall conclude this paper by some observations on the practical uses and improvements that may probably be derived from these discoveries, respecting the great expansive force of the fluid generated in the combustion of gunpowder.

As the *slowness* of the combustion of gunpowder is undoubtedly the cause which has prevented its enormous and almost incredible force from being discovered, so it is evident, that the readiest way to increase its effects is to contrive matters so as to accelerate its inflammation and combustion. This may be done in various ways, but the most simple and most effectual manner of doing it would, in my opinion, be to set fire to the charge of powder by shooting (through a small opening) the flame of a smaller charge into the midst of it.

I contrived an instrument on this principle for firing cannon three or four years ago, and it was found on repeated trials to be useful, convenient in practice, and not liable to accidents. It likewise supersedes the necessity of using priming, of vent tubes, port-fires, and matches; and on that account I imagined it might be of use in the British navy. Whether it has been found to be so or not I have not yet heard.

Another infallible method of increasing very considerably the effect of gunpowder in fire-arms of all sorts and dimensions, would be to cause the bullet to fit the bore exactly, or without windage, *in that part of the bore at least where the bullet rests on the charge*: for when the bullet does not completely close the opening of the chamber, not only much of the elastic fluid generated in the first moment of the combustion of the charge escapes by the sides of the bullet, but, what is of still greater importance, a considerable part of the unconsumed powder is blown out of the chamber along with it, in a state of actual combustion, and getting before the bullet continues to burn on as it passes through the whole length of the bore, by which the motion of the bullet is much impeded.

The loss of force which arises from this cause is, in some

cases, almost incredible; and it is by no means difficult to contrive matters so as to render it very apparent, and also to prevent it.

If a common horse pistol be fired with a loose ball, and so small a charge of powder that the ball shall not be able to penetrate a deal board so deep as to stick in it when fired against it from the distance of six feet; the same ball, discharged from the same pistol, with the same charge of powder, may be made to pass quite through one deal board, and bury itself in a second placed behind it, merely by preventing the loss of force which arises from what is called windage; as I have found more than once by actual experiment.

I have in my possession a musket, from which, with a common musket charge of powder, I fire two bullets at once with the same velocity that a single bullet is discharged from a musket on the common construction, with the same quantity of powder. And, what renders the experiment still more striking, the diameter of the bore of my musket is exactly the same as that of a common musket, except only in that part of it where it joins the chamber, in which part it is just so much contracted that the bullet which is next to the powder may stick fast in it. I ought to add, that though the bullets are of the common size, and are consequently considerably less in diameter than the bore, means are used which effectually prevent the loss of force by windage; and to this last circumstance it is doubtless owing, in a great measure, that the charge appears to exert so great a force in propelling the bullets.

That the conical form of the lower part of the bore, where it unites with the chamber, has a considerable share in producing this extraordinary effect, is however very certain, as I

have found by experiments made with a view merely to ascertain that fact.

I will finish this paper by a computation, which will show that the force of the elastic fluid generated in the combustion of gunpowder, enormous as it is, may be satisfactorily accounted for upon the supposition that its force depends *solely* on the elasticity of watery vapour, or steam.

It has been shown by a variety of experiments made in England, and in other countries, and lately by a well conducted set of experiments made in France by M. DE BETANCOUR, and published in Paris under the auspices of the Royal Academy of Sciences, in the year 1790, that the elasticity of steam is doubled by every addition of temperature equal to 30 degrees of FAHRENHEIT'S thermometer.

Supposing now a cavity of any dimensions (equal in capacity to 1 cubic inch, for instance) to be filled with gunpowder, and that on the combustion of the powder, and in consequence of it, this space is filled with steam (and I shall presently show that the water, existing in the powder *as water*, is abundantly sufficient for generating this steam); if we know the heat communicated to this steam in the combustion of powder, we can compute the elasticity it acquires by being so heated.

Now it is certain that the heat generated in the combustion of gunpowder cannot possibly be less than that of red-hot iron. It is probably much greater; but we will suppose it to be only equal to 1000 degrees of FAHRENHEIT'S scale, or something less than iron visibly red-hot in daylight. This is about as much hotter than boiling linseed oil, as boiling linseed oil is hotter than boiling water.

As the elastic force of steam is just equal to the mean pres-

sure of the atmosphere when its temperature is equal to that of boiling water, or to 212° of FAHRENHEIT'S thermometer, and as its elasticity is doubled by every addition of temperature equal to 30 degrees of the same scale, with the heat of $212^{\circ} + 30^{\circ} = 242^{\circ}$ its elasticity will be equal to the pressure of 2 atmospheres; at the temperature of $242^{\circ} + 30^{\circ} = 272^{\circ}$ it will equal 4 atmospheres;

at $272^{\circ} + 30^{\circ} = 302^{\circ}$ it will equal 8 atmospheres;

at $302^{\circ} + 30^{\circ} = 332^{\circ}$ ——— 16 ———

at $332^{\circ} + 30^{\circ} = 362^{\circ}$ ——— 32 ———

at $362^{\circ} + 30^{\circ} = 392^{\circ}$ ——— 64 ———

at $392^{\circ} + 30^{\circ} = 422^{\circ}$ ——— 128 ———

at $422^{\circ} + 30^{\circ} = 452^{\circ}$ ——— 256 ———

at $452^{\circ} + 30^{\circ} = 482^{\circ}$ ——— 512 ———

at $482^{\circ} + 30^{\circ} = 512^{\circ}$ ——— 1024 ———

at $512^{\circ} + 30^{\circ} = 542^{\circ}$ ——— 2048 ———

at $542^{\circ} + 30^{\circ} = 572^{\circ}$ ——— 4096 ———

at $572^{\circ} + 30^{\circ} = 602^{\circ}$, (or 2 degrees above the heat of boiling linseed oil,) its elasticity will be equal to the pressure of 8192 atmospheres, or above *eight times* greater than the utmost force of the fluid generated in the combustion of gunpowder, according to Mr. ROBINS'S computation. But the heat generated in the combustion of gunpowder is much greater than that of 602° of FAHRENHEIT'S thermometer, consequently the elasticity of the steam generated from the water contained in the powder must of necessity be much greater than the pressure of 8192 atmospheres.

Following up our computations on the principles assumed, (and they are founded on the most incontrovertible experiments) we shall find that,

at the temperature	}	the elasticity will be equal to
of		the pressure of
$602^{\circ} + 30^{\circ} = 632^{\circ}$		16,384 atmospheres;
at $632^{\circ} + 30^{\circ} = 662^{\circ}$	—	32,768 —
at $662^{\circ} + 30^{\circ} = 692^{\circ}$	—	65,536 —

and at $692^{\circ} + 30^{\circ} = 722^{\circ}$, the elasticity will be equal to the pressure of 131,072 atmospheres, which is 130 times greater than the elastic force assigned by Mr. ROBINS to the fluid generated in the combustion of gunpowder; and about *one sixth* part greater than my experiments indicated it to be.

But even here the heat is still much below that which is most undoubtedly generated in the combustion of gunpowder. The temperature which is indicated by 722° of FAHRENHEIT'S scale, (which is only 122 degrees higher than that of boiling quick-silver, or boiling linseed oil,) falls short of the heat of iron which is visibly red-hot in daylight by 355 degrees: but the flame of gunpowder has been found to melt brass, when this metal, in very small particles, has been mixed with the powder; and it is well known that to melt brass a heat is required equal to that of 3807 degrees of FAHRENHEIT'S scale; 2730 degrees above the heat of red-hot iron, or 3085 degrees higher than the temperature which gives to steam an elasticity equal to the pressure of 131072 atmospheres.

That the elasticity of steam would actually be increased by heat in the ratio here assumed, can hardly be doubted. It has absolutely been found to increase in this ratio in all the changes of temperature between the point of boiling water (I may even say of freezing water) and that of 280° of FAHRENHEIT'S scale; and there does not appear to be any reason why the same law should not hold in higher temperatures.

A doubt might possibly arise with respect to the existence of a sufficient quantity of water in gunpowder, to fill the space in which the powder is fired, with steam, at the moment of the explosion; but this doubt may easily be removed.

The best gunpowder, such as was used in my experiments, is composed of 70 parts (in weight) of nitre, 18 parts of sulphur, and 16 parts of charcoal; hence 100 parts of this powder contain $67\frac{3}{10}$ parts of nitre, $17\frac{1}{10}$ parts of sulphur, and of charcoal $15\frac{4}{10}$ parts.

Mr. KIRWAN has shown that in 100 parts of nitre there are 7 parts of water of crystallization; consequently, in 100 parts of gunpowder, as it contains $67\frac{3}{10}$ parts of nitre, there must be $4\frac{7\frac{1}{10}}{1000}$ parts of water.

Now as 1 cubic inch of gunpowder, when the powder is well shaken together, weighs exactly as much as 1 cubic inch of water at the temperature of 55° F. namely 253.175 grains Troy, a cubic inch of gunpowder in its driest state must contain at least $10\frac{9\frac{27}{1000}}{1000}$ grains of water; for it is 100 to 4.711, as 253.175 to 10.927. But besides the water of crystallization which exists in the nitre, there is always a considerable quantity of water in gunpowder, in that state in which it makes bodies *damp* or *moist*. Charcoal exposed to the air has been found to absorb nearly $\frac{1}{8}$ of its weight of water; and by experiments I have made on gunpowder, by ascertaining its loss of weight on being much dried, and its acquiring this lost weight again on being exposed to the air, I have reason to think that the power of the charcoal, which enters into the composition of gunpowder, to absorb water remains unimpaired, and that it actually retains as much water in that state, as it would retain were it not mixed with the nitre and the sulphur.

As there are $15\frac{4}{10}$ parts of charcoal in 100 parts of gunpowder, in 1 cubic inch of gunpowder (= 253.175 grains Troy,) there must be 38.989 grains of charcoal; and if we suppose $\frac{1}{8}$ of the apparent weight of this charcoal to be water, this will give 4.873 grains in weight for the water which exists in the form of *moisture* in 1 cubic inch of gunpowder.

That this estimation is not too high is evident from the following experiment. 1160 grains Troy of apparently dry gunpowder, taken from the middle of a cask, on being exposed 15 minutes in dry air, heated to the temperature of about 200°, was found to have lost 11 grains of its weight. This shews that each cubic inch of this gunpowder actually gave out $2\frac{4}{10}$ grains of water on being exposed to this heat; and there is no doubt but that at the end of the experiment it still retained much more water than it had parted with.

If now we compute the quantity of water which would be sufficient, when reduced to steam under the mean pressure of the atmosphere, to fill a space equal in capacity to 1 cubic inch, we shall find that either that contained in the nitre which enters into the composition of 1 cubic inch of gunpowder as *water of crystallization*, or even that small quantity which exists in the powder in the state of *moisture*, will be much more than sufficient for that purpose.

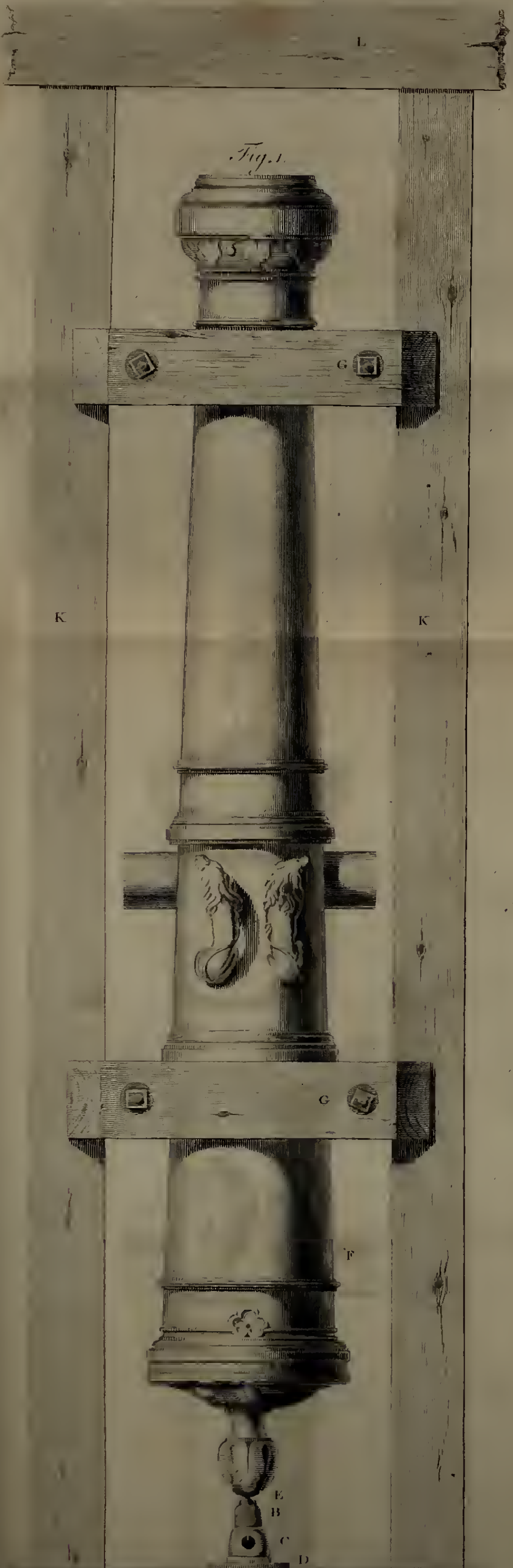
Though the density of steam has not been determined with that degree of precision that could be wished, yet it is quite certain that it cannot be less than 2000 times rarer than water, when both are at the temperature of 212°. Some have supposed it to be more than 10,000 times rarer than water, and experiments have been made which seem to render this opinion not improbable; but we will take its density at the highest

possible estimation, and suppose it to be only 2000 times rarer than water. As 1 cubic inch of water weighs 253.175 grains, the water contained in 1 cubic inch of steam at the temperature of 212° will be $\frac{1}{2000}$ part of 253.175 grains, or 0.12659 of a grain.

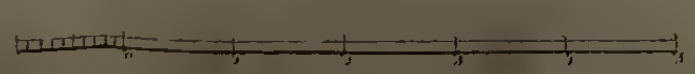
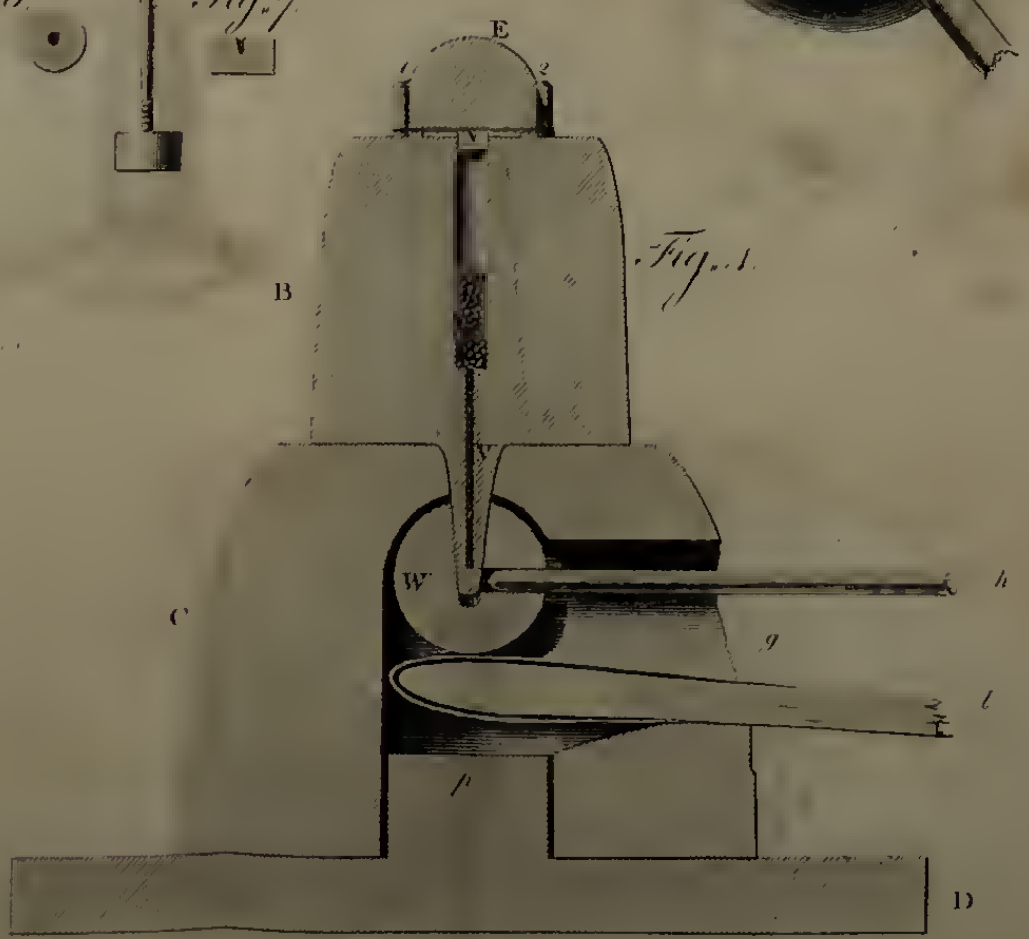
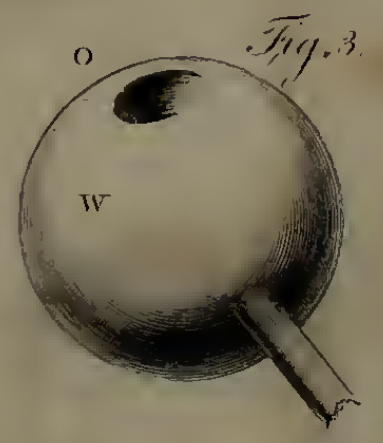
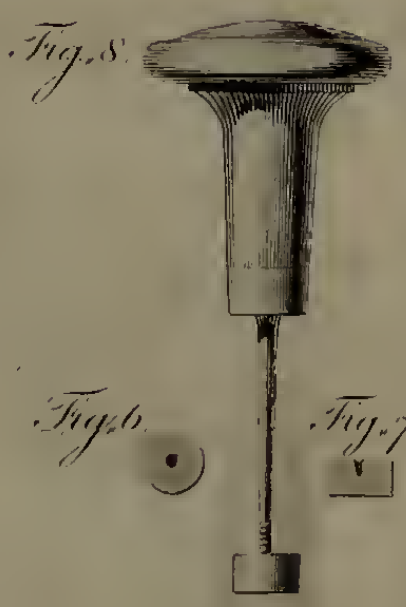
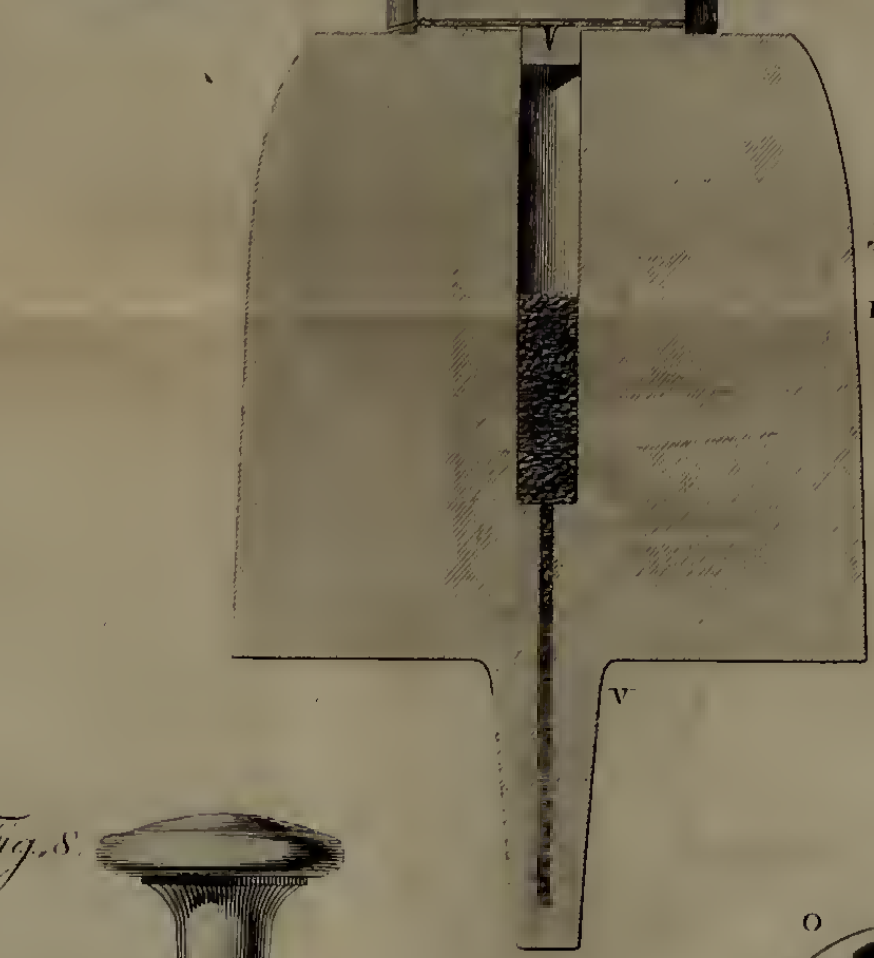
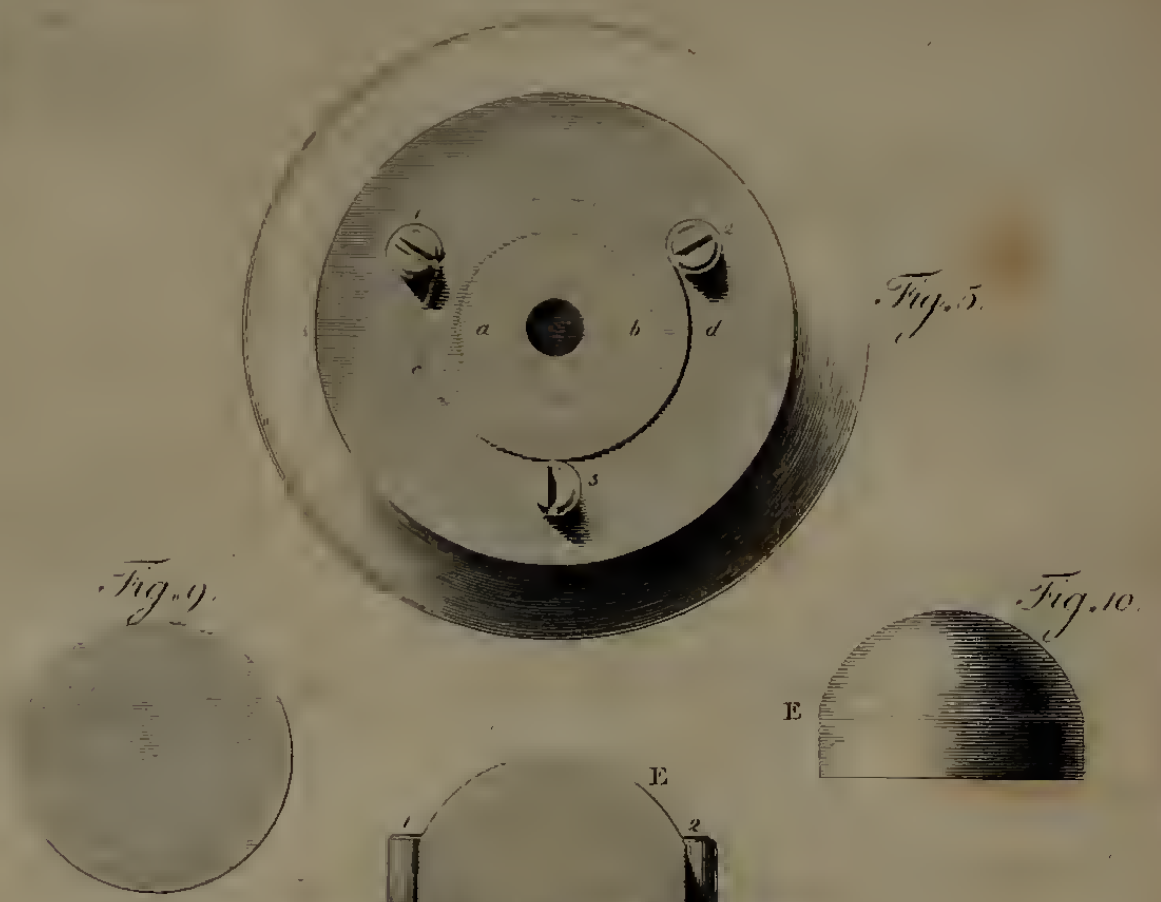
But we have seen that 1 cubic inch of gunpowder contains 10.927 grains of water of crystallization, and 4.873 grains in a state of moisture. Consequently the quantity of water of crystallization in gunpowder is 86 times greater, and the quantity which exists in it in a state of *moisture* is 38 times greater, than that which would be required to form a quantity of steam sufficient to fill completely the space occupied by the powder.

Hence we may venture to conclude, that the quantity of water actually existing in gunpowder is much more than sufficient to generate all the steam that would be necessary to account for the force displayed in the combustion of gunpowder (supposing that force to depend solely on the action of steam), even though no water should be generated in the combustion of the gunpowder. It is even very probable that there is more of it than is wanted, and that the force of gunpowder would be still greater, could the quantity of water it contains be diminished.

From this computation it would appear, that the difficulty is not to account for the force actually exerted by fired gunpowder, but to explain the reason why it does not exert a much greater force. But I shall leave these investigations to those who have more leisure than I now have to prosecute them.



1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20





XIII. *A Third Catalogue of the comparative Brightness of the Stars; with an introductory Account of an Index to Mr. FLAMSTEED'S Observations of the fixed Stars contained in the second Volume of the Historia Cœlestis. To which are added, several useful Results derived from that Index. By William Herschel, LL.D. F. R. S.*

Read May 18, 1797.

IN my earliest reviews of the heavens, I was much surprised to find many of the stars of the British catalogue missing. Taking it for granted that this catalogue was faultless, I supposed them to be lost. The deviation of many stars from the magnitude assigned to them in that catalogue, for the same reason, I looked upon as changes in the lustre of the stars. Soon after, however, I perceived that these conclusions had been premature, and wished it were possible to find some method that might serve to direct us from the stars in the British catalogue, to the original observations which have served as a foundation to it. The labour and time required for making a proper index, withheld me continually from undertaking the construction of it: but when I began to put the method of comparative brightness in practice, with a view to form a general catalogue, I found the indispensable necessity of having this index recur so forcibly, that I recommended it to my Sister to undertake the arduous task. At my request, and according

to a plan which I laid down, she began the work about twenty months ago, and has lately finished it.

The index has been made in the following manner. Every observation upon the fixed stars contained in the second volume of the *Historia Cælestis* was examined first, by casting up again all the numbers of the screws, in order to detect any error that might have been committed in reading off the zenith-distance by diagonal lines. The result of the computation being then corrected by the quantity given at the head of the column, and refraction being allowed for, was next compared with the column of the correct zenith-distance as a check.

Every star was now computed by a known preceding or following star; and its place according to the result of the computation laid down in the *Atlas Cælestis*, by means of proportional compasses. This was necessary, in order to ascertain the observed star: for the observations contain but little information on the subject; most of the small stars being without names, letters, or descriptions. The many errors in the names of the constellations affixed to the stars, and in the letters by which they are denoted, also demanded a more scrupulous attention; so that only their relative situation, examined by calculation, could ascertain what the stars really were which had been observed.

Every observed star being now ascertained, its number in the British catalogue was added in the margin at the end of the line of the observation; and a book with all the constellations and number of the stars of the same catalogue, with large blank spaces to each of them, being provided, an entry of the page where FLAMSTEED'S observation is to be found, was made in its proper place.

If the star observed was not in the British catalogue, it was marked as such in the margin of the observations; and being provided with another book of constellations and numbers, it was entered into the blank space belonging to some known preceding or following star, by which its place had been settled. The Greek and English letters used by FLAMSTEED, whether they were such as had been introduced before, or which he thought it expedient to add to them at the time of observation, were also entered into their proper places; and to complete the whole, the magnitude affixed to the stars was likewise joined to the entry made in the blank spaces of the index.

I have been so far particular in giving the method by which the index has been constructed, that it may appear what confidence ought to be given to the conclusions which will be drawn from its report.

About three or four examples of its use, will completely shew how the results, which will be mentioned, have been obtained.

Suppose I wish to be informed of the particulars relating to the 13th Arietis. Then by the index I am referred, in the column allotted for that star, to 77 observations; and find that FLAMSTEED used the letter α 72 times, and that in two places he calls it a star of the 2d magnitude; the rest of the observations being without any estimation of its brightness.

If it be required to know FLAMSTEED's observations upon the 34th Tauri, which star is supposed to have been the Georgian planet, mistaken by FLAMSTEED for a small fixed star; * we find in our index, that on page 86, December 13, 1690, a star of the 6th magnitude was observed, which answers to the

* See *Astronomisches Jahr-Buch* for 1789, page 202.

place of the 34th Tauri in the British catalogue; and that no other observation of the same star occurs in the second volume. In my catalogue of comparative brightness, the 34th Tauri is put down among the lost stars, it being no longer to be seen in the place where it was observed by FLAMSTEED.

If in my review of the heavens I cannot find 38 Leonis, and examine this index, I am at once informed that FLAMSTEED never observed such a star; and that of consequence it has been inserted in the British catalogue by some mistake or other. In many cases, these mistakes may be easily traced, as has been shewn with regard to this star in my second catalogue of comparative brightness. See the note to 38 Leonis.

When we wish to examine 90 Ceti in the heavens, and cannot find it, we are informed by our index, that 90 Ceti is the same star with 1 Eridani; and that, consequently, we are not to look out for two different stars.

We may now proceed to give some general results that are to be obtained from an inspection of our index. They are as follows.

111 Stars inserted in the British catalogue have never been observed by FLAMSTEED. This will explain why so many stars in the heavens seem to have been lost.

There are 39 stars in the same catalogue that want considerable corrections in right-ascension or polar-distance. In many it amounts to several degrees.

54 stars more, besides the 39 that are taken from the erroneous stars in the catalogue, want corrections in the *Atlas Cœlestis*; several of them also of many degrees.

42 stars are put down, which must be reduced to 21; each going by two names in different constellations.

371 stars, completely observed both in right-ascension and zenith-distance, have been totally overlooked.

35 more, which have one of the two, either right-ascension or polar-distance doubtful, have been omitted.

86 with only the polar-distance, and 13 with only the right-ascension, have also been unnoticed.

About 50 more that are pointed out by pretty clear descriptions, are likewise neglected; so that upon the whole between five and six hundred stars observed by FLAMSTEED, have been overlooked when the British catalogue was framed.

These additional stars will make a considerable catalogue, which is already drawn up and nearly finished by Miss HERSCHEL, who is in hopes that it may prove a valuable acquisition to astronomers.

Neither the index to FLAMSTEED's observations, nor the catalogue of omitted stars, were finished when my former two catalogues of comparative brightness were given; I shall therefore now select a few notes to be added to those which are at the end of these catalogues. They will contain such additional light as I have been enabled to gather from this newly acquired assistance.

Additional Notes to the Stars in the First Catalogue of the comparative Brightness of the Stars.

Aquarius.

25 Is the same star with 6 Pegasi. There are but two observations upon it. The first is on page 57; FLAMSTEED calls it "*in constellatione Pegasi sub capite.*" The second, on page

71, is described "*in constellatione Aquarii trianguli in capite præcedens et borealis.*" Here we see that the double insertion in the catalogue is owing to the star's having been called by different names in the observations. See also Mr. WOLLASTON'S catalogue, zone 88°.

27 Is the same with 11 Pegasi. There are three observations: the first places the star in the constellation of Pegasus, the two latter in that of Aquarius. See also Mr. WOLLASTON'S catalogue for this star, and others of the same kind.

65 Has not been observed by FLAMSTEED; notwithstanding which we find it inserted in my first catalogue, where its relative brightness is given. It should be considered that, in the first place, several stars of which there are no observations in the second volume of FLAMSTEED'S works, and which are, nevertheless, inserted in the British catalogue, such for instance as θ and ι Draconis, are well known to exist in the heavens. Now whether they were put into the catalogue from observations that are not in the second volume, or taken from other catalogues, it so happens that observations of them cannot be found. Therefore the want of a former observation by FLAMSTEED, is not sufficient to prove that a star does not exist. In the next place it should be recollected, that the method used to ascertain the stars in estimating their brightness, is not so accurate, as to point out with great precision the absolute situation of a star; and that, consequently, another star which happens to be not far from the place where the catalogue points out the star we look for, may be taken for it; especially when there are no neighbouring stars of the British catalogue that may induce us to exert uncommon attention in ascertaining the identity of such a star. MAYER, however, has an obser-

vation of 65 Aquarii in his zodiacal catalogue, No. 932, which puts the existence of the star out of doubt.

72 As the star neither was observed by FLAMSTEED, nor does exist, we cannot admit the remark which Mr. WOLLASTON in his catalogue, zone 95°, has upon MAYER's 939 star; where he supposes an error in declination of 3 degrees to have been committed, on a supposition of its being FLAMSTEED's 72.

80 Requires + 2' in time in RA, and therefore is not the star I have given, which requires — 1' 35'.

104 Which is without RA in the British catalogue, has three complete observations, page 8, 70, and 331.

Aquila.

29 Is without RA. There is but one observation of FLAMSTEED, page 53, which has no time. The RA is given by M. DE LA LANDE, in Mr. BODE's *Jabr-Buch* for 1796, page 163.

33 and 34 Which do not exist, were probably inserted by a mistake of one hour in the time of one of the observations on the two stars 68 and 69. In the zenith-distance, page 71 of FLAMSTEED's observation of 69 Aquilæ, for 53° read 55°.

40 and 43 Which do not exist, were probably also inserted by the same mistake of one hour in the RA of 70 and 71.

Capricornus.

1 and 2 Should be ξ' ξ'' . FLAMSTEED calls them so in his observations, and MAYER has also adopted the same letters in his catalogue, No. 821 and 822.

Cygnus.

5 Is without RA in the British catalogue; but the star has not been observed by FLAMSTEED.

9 Is without RA; FLAMSTEED, however, has a complete observation of it, page 67.

24 Has no RA. The time observed by FLAMSTEED is only doubtful in the seconds. Its RA has been given in Mr. BODE's *Jabr-Buch* for 1797, page 163.

33 Has no RA. FLAMSTEED never observed this star; but it is 3 Cephei Hevelii.

38 Has no RA in the British catalogue; but as the defective and only observation of FLAMSTEED on page 75, which might be supposed to belong to 38, will agree better with 43, it follows that he never observed 38.

68 Has no RA. There is a complete observation by FLAMSTEED, page 75.

78 Has no time in FLAMSTEED's observations. It is No. 146 in DE LA CAILLE's catalogue.

79 Has no RA. FLAMSTEED has but one observation, which is without time. Mr. BODE gives it in his *Jabr-Buch* for 1797, page 163.

Hercules.

24 Is the same with 51 Serpentis.

28 Is the same with 11 Ophiuchi.

54 There is no observation of this star. The zenith-distance of 55 was taken twice April 8, 1703 (instances of which we find in several other stars), which occasioned its being inserted as two stars.

63 There is no observation of this star, nor does it exist. The star of which the brightness is given in my catalogue, is at some distance from the place assigned in the British catalogue. FLAMSTEED observed a star, page 444, which will be No. 269 in Miss HERSCHEL'S manuscript catalogue. This, with an error in the calculation of the PD, probably occasioned the insertion of 63. And if this be the star, the PD of the British catalogue must be corrected $+ 3^{\circ}$.

71 Has never been observed by FLAMSTEED, nor does it exist. A small error in the calculation of one of the four observations of 70, may have produced it.

80 and 81 Were never observed. The two stars ν 24 and 25 Draconis, miscalled ι in FLAMSTEED'S observations, page 55 and 175, with an error of PD, accounts for the insertion of these stars. See Mr. BODE'S *Jahr-Buch* for 1787, page 194.

93 The PD is marked : : (doubtful), in the British catalogue; but the observation of FLAMSTEED, page 520, is complete.

Pegasus.

6 Is the same star with 25 Aquarii.

11 Is the same star with 27 Aquarii.

Additional Notes to the Stars in the Second Catalogue of the comparative Brightness of the Stars.

Aries.

1 There is an observation of a star by FLAMSTEED, which being calculated with an error of 10' of time in RA, would produce 1 Arietis; we may therefore correct the British cata-

logue RA $+ 10'$, and the star will be found to exist. In Miss HERSCHEL'S manuscript catalogue it is No. 143.

2 Is the same star with 107 Piscium.

38 is the same star with 88 Ceti. In three observations, page 85, 285, and 485, FLAMSTEED has called it Arietis; and on page 481 he has called it Ceti. See also Mr. BODE'S *Jahr-Buch* for 1793, page 200.

50 By FLAMSTEED'S observation, page 273, the catalogue requires — $1'$ in time of RA.

Cassiopea.

3 The place in the catalogue by two observations of FLAMSTEED requires $+ 5\frac{1}{4}'$ of time in RA, and $+ 7'$ of PD.

8 Is marked :: but has four complete observations on page 140, 144, 145, and 147.

29 There is an observation of FLAMSTEED on page 144 which has produced this star, but the time of it requires a correction of $+ 6'$; and it will then belong to 32. That this correction should be used, will appear when we compare this observation with another on page 213. In both places a star which is not inserted in the British catalogue, but which is No. 384 of Miss HERSCHEL'S manuscript catalogue, was taken at the same time. On page 144 it is "Duarum infra γ , versus polum, borealis. Simul fere transit, austrea;" and on page 213 we have "post transitum" for the new star, and "cum priore" for 32; and in both places the zenith-distance perfectly shews that they were the same stars: the 32d and a star south of it. And they are now both in the places where FLAMSTEED has observed them.

30 FLAMSTEED has no observation of this star. It is μ 21 Cassiopeæ Hevelii.

33 FLAMSTEED observed no RA of this star. It is θ 23 Cassiopeæ Hevelii.

34 Is wrong in the catalogue. By two observations of FLAMSTEED, page 144 and 521, it requires a mean correction of $-9'$ of time in RA. In this case my double star III. 23 will no longer be ϕ 34 Cassiopeæ, but a star $9'$ of time preceding ϕ ; for it exists in the place where 34 is put in Atlas, according to the erroneous catalogue, and is rather larger than FLAMSTEED's star ϕ .

35 The RA is marked :: The single observation, page 207, has the time marked *circiter*, being probably set down to the nearest minute only; and by the same observation the PD requires $+20'$.

47 Is also marked :: but has one complete observation, page 149.

51 The observation of FLAMSTEED which produced this star should be corrected $+1$ hour. This makes it 37 Cassiopeæ Hevelii.

52 and 53 By FLAMSTEED's observation page 208, should be the reverse in PD of what they are.

Cetus.

14 If we correct the British catalogue $+3'$ in PD, it will become a star observed by FLAMSTEED, which is No. 312 in Miss HERSCHEL's manuscript catalogue.

26 FLAMSTEED has no observation of this star; but we find it in DE LA CAILLE's zodiacal catalogue, No. 10.

51 Is the same with 106 Piscium. FLAMSTEED has 23 ob-

servations of the star, and has always called it ν , except once on page 482, where it is without letter, and where the constellation is marked Aquarii; now, as there was immediately following an observation of ζ Ceti, and Aquarius was evidently wrong, the star has been put in Cetus.

58 By FLAMSTEED'S observation, page 358, the RA in the British catalogue requires a correction of $-3'$ in time.

74 FLAMSTEED has no observation of this star, nor can I find it in any other catalogue. The place of it is so distant from other stars of the British catalogue, that my estimation of brightness may belong to some star not far from the situation assigned, and that the star of the British catalogue may not exist.

88 Is the same with ζ Arietis. See Mr. BODE'S *Jabr-Buch* for 1793, page 200.

Eridanus.

44 In the British catalogue is marked :: The single observation of FLAMSTEED, page 153, is perfect, all but a difference of $5'$ between the zenith-distance by the diagonal lines and by the screw.

45 Marked :: has a complete observation, page 153.

68 Marked :: has a complete observation, page 146.

Gemini.

50 There is no observation on this star. The star I have given is at a considerable distance from the place assigned by the British catalogue, so that in fact the star of the catalogue does not exist. It has been inserted in the British catalogue by a mistake in the calculation of a star which is about $1^{\circ}49'$ more

south. This will be No. 139 in Miss HERSCHEL'S manuscript catalogue, and it is probably the real intended 50 of FLAMSTEED. The expression of its brightness 41,50 of my catalogue will do very well for it.

70 and 71 By FLAMSTEED'S observations should be called π^1 , and π^2 . TYCHO and HEVELIUS also call 71 π .

72 and 73 Have been inserted by a mistake in 64 and 65. See Mr. BODE'S *Jabr-Buch* for 1788, page 175.

76 FLAMSTEED has no observation of this star. It is, however, MAYER'S No. 310.

80 Is not π , but according to FLAMSTEED'S observation *quæ sequitur* π ; and has no letter.

Leo.

10 Is the same with 1 Sextantis.

25 This star does not exist in the place where the British catalogue gives it; but if we admit that it has been inserted by a mistake in the calculation of 10 Sextantis, it may be taken into the constellation of Leo, as a star inserted in two constellations; and it will then be "25 is the same with 10 Sextantis."

26 In FLAMSTEED'S observations, page 299, the *strias cochleæ* give 26' less than the *lineas diagonales*. The former are right; therefore the British catalogue must be corrected PD — 26'.

28 FLAMSTEED has no observation of this star. It was probably inserted by a mistake in calculating an imperfect observation of 11 Sextantis. If this be allowed, we then must say "28 is the same with 11 Sextantis."

66 FLAMSTEED has no observation of this star. There is

a small star near the place where the British catalogue has given it, of which I have expressed the brightness; but as its situation is not exactly where it ought to be, my catalogue should have, "does not exist."

67 Is the same with 53 Leonis minoris.

71 May have been inserted by a mistake in one of the three observations of 73; putting the star north of θ instead of south.

III. Catalogue of the comparative Brightness of the Stars.

Lustre of the stars in Andromeda.			
1	o	3.4	15-1-16
2		6	20-2, 4
3		6	8.3
4		6	2, 4, 6
5		6	11, 5
6		6.7	4, 6
7		5.6	7-8
8		6	7-8, 11 8.3
9		6	10.9
10		6.7	13-10.9
11		6	8, 11, 5
12		6	15.12, 13
13		6	12, 13-10
14		6	14, 15
15		6	14, 15.12
16	λ	4	16-17 1-16
17	ι	4	16-17, 19 19, 17
18		6	20.18
19	κ	4	17, 19-20 19, 17
20	ψ	5.6	19-20 20-2 22-20.18 22-20-23
21	α	2	21, 43 21, 8 Pegasi 21, 43 21-43
22		5	22-20
23		6	20-23, 26
24	θ	4.5	25, 24-27
25	σ	5	25, 24
26		6	23, 26
27	ε	5	24-27
28		6	29-28 32.28, 40

Lustre of the stars in Andromeda.

29	π	4.5	30. 29 - 28	29, 35
30	ϵ	4	37 - 30. 29	
31	δ	3	4 Trianguli =	31 -, 2 Trianguli
32		6	35 - 32. 28	32 - 39
33		Neb.	is a Nebula	
34	ζ	4	35, 34, 38	
35	ν	4	29, 35 - 32	35, 34 35 - 48 50 -, 35, 53
36		6	38 -, 36	
37	μ	4.3	37 - 30	37 - 50
38	η	4.5	34, 38 -, 36	
39		6	32 - 39	
40		6	28 $\bar{7}$ 40	
41	d	5	42 -, 41. 45	
42	ϕ	5	54 ; 42 -, 41	
43	β	2	21, 43. 57	21 $\bar{7}$ 43 ; 57 21 - 43, 57 43 $\bar{7}$ 13 Ari 43 - 13 Ari 43 $\bar{7}$ 57
44		6	45. 44	
45		5.6	41. 45. 44	45. 47
46		4.5	48, 46, 49	
47		6	45. 47	
48		5	35 - 48, 46	
49	ξ	5	46, 49	
50	ν	6.5	37 - 50 -, 35	
51	ν	5	51 - 1	
52	λ	6	53, 52. 55	
53	τ	5	35, 53, 52	58, 53 - 56 53, 60
54	ϕ	4	54 ; 42	
55		Neb.	52. 55	
56		6	53 - 56. 59	60, 56

Lustre of the stars in Andromeda.

57	γ	2.3	43.57	57; 13 Arietis	43; 57	43, 57
			43	$\bar{57}$		
58		6	58, 53			
59		6	56.59			
60	<i>b</i>	6	53, 60, 56			
61		6	63, 61	66, 61		
62	<i>c</i>	6	65, 62			
63		6	64.63, 61	6 Persei, 63		
64		6	65-64.63			
65		5	65-64	65, 62	65, 6 Persei	
66		6.7	66; 61			

Lustre of the stars in Bootes.

1		6	7, 1	6-1, 2		
2		6	1, 2.10			
3		6	11.3			
4	τ	4	5 $\bar{7}$ 4-6			
5	ν	4	5 $\bar{7}$ 4	30-, 5, 35		
6		5.6	6, 7	4-6-1		
7		7	6, 7, 1	7-26		
8	η	3	8, 27	79 Virginis; 8	8-27	36-8
9		5	12 $\bar{7}$ 9-11			
10	<i>e</i>	7	2.10			
11		7.6	9-11.3			
12	<i>d</i>	5	28; 12 $\bar{7}$ 9			
13		6	13-24			
14		6	18, 14, 15			
15		6	14, 15			
16	α	1	16--3	Lyræ		
17	κ	4	21.17			

Lustre of the stars in Bootes.			
18		6	20, 18, 14
19	λ	4	19. 23
20		5	20, 18 20; 22
21	ι	4	23, 21. 17
22	f	5	20; 22
23	θ	4	19. 23, 21
24	g	6. 7	13-24
25	q	4	25--51
26		7	7-26 34--26
27	γ	3	8, 27-49 27-, 49 8-27 27-, 42
28	σ	5	51-28 28; 12
29	π	4. 3	35, 29
30	ζ	3	30-, 5
31		5	35-31-32
32		6	31-32
33	b^1	6	39. 33-38
34		6	34--26
35	o	4. 5	5. 35, 29 37. 35-31
36	ϵ	3	5 Coronae-36-8
37	ξ	4	37. 35
38	b^2	6	33-38
39		6	47. 39 39. 33
40		6. 7	47-40
41	ω	5	45; 41-46 41, 48 41. 50
42	β	3	49, 42 42 49 27-, 42. 49 42; 49
43	ψ	5	43-45
44		6	44, 47
45	c	5	43-45; 41
46	b	6	41-46 48, 46
47	k	5	44, 47. 39 47-40
48	χ	5	41, 48, 46

Lustre of the stars in Bootes.

49	δ	3	27 -, 49 42 $\frac{1}{2}$ 49	42 -, 49 27 - - 49	42 . 49	42 , 49
50		5	41 . 50			
51	μ	4	25 - - 51 - 28	4 Coronæ , 51 , 7 Coronæ		
52	ν^1	6	53 ; 52 , 54			
53	ν^2	6	53 ; 52			
54	ϕ	6	52 , 54			

Lustre of the stars in Cancer.

1		6	5 . 1			
2	ω^1	6	9 , 2 , 4	14 , 2 , 4		
3		6	16 -, 3 , 5	8 - 3 , 12		
4	ω^2	6	2 , 4 . 13			
5		6	3 , 5 . 1			
6	χ	5	6 - 14	6 - 15	6 $\bar{1}$ 18	
7		8	9 , 7			
8		6	8 - 3			
9	μ^1	7	10 - 9 , 2	9 , 7		
10	μ^2	5	10 - 9			
11		6	14 , 11	15 - 11		
12		6	3 , 12			
13	ψ^1	6.7	4 . 13			
14	ψ^2	4	14 , 2	6 - 14 , 11		
15	ψ^3	5	6 - 15 - 11			
16	ζ	5.6	43 . 16 -, 3			
17	β	4.3	17 $\bar{1}$ 47	17 , 48		
18	χ	6	6 $\bar{1}$ 18 , 23			
19	λ	6	19 - 30 , 28			
20	d^1	6	31 , 20 , 25			
21		6	37 , 21 , 34	29 - 21		

Lustre of the stars in Cancer.			
22	ϕ^1	6.7	23, 22
23	ϕ^2	6	18, 23, 22
24	ν^1	6	32. 24
25	d^2	6	20, 25
26	ϕ^3	6	Does not exist.
27		6	27; 29
28	ν^2	6.7	30, 28, 32
29		6.7	27; 29 - 21
30	ν^3	6	19 - 30, 28
31	θ	6.5	31, 20 31. 33
32	ν^4	7.8	28, 32. 24
33	η	6.7	31. 33
34		6	21, 34. 36
35		7	42; 35. 38
36	c^1	6	34. 36
37	c^2	6	49 - 37, 21
38	o	8	42; 38. 40 35. 38
39		6	39, 41
40		6	38. 40
41	ϵ	7	39, 41. 42
42	c	7.8	41. 42; 38 42; 35
43	γ	4	43. 16 47 -, 43
44		6	20 --- 44 4 --- 44
45	A^1	6	76, 45. 60
46		6	55; 46. 61
47	δ	4	17 -, 47 -, 43 65. 47 -- 76 48, 47
48	i^1	5	17, 48, 47 48 -- 58
49	b	6	49 - 37
50	A^2	6	60, 50
51	σ^1	6	51. 64
52		6	54 - 52

Lustre of the stars in Cancer.

53	ρ^1	6	55; 53
54		7	62-, 54-52 82, 54, 81
55	ρ^2	6	58-55; 53 67.55, 70 57-55; 46
56	ρ^3	6	Does not exist.
57	i^2	5.6	58; 57-55 57, 72
58	ρ^3	6	48--58-55 58, 75 58; 57
59	σ^2	5.6	64.59.66
60	α^1	4.5	45.60, 50
61		6	46.61
62	σ^1	6	63.62-, 54
63	σ^2	6	63.62
64	σ^3	6	51.64.59
65	α^2	4	65.47
66	σ^4	6	59.66
67	ρ^4	6.7	67.55
68		6	81-68, 71 68.78 68; 80
69	ν	6	69; 77
70	ρ^5	6.7	55, 70
71		7	68, 71 78, 71
72	τ	6.7	57, 72
73		6	Does not exist.
74		6	Does not exist.
75		6.7	58, 75
76	κ	4.5	47--76, 45
77	ξ	5.6	69; 77-, 79
78		6	68.78, 71 83, 78 80; 78, 71
79		8	77-, 79
80		7	80-83 68; 80; 78
81	π	7	54, 81 81-68 81, 83
82		6	82, 54
83		6	81, 83 80-83, 78

Lustre of the stars in Centaurus.

1	<i>i</i>	4.5	3. 1. 5
2	<i>g</i>	4.5	5-2
3	<i>k</i>	4.5	4. 3. 1
4	<i>b</i>	4.5	4, 3
5	<i>θ</i>	2.3	1. 5-2

Lustre of the stars in Cepheus.

1	<i>α</i>	5	1. 17
2	<i>θ</i>	5	3-, 2
3	<i>η</i>	4	3 ² , 3-, 2 21 $\frac{1}{2}$ 3 3 ² $\frac{1}{2}$ 3
4		6	6-4. 7
5	<i>α</i>	3	5-, 37 Cygni 5. 37 Cassiopeæ
6		6	6-4
7		6	4. 7
8	<i>β</i>	3	35-, 8-, 3 ²
9		6	17-9-12 11, 9
10		5	10. 17
11		5	11, 9
12		7	9-12
13	<i>μ</i>	6	13, 14
14		6	13. 14-, 15
15	<i>ν</i>	7.6	14-, 15-15
16		5.6	24, 16, 78 Draconis
17	<i>ξ</i>	5	1. 17, 33 10. 17-9 23, 17--30
18			19, 18; 20
19		6	22. 19, 18
20		6	18; 20
21	<i>ζ</i>	4.5	21 $\frac{1}{2}$ 3
22	<i>λ</i>	6	22. 19
23	<i>ε</i>	4	23, 17
24		5.6	24, 16

Lustre of the stars in Cepheus.

25		7	26-, 25
26		6	30-, 26-, 25
27	δ	4.5	32, 27 27.23 21-27-23 21 = 27
28		6	28-29
29	ε	6	28-29
30		6	17--30-, 26
31		6	34-31
32	ι	4	8-, 32.3 32; 3
33	π	5	17, 33
34	ο	5	34-31
35	γ	3	35-, 8

Lustre of the stars in Corona Borealis.

1	ο	6	2--1
2	η	5	2--1
3	β	4	8; 3-13
4	θ	4.5	13; 4-10 4, 7
5	α	2.3	55 Ophiuchi, 5 5-36 Bootis
6	μ	5	11-6, 9
7	ζ	4	4, 7, 10
8	γ	4	8; 3
9	π	5	6, 9 12, 9
10	δ	4	4-10 7, 10
11	κ	5	11-6
12	λ	5	12, 9
13	ε	4.5	3-13; 4
14	ι	5.6	14, 19
15	ρ	6	17, 15
16	τ	6	16-17
17	σ	6	16-17 17, 15
18	υ	6	19-, 18

Lustre of the stars in Corona Borealis.			
19	ξ	5	14, 19, 18
20	ν^1	5	20 = 21
21	ν^2	5	20 = 21
Lustre of the stars in Lacerta.			
1		5	7, 1, 8 1, 1 Hevelii . 6
2		5	7, 2, 5
3		4.5	4.3, 9
4		5	5.4.3
5		4.5	7-5 2, 5.4
6		5	7-6, 11 1 Hevelii . 6
7		4	7-5 7, 2 7-6 7, 1
8		6	1, 8, 10
9		6	3, 9
10		6	8, 10, 12
11		5	6, 11, 15
12		6	10, 12
13		6	15, 13, 14
14		6	13, 14, 16
15		5	11, 15, 13
16		6	14, 16
Lustre of the stars in Lepus.			
1		9	7-1 10. 1. 12
2	ϵ	4	5, 2, 13
3	ι	5	3, 6
4	κ	5	6, 4, 7 4, 8
5	μ	4	9, 5, 2 5, 14
6	λ	4.5	3, 6, 4
7	ν	5.6	4, 7 8; 7-1
8		6	4, 8; 7

Lustre of the stars in Lepus.

9	β	3	11-9, 5
10		6	10.1
11	α	3	11-9
12		6	1.12
13	γ	3.4	2, 13, 15
14	ζ	4	5, 14, 16
15	δ	4.3	13, 15
16	η	4	14, 16-18
17		6	18, 17-, 19
18	θ	4	16-18, 17
19		6	17-, 19

Lustre of the stars in Navis.

1		6	686 De la Caille - 1 - 12
2		6	5.2.10
3	τ	4.5	3, 11
4		6	4, 9 4, 6
5		6	9.5.2
6		5	4, 6.9
7	ξ	3.4	15--7 = 7 11
8		5.6	10, 8
9		4	4, 9.5 6.9
10		6	2.10, 8
11	e	4	7 = 7 11 - 12 11.16 11-, 12 3, 11, 665 De la Caille.
12		6	11-12 11-, 12-1
13		4	13, 13 Canis min. 13-13 Canis min.
14		6	16-14 16-, 14
15	i	3	15, 31 Canis maj. 15--7
16		5	11.16-14 16-, 14
17		6	20-17-, 18

Lustre of the stars in Navis.					
18		6	20, 18-	22	
19		4.5	19, 20		
20		5.6	19, 20-21	19, 20, 18	20-, 21 20-17
21		6	20-21	20-, 21	
22		6	18-, 22		
Lustre of the stars in Orion.					
1		4	1-, 3	1-8	
2	π^1	4	3--2-7		
3		4	1-, 3	8, 3--2	3, 9
4	σ^1	4.5	9-4	11, 4, 15	4--9 ^b Tauri
			4, 97	Tauri.	
5		6	10-, 5		
6	g	6	7-6, 14		
7	π^2	6	2-7-6		
8	z	4	1-8, 3	8-, 10	
9	σ^2	4.5	3-9-4		
10		4.5	8-, 10-, 5		
11	γ^1	5	11, 4		
12		6	Does not exist.		
13		6	16-13	18-13	
14	i	5	6, 14; 16		
15	γ^2	5	4, 15-35		
16	b	6	14; 16-13	16. 18	
17	ξ^1	4.5	25, 17-21		
18		6.5	16. 18-13		
19	β	1	19 $\bar{7}$ 10 Canis min.	19 = $\bar{7}$ 87 Tauri	
			19-, 10 Canis min.		
20	τ	4	20, 29	28-20 = 29	
21		6	17-21		
22		5	22, 27	22. 31	22-11 Monocerotis

Lustre of the stars in Orion.

23	<i>m</i>	6	30, 23, 38
24	γ	2	112 Tauri -, 24 - 46 24 -, 46
25	ψ^1	5	25, 17 47, 25, 30
26		6*	Does not exist.
27	ρ^2	6	22, 27 31, 27
28	η	3	44 - 28, 48 28 - 20
29	<i>e</i>	5	20, 29, 36 20 = 29; 53
30	ψ^2	5	25, 30, 23
31		6	22, 31, 27
32	<i>A</i>	5	32, 47
33	<i>n</i>	6	38, 33
34	δ	2	50 - 34; 53 50 -, 34 53; 34
35		6	15 - 35
36	<i>v</i>	4	29, 36 - 49
37	ϕ^1	5	40 - 37 61, 37
38		6	23, 38, 33
39	λ	4	39 - 40
40	ϕ^2	5	39 - 40 - 37 40, 61
41	θ^1	6	41, 43
42	<i>c^1</i>	5	42, 45
43	θ^2	4	41, 43
44	<i>r</i>	3, 4	44 - 28
45	<i>c^2</i>	5	42, 45
46	ϵ	2	46, 50 - 34 24 - 46 46 - 30 Hydræ 46 - 50 24 -, 46
47	ω	5	32, 47, 25
48	σ	4	28, 48
49	<i>d</i>	5	36 - 49 49 - 55
50	ζ	2	50, 24 Gemin 46, 50 - 34 46 - 50 -, 34 50 - 24 Gemin
51	<i>b</i>	5	50; 51; 52

Lustre of the stars in Orion.			
52		6	51; 52, 60
53	α	3	35; 53 29; 53 30 Hyd - 53 53; 34
54	χ^1	5	54--57 54-62
55		6	49-55
56		6	56; 51
57	χ^2	5	54--57 68, 57
58	α	1	58. 10 Canis min. 58--87 Tauri 58-, 10 Canis min.
59		6	60-, 59
60		6	52, 60-, 59
61	μ	4	40, 61, 37
62	χ^3	6	54-62-64
63		6	66. 63 66; 63
64	χ^4	6	62-64
65	χ^5	5.6	Does not exist.
66		6	66. 63 66; 63
67	ν	4.5	67; 70 67, 70
68		6	71, 68, 57
69	f^4	6	70-69. 72
70	ξ	4.5	67; 70-, 74 70-75 67, 70-69
71		6	71, 68
72	f^2	6	69. 72
73	k^1	6	74; 73
74	k^2	6	70-, 74; 73 75, 74
75	l	6	70-75, 74
76		6	Does not exist.
77		6	77-78
78		6	77-78

Notes to Andromeda.

1 By three observations of FLAMSTEED, page 130, 138, and 140, the polar-distance in the edition of 1725 requires $+ 9^{\circ}$.

40 Is the same with 69 Piscium. FLAMSTEED observed it five times; twice among the stars of the constellation Pisces, and three times among those of Andromeda. See page 14, 134, 139, 149, and 210.

61 M. DE LA LANDE says is lost. See Mr. BODE's *Fabr-Buch* for 1794, page 97; but as the star is now in its place, it may perhaps be changeable, and ought to be looked after.

Notes to Bootes.

47 The RA in the British catalogue is only given to the nearest degree, and Mr. BODE and Mr. WOLLASTON, in their catalogues, have left it out; but FLAMSTEED has four complete observations of it, on page 166, 168, 414, and 415, and the star is called *k* in all of them.

Notes to Cancer.

26 Was not observed by FLAMSTEED. An observation on page 297 has occasioned the insertion of this star; but by correcting the time — 1', it will agree with two other observations of 22 Cancri on page 21 and 26. See Mr. BODE's *Fabr-Buch* for 1788, page 172.

56 This star has not been observed by FLAMSTEED, nor does it exist. Page 25 FLAMSTEED observed 55 Cancri with a memorandum, "*Hec habet comitem sequentem ad austrum;*" which has probably occasioned the insertion of this star; but he had not then observed all the ϱ 's, and might possibly mean

to point out ρ 53; which he afterwards observed on page 27. The stars are so near together that he might easily mistake *sequens* for *præcedens ad austrum*. FLAMSTEED in his observations calls 58 3d ρ , 67 4th ρ , and 70 5th ρ ; this shews that there is no authority for six ρ 's. See Mr. BODE's account of the same star in his *Jabr-Buch* for 1788, page 171.

71 "April 5, 1796. 71 Cancri is 15' nearer to 78 and 15' "farther from 68 than it is placed in Atlas."

73 and 74 Have not been observed by FLAMSTEED, nor do they exist. How they came to be inserted, does not appear to be satisfactorily accounted for by Mr. BODE in his *Jabr-Buch* for 1788, page 172. He gives us four observations of 62 and 63 Cancri; but FLAMSTEED has thirteen, and they are all perfect except the last on page 564.

Notes to Cepheus.

15 "October 25, 1796. 15 Cephei consists of two stars. "Both taken together for one, by the naked eye, give 14 . 15 "In the telescope they are 14 - , 15 - 15."

18 Has no time in FLAMSTEED's observations. "March 26, "1797. 18 is a very little preceding 19. It is $1\frac{1}{2}$ degree from "17. The stars 18, 20 and 19 are in a line which bends a "little at 18 towards the preceding side."

Notes to Corona Borealis.

21 In the British catalogue requires a correction of - 28' 21" in time of RA and - 14' 55" in PD. In the place where it is marked in Atlas, according to the erroneous catalogue, is no star; but very unaccountably it is also marked in its right place in the same Atlas. FLAMSTEED has four complete obser-

vations of it on page 167, 445, 477, and 478. Mr. WOLLASTON not being acquainted with the existence of $\alpha 1$ Coronæ in its right place, supposes zone 55° , that I have made a mistake in calling my double star VI. 18, very unequal; but in his corrections he gives us the place of a star, as he calls it "near ν ," which is the real second ν of FLAMSTEED; who very particularly describes it on page 167, "*Duarum ad ν sequens et clarior;*" and this is the double star I have given in my catalogue as $\alpha 1$ Coronæ.

Notes to Navis.

1 There is no observation of this star; but in Miss HERSCHEL'S manuscript catalogue, No. 92, is a star 2° more south, which has probably been calculated wrong, and has given occasion for its insertion; correcting, therefore, the PD of 1 Navis $+ 2^\circ$, the expression of its brightness is as I have given it.

17 There is no observation of this star; but if we correct the PD $+ 3^\circ$, it will then agree with No. 238 in Miss HERSCHEL'S manuscript catalogue.

21 By FLAMSTEED'S observation page 431, the PD of the British catalogue requires $+ 18'$.

Notes to Orion.

12 FLAMSTEED never observed this star. It does not appear how it came to be inserted in the British catalogue.

26 FLAMSTEED never observed this star. An error of $20'$ in PD in the calculation of one of the four observations of $\alpha 5$ Orionis, may have occasioned the insertion of it.

35 Is marked :: in the British catalogue; but FLAMSTEED

has seven complete observations of this star; therefore the marks :: should be out.

63 There is no observation of this star; but supposing an error of $+ 2' 14''$ of time in RA, and of $+ 0' 22''$ in PD, it will then agree with No. 33 of Miss HERSCHEL'S manuscript catalogue. I have taken the comparative brightness of that star, supposing it to be 63.

64 and 65 Have no observation by FLAMSTEED; but their insertion has been accounted for by Mr. BODE in his *Jahrbuch* for 1793, page 195. He mentions FLAMSTEED'S two observations on page 17 and 94. There is a third on page 292, which confirms what Mr. BODE says. The 64 of which I give the brightness, is not far from the place assigned to it in the British catalogue. It is No. 1 in Miss HERSCHEL'S manuscript catalogue.

76 There is no observation of this star. A mistake of $41'$ in PD in calculating one of the four observations of 8 Monocerotis, might occasion its insertion.

WM. HERSCHEL.

Slough, near Windsor,

April 12, 1797.

XIV. *An Account of the Means employed to obtain an overflowing Well. In a Letter to the Right Honourable Sir Joseph Banks, Bart. K. B. P. R. S. from Mr. Benjamin Vulliamy.*

Read May 25, 1797.

SIR,

PERMIT me, in compliance with your request, to give you a short account of the well at Norland House, belonging to Mr. L. VULLIAMY; a work of great labour and expence, executed entirely under my direction, and finished in November, 1794.

Before I began the work, I considered that it would be of infinite advantage, should a spring be found strong enough to rise over the surface of the well; and though I thought it very improbable, yet I resolved to take from the beginning the same precautions in doing the work, as if I had been assured that such a spring would be found. But although this very laborious undertaking has succeeded beyond my expectation, yet from the knowledge I have acquired in the progress of the work, I am of opinion that it will very seldom happen that the water will rise so high; nor will people, I believe, in general, be so indefatigable as I have been in overcoming the various difficulties that did and ever will occur, in bringing such a work to perfection.

In beginning to sink this well, which has a diameter of four feet, the land springs were stopped out in the usual manner, and the well was sunk and steined to the bottom. When the

workmen had got to the depth of 236 feet, the water was judged not to be very far off, and it was not thought safe to sink any deeper. A double thickness of steining was made about 6 feet from the bottom upwards, and a borer of $5\frac{1}{4}$ inches diameter was made use of. A copper pipe of the same diameter with the borer was driven down the bore-hole to the depth of 24 feet, at which depth the borer pierced through the rock into the water; and by the manner of its going through, it must probably have broken into a stratum containing water and sand. At the time the borer burst through, the top of the copper pipe was about 3 feet above the bottom of the well: a mixture of sand and water instantly rushed in through the aperture of the pipe. This happened about two o'clock in the afternoon, and by twenty minutes past three o'clock the water of the well stood within 17 feet of the surface. The water rose the first 124 feet in eleven minutes, and the remaining 119 feet in one hour and nine minutes. The next day several buckets of water were drawn out, so as to lower the water 4 or 5 feet; and in a short time the water again rose within 17 feet of the surface. A sound-line was then let down into the well in order to try its depth. To our great surprise the well was not found by 96 feet so deep as it had been measured before the water was in it; and the lead brought up a sufficient quantity of sand to explain the reason of this difference, by shewing that the water had brought along with it 96 feet of sand into the well. Whether the copper pipe remained full of sand or not, is not easy to be determined; but I should rather be inclined to think it did not.

After the well had continued in the same state several days, the water was drawn out so as to lower it 8 or 10 feet; and

it did not rise again by about a foot so high as it had risen before. At some days interval water was again drawn out, so as to lower the water as before; which at each time of drawing rose less and less, until after some considerable time it would rise no more; and the water being then all drawn out, the sand remained perfectly dry and hard. I now began to think the water lost; and, consequently, that all the labour and expence of sinking this well, which by this time were pretty considerable, had been in vain. There remained no alternative but to endeavour to recover it by getting out the sand, or all that had been done would be useless; and although it became a more difficult task than sinking a new well might have been, yet I determined to undertake it, because I knew another well might also be liable to be filled with sand in the same manner that this was. The operation of digging was again necessarily resorted to, and the sand was drawn up in buckets until about 60 feet of it were drawn out, and, consequently, there remained only 36 feet of sand in the well: that being too light to keep the water down, in an instant it forced again into the well with the same violence it had done before; and the man who was at the bottom getting out the sand, was drawn up almost suffocated, having been covered all over by a mixture of sand and water. In a short time the water rose again within 17 feet of the surface, and then ceased to rise, as before. When the water had ceased rising, the sounding-line was again let down, and the well was found to contain full as much sand as it did the first time of the water's coming into it.

Any further attempt towards recovering the water appeared now in vain; and most people would, I believe, have abandoned the undertaking. I again considered that the labour and the

expençe would be all lost by so doing; and I determin'd without delay to set about drawing the sand out through the water, by means of an iron box made for that purpose, without giving it time to harden as before. The labour attending on this operation was very great, as it was necessary continually to draw out the water, for the purpose of keeping it constantly rising through the sand, and thereby to prevent the sand from hardening. What rendered this operation the more discouraging was, that frequently after having drawn out 6 or 7 feet of sand in the course of the day, upon sounding the next morning the sand was found lowered only 1 foot in the well, so that more sand must have come in again. This, however, did not prevent me from proceeding in the same manner during several days, though with little or no appearance of any advantage arising from the great exertions we were making. After persevering, however, for some considerable time, we perceived that the water rose a little nearer to the surface, and I began to entertain some hopes that it might perhaps rise high enough to come above the level of the ground; but when the water had risen a few feet higher in the well, some difficulties occurred, occasioned by accidental circumstances, which very much delayed the progress of the work; and it remained for a considerable time very uncertain whether the water would run over the top of the well or not.

These difficulties being at length surmounted, we continued during several days the process before mentioned, of drawing out the sand and water alternately; and I had the satisfaction of seeing the water rise higher and higher, until at last it ran over the top of the well, into a temporary channel that conveyed it into the road. I then flattered myself that every difficulty

was overcome; but a few days afterwards I discovered that the upper part of the well had not been properly constructed, and it became necessary to take down about 10 feet of brick-work. The water, which was now a continued stream, rendered this extremely difficult to execute. I began by constructing a wooden cylinder 12 feet long, which was let down into the well, and suspended to a strong wooden stage above, upon which I had fixed two very large pumps, of sufficient power to take off all the water that the spring could furnish, at 11 feet below the surface. The stage and cylinder were so contrived as to prevent the possibility of any thing falling into the well; and I contrived a gage, by which the men upon the stage could always ascertain to the greatest exactness the height of the water within the cylinder. This precaution was essentially necessary, in order to keep the water a foot below the work which was doing on the outside of the cylinder, to prevent the new work from being wetted too soon. After every thing was prepared, we were employed eight days in taking down 10 feet of the wall of the well, remedying the defects, and building it up again; during which time ten men were employed, five relieving the other five, and the two pumps were kept constantly at work during one hundred and ninety-two hours. By the assistance of the gage, the water was never suffered to rise upon the new work until it was made fit to receive it. When the cylinder was taken out, the water again ran over into the temporary channel that conveyed it into the road.

The top of the well was afterwards raised 18 inches, and constructed in such a manner as to be able to convey the water

five different ways at pleasure, with the power of being able to set any of these pipes dry at will, in order to repair them whenever occasion should require. The water being now entirely at command, I again resolved upon taking out more sand, in order to try what additional quantity of water could be obtained thereby. I cannot exactly ascertain the quantity of sand taken out, but the increase of water obtained was very great; as instead of the well discharging thirty gallons in a minute, the water was now increased to forty-six gallons in the same time.

If you think, Sir, that the above account of an overflowing well, the joint production of nature and art, is deserving your attention, I feel myself much gratified in the pleasure I have in giving you this description of it; and have the honour of being, with the greatest regard, SIR, &c.

B. VULLIAMY.

EXPLANATION OF THE PLATE. (Tab. VII.)

Fig. 1.

- a* Top of the well, with the water running over.
- b b* Ground line.
- c* Sand lying in the well.
- d* Copper pipe.
- ffffff* Steining of the well.
- g g* Double steining six feet from the bottom upwards.
- b* Stratum which the end of the copper pipe was driven into.

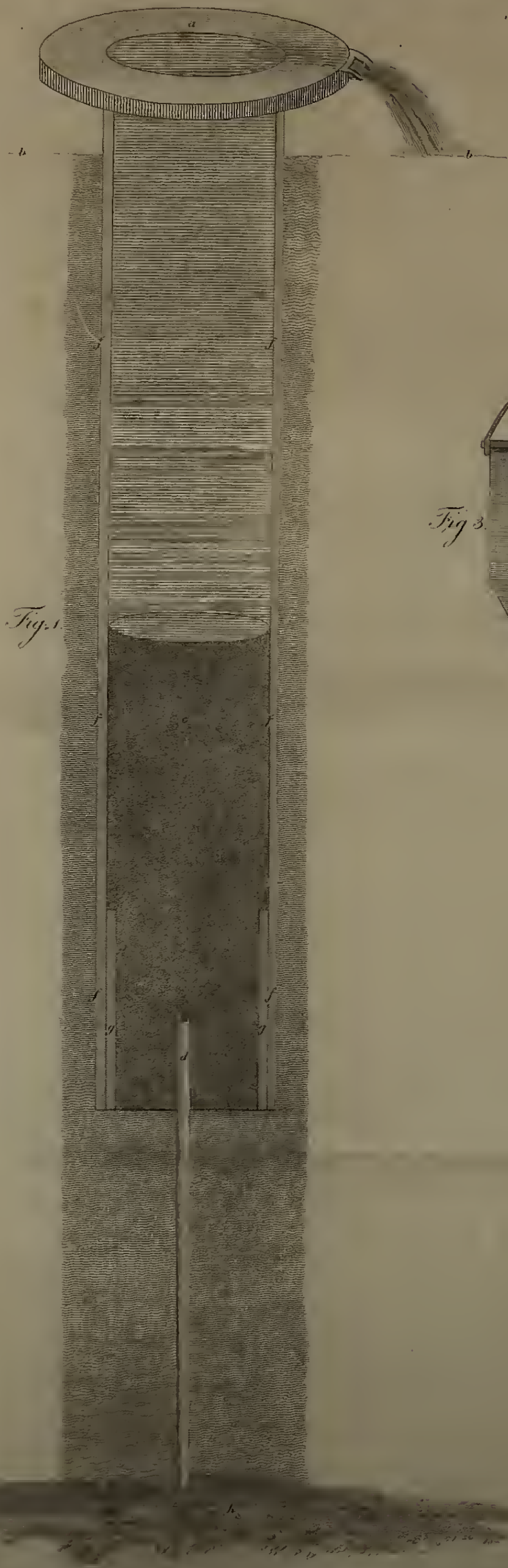


Fig. 1.

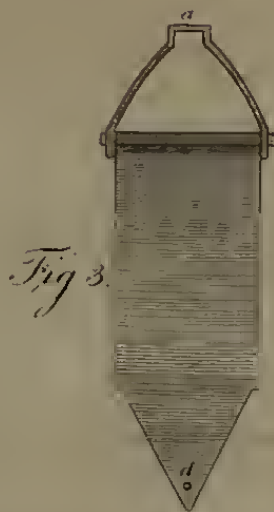


Fig. 3.



Fig. 2.



Fig. 2. and 3.

Iron box for drawing sand out of the well, weighing about 60lbs. one foot square, and two feet nine inches long.

a Handle of the box.

b A flap or door, which opens inwards by a joint at *c*. There is another door like this on the other side.

c The joint.

d The centre or pin of the joint,

XV. *Observations of the changeable Brightness of the Satellites of Jupiter, and of the Variation in their apparent Magnitudes; with a Determination of the Time of their rotatory Motions on their Axes. To which is added, a Measure of the Diameter of the Second Satellite, and an Estimate of the comparative Size of all the Four.* By William Herschel, LL.D. F.R.S.

Read June 1, 1797.

IT may be easily supposed when I made observations on the brightness of the 5th satellite of Saturn, by way of determining its rotation upon its axis, and found that these observations proved successful, that I should also turn my thoughts to the rest of the satellites, not only of Saturn, but likewise of Jupiter, and of the Georgian planet. Accordingly I have from time to time, when other pursuits would permit, attended to every circumstance that could forward the discovery of the rotation of the secondary planets; especially as there did not seem to lie much difficulty in the way. For since I have determined, by observation, that the 5th satellite of Saturn is in its rotation subject to the same law that our moon obeys, it seems to be natural to conclude that all the secondary planets, or satellites, may probably stand in the same predicament with the two I have mentioned; consequently a few observations that coincide with this proposed theory, will go a good way towards a confirmation of it.

I had another point in view when I made the observations

which are contained in this paper. It was an attempt to avail myself of the abundant light and high powers of my various telescopes, to examine the nature and construction of the bodies of the satellites themselves, and of their real magnitudes. Here phænomena occurred that will perhaps be thought to be remarkable, and even inconsistent or contradictory. So far from attempting to lessen the force of such animadversions, I shall be the first to point out difficulties, in order that future observations may be made to resolve them.

Perhaps it would have been better to delay the communication of these observations, till I had continued them long enough to be able to account for things which at present must be left doubtful. But as in final conclusions to be drawn from astronomical observations, we ought to take care not to be precipitate; so on the other hand I am perhaps too scrupulous in satisfying myself, and should probably require the observations of several years before I could venture to be decisive. It will also be seen by the dates of the first observations, that a further delay in the communication cannot be adviseable; since much information may possibly be gained by throwing open, to other observers, the road it will be eligible to take for a satisfactory investigation of the subject; especially as we have reason to congratulate ourselves on the spirit of observation, and increase of large instruments, that seem to have taken place in various parts of Europe.

I shall now transcribe the observations from my journals. They are as follows.

OBSERVATIONS.

A remarkable Conjunction of two Satellites of Jupiter.

May 14, 1790. $11^{\text{h}} 30' 10''$; correct sidereal time. The 2d and 3d satellites of Jupiter are so closely in conjunction, that with a 7-foot reflector, charged with a magnifying power of 350, I cannot see a division between them.

$11^{\text{h}} 34' 10''$. The shadow of the 1st satellite is still upon the disc of the planet.

Intenseness of Light and Colour of the Satellites.

July 19, 1794. $17^{\text{h}} 12' 47''$. 7-foot reflector. The 1st satellite of Jupiter is of a very intense bright, white, shining light. It is brighter than the 2d or 4th. I speak only of the light, and not of the size.

The colour of the 4th satellite is inclining to red. In brightness it is very nearly, but not quite equal to the 2d. I make no allowance for its being farther from the bright disc of Jupiter than the 2d.

10-foot reflector, power 170. The 3d satellite is just gone upon the body; before it went on, it appeared to me to be smaller than usual.

The 2d satellite is of a dull, ash-colour; not in the extreme, but rather inclining to that tint.

July 21, 1794. $16^{\text{h}} 56' 45''$. 10-foot reflector; power 170. The 3d satellite of Jupiter is round, large, and well defined. It is very bright, and its light is very white.

The 4th satellite is also round, large, and well defined. I estimate its magnitude in proportion to that of the 3d satellite to be as 4 to 5. Its light is not white, but inclined to orange.

Brightness and Diameter distinguished.

July 26, 1794. $17^{\text{h}} 14' 41''$. 10-foot reflector; power 170.
The 4th satellite is very dim. It is of a pale, dusky, reddish colour.

The 2d satellite is of a bright, white colour.

The 3d satellite is very bright, and white.

The 1st satellite is very brilliant, and white.

$17^{\text{h}} 22' 41''$. *The Magnitudes with 240.*

The 3d satellite is the largest.

The 2d satellite is the smallest.

With 300.

The 4th satellite is a very little larger than the 2d, though less bright.

The 1st satellite is larger than either the 4th or 2d.

With 400, the order of the magnitudes is 3 1 4 2.

With the same power, the order of the light is 3 1 2 4.

Now and then it appeared to me doubtful whether the 4th satellite was larger than the 2d; and as their light is of an unequal intensity, it is difficult without much attention, to be decisive about the magnitudes.

Diameter of the second Satellite by entering on the Disc of the Planet.

July 28, 1794. $17^{\text{h}} 25' 40''$. 10-foot reflector; power 170.
The 2d satellite is nearly in contact with the following limb of Jupiter.

17^h 29' 40". It seems to be very near the contact. With 300, very near the contact.

17^h 30' 40". It seems to be in contact. It is brighter than that part of Jupiter where it enters.

17^h 31' 40". It is more than half entered.

17^h 33' 40". It seems to be nearly quite entered. Its superior brightness makes it seem protuberant.

17^h 34' 40". It is certainly quite entered.

17^h 35' 25". I see a little of the disc of Jupiter on the outside of the satellite, equal to about $\frac{1}{4}$ of its diameter.

17^h 39' 40". The 3d satellite is very bright, and of its usual colour.

The 4th satellite is faint, and also of its usual colour.

The 1st satellite is very bright, and the light of it is of its usual intenseness.

The Magnitudes with 300.

The diameter of the 4th seems to be to that of the 3d, as 2 to 3; or perhaps more exactly, as 3 to 5.

The diameter of the 4th satellite exceeds that of the 1st a very little.

With 400.

With this power the diameter of the 4th satellite certainly exceeds that of the 1st.

The diameter of the 4th, is to that of the 3d, as 3 to 5.

July 30, 1794. 19^h 1' 37". 10-foot reflector; power 300. The 4th satellite of Jupiter is a little larger than the 1st. It is of its usual colour.

The 2d is less than the 1st.

The 3d is larger than the 4th.

July 31, 1794. $17^h 18' 38''$. 10-foot reflector; power 170.

The four satellites of Jupiter are very favourably placed for my purpose.

The 1st is less bright than the 2d; it is a very little larger than the 2d: the difference in the size is but barely visible.

The light of the 2d is very intense and white.

The light of the 3d is very intense and bright.

The light of the 4th is dull; and seems to be inferior to the usual proportion it bears to the other satellites.

$18^h 38' 38''$. *With 300.*

The 4th satellite is larger than the 1st.

The 2d satellite is a little larger than the 1st, or at least equal to it.

The 3d is undoubtedly the largest. The order of the magnitudes therefore is, 3 4 2 1.

My Brother, ALEXANDER HERSCHEL, looked at the satellites, and estimated the order of their magnitudes exactly the same; though he was not present when I made the foregoing estimations.

August 1, 1794. $17^h 38' 37''$. 10-foot reflector; power 170. The light of all the four satellites is very brilliant, the evening being very fine.

With 300.

The northmost and farthest of the two satellites which are in conjunction, is the smallest: I suppose it to be the 2d.

The southmost and nearest of the two satellites in conjunction, is the next in size: I suppose it to be the 1st.

The 4th satellite is a little larger than the largest of the two satellites which are in conjunction; but the difference is only visible with a great deal of attention.

The 3d satellite is much larger than the 4th.

August 9, 1794. $17^{\text{h}} 56' 32''$. 10-foot reflector; power 170. The light of the 1st satellite is very intense and white.

The light of the 2d satellite is also pretty intense and white.

The light of the 3d satellite is neither so intense nor so white as that of the 1st.

The light of the 4th is dull and of a ruddy tinge.

With 300, and 400, the second is the least, and the 3d is the largest. I am in doubt whether the 4th or the 1st is largest; with 600, I suppose the 1st to be larger than the 4th.

September 30, 1795. $20^{\text{h}} 15' 17''$. 7-foot reflector; power 210. Order of the magnitudes of the satellites of Jupiter 3 - 2 . 1 , 4. Power 110. 3 - 2 , 1 . 4. With 460, 3 - 2 , 1 , 4.*

October 2, 1795. $20^{\text{h}} 18' 22''$. 7-foot reflector; power 287. Jupiter's satellites 3 - - 2 - 1 , 4. The 2d and 3d satellites are not yet in conjunction.

$20^{\text{h}} 43' 22''$. The conjunction between the 3d and 2d satellites is past. The distance between them is now one diameter of the 3d.

August 18, 1796. $18^{\text{h}} 47' 21''$. 7-foot reflector; power 287. The 4th satellite is less bright than the 1st; notwithstanding

* Here, in order to denote the different magnitudes of the satellites, I used the notation which has been explained in my *First Catalogue of the comparative Brightness of the Stars*. See Phil. Trans. for the year 1796, Part I. page 189.

the latter is so near the planet as to have its light overpowered by Jupiter, while the 4th is at a great distance. I mean light or brightness, not magnitude.

The 1st is very bright.

September 15, 1796. $19^{\text{h}} 25' 25''$. 10-foot reflector; power 300. The 2d satellite of Jupiter is a little less than the 1st.

The 3d is much larger than any of the rest.

Power 600. The difference in the magnitude of the 1st and 2d satellites, with this power, is pretty considerable.

September 21, 1796. $19^{\text{h}} 24' 5''$. 10-foot reflector; power 600. The shadow of the 1st satellite is upon one of the dark belts of Jupiter.

In order to use very high powers with this telescope, I tried it upon the double star ζ Aquarii with 1200. The air is very tremulous, but I see now and then the two stars of this double star very well defined.

With the same power, the satellites of Jupiter are very large, but not so well defined as the above star.

The Brightness of the Satellites compared to the Belts and Disc of the Planet.

The 1st satellite, which is lately come off the southern belt, is nearly of the same brightness with that belt; power 600. With 400, it is nearly as bright as the brighter part of the planet, or rather a mean between the belt and the planet.

The 2d satellite is considerably bright; its colour is whiter than that of the 1st; it is however not so white as the colour of the bright part of Jupiter.

The colour of the 4th satellite is as dingy as that of the belt; very much less bright and less white than that of the 2d.

The brightness of the 3d satellite is not intense; its colour, however, is white, though not so white as the bright part of the planet.

September 24, 1796. $20^h 55' 24''$. 10-foot reflector; power 600. The 1st satellite of Jupiter is very bright, and of a white colour; it is also very large.

The 2d satellite is faint and bluish; its light is not much brighter than that of the belt.

The 3d satellite is pretty bright; its light is whitish. It seems to be comparatively less than it ought to be; or rather, its apparent smallness is owing to the uncommon largeness of the 1st.

The 1st satellite, with 200, compared to the 3d, is proportionally larger than I have seen it before.

September 30, 1796. $20^h 8' 4''$. 10-foot reflector; power 600. The satellites of Jupiter are well defined, and the night is beautiful.

The 3d satellite, in proportion to the 1st, is much larger than it was September 24. I ascribe the change to an apparent diminution of the 1st.

$20^h 30' 4''$. The 1st satellite is evidently less in proportion to the 3d, than it was September 24.

The 2d satellite is considerably bright; its light is whitish; much brighter than the belt, but not so bright as the bright part of the disc. Its magnitude is less than that of the 4th; but its light is considerably superior.

The 3d satellite is remarkably well defined. Its light is considerably brighter than that of the belts.

The magnitude of the 1st satellite exceeds that of the 2d. It is nearly equal to that of the 4th.

22^h 58' 4". Appearances as before.

October 15, 1796. 21^h 23' 42". 10-foot reflector; power 600. The 2d satellite is uncommonly bright; its apparent magnitude is also larger than usual.

The 4th satellite is very faint; it is not brighter than the belt, but is of a bluish, ruddy colour.

The apparent magnitude of the 2d satellite, after long looking, is very nearly equal to that of the 1st; but at first sight it seems to be larger, owing to its superior brightness.

The apparent diameter of the 2d satellite is certainly larger than that of the 4th.

23^h 55' 42". The light of the 1st satellite, compared to that of the 2d, is considerably increased since the last observation. It is now nearly as bright as the 2d.

October 16, 1796. 0^h 23' 49". 10-foot reflector; power 600. The 1st, 2d, and 3d satellites of Jupiter seem all considerably bright.

The 3d is much larger than the 1st, and the 1st a little larger than the 2d.

The intensity of the light seems to be pretty equal in all the three; that of the 2d, however, is perhaps a little stronger than that of the 1st; for, notwithstanding its apparent less diameter, it seems to make as strong an impression as the 1st.

October 25, 1796. 21^h 44' 48". 10-foot reflector; power 600. The 1st satellite of Jupiter, compared to the 3d, is small.

The 3d satellite is bright and large.

The 2d is brighter than the 1st. Compared to its usual brightness and magnitude, it is very bright and small.

The 1st satellite, compared to its usual brightness and magnitude, is faint and small.

The air is so tremulous that the power of 600 is too high, and the necessary uniformity required in these observations will not permit a lower to be used. Perhaps one of 400 might be more generally employed; and it may be proper to use it constantly.

November 3, 1796. $23^{\text{h}} 55' 47''$. 10-foot reflector; power 600. The 4th satellite of Jupiter is large and bright.

The 3d satellite is large and bright.

The 1st satellite is pretty small, and not very bright.

The 2d satellite is small, and considerably bright.

The brightness and magnitude of each satellite refer to its own usual brightness and magnitude.

Before we can proceed to draw any conclusions from these observations, we ought to take notice of many causes of deception, and of various difficulties that attend the investigation of the brightness of the satellites.

The difference in the state of the atmosphere between two nights of observation, cannot influence much our estimation of the brightness of a satellite, provided we adopt the method of comparative estimations. If we endeavour by much practice to fix in our mind a general ideal standard of the brightness of each satellite, we shall find the state of the atmosphere in different nights very much disposed to deceive us; but if we learn to acquire a readiness of judging of the comparative brightness of each satellite with respect to the other three, we may arrive at much more precision, since the different disposition of the air will nearly affect all the satellites alike. But here, as we get rid of one cause of deception, we fall under the penalty of another. The situation of those very satellites to which we are

to refer the light of the satellite under estimation, being changeable, permits us no longer to trust to their standard, without a full scrutiny of the causes that may have produced an alteration in them.

In the foregoing observations it will also be seen, that I attempted to compare the intenseness of the light of the satellites with the different brightness of the disc of Jupiter; but these endeavours will always fail, on account of the little assurance we can have that the parts of the disc, setting aside its quick rotation, will remain for any time of the same lustre.

A very material difficulty arises from the magnifying power we use in our estimations. If it be a low one, such as for instance 180 (for a lower should not even be attempted), then we run the risk of being disappointed in bright nights by the sparkling of the brilliant light of the satellites. Besides, we cannot then see the bodies of them, and judge of their comparative magnitude, with the same power that we view their light. If we choose a high magnifier, we shall be often disappointed in the state of the atmosphere, which will of course occasion an interruption in the series of our observation, of which the regular continuance is of the greatest consequence. If we change our power according to the state of the atmosphere, we introduce a far worse cause of confusion; for it will be next to impossible to acquire, for each magnifying power, an ideal standard of comparative brightness to which we can trust with confidence.

If the magnitudes are not attended to, and carefully distinguished from the intenseness of light, we shall run into considerable error, by saying that a satellite is large, when we mean to express that it is bright. It is so common to call stars that are less bright than others, small, that we must be careful

to avoid such ambiguities, when the condition of the satellites is under investigation. Nor is it possible to throw the size and light into one general idea, and take the first *coup d'oeil* in looking at them, to decide about the general impression this compound may make. When our attention is forcibly drawn by a considerable power to the apparent size of the satellite we are looking at, its brightness can no longer be taken in that general way, but must be abstracted from size.

Let us now see what use may be drawn from the observations I have given.

It appears in the first place very obviously, that considerable changes take place in the brightness of the satellites. This is no more than might be expected. A variegated globe, whether terraqueous like the earth, or containing regions of soil of an unequal tint, like that side of the moon which is under our inspection, cannot, in its rotation, present us with always the same quantity of light reflected from its surface.

In the next place, the same observations point out what we could hardly expect to have met with; namely, a considerable change in the apparent magnitude of the satellites. Each of them having been at different times the standard to which another was referred, we cannot refuse to admit a change so well established, singular as it may appear.

The first of these inferences proves that the satellites have a rotatory motion upon their axes, of the same duration with their periodical revolutions about the primary planet.

The second either shews that the bodies of the satellites are not spherical, but of such forms as they have assumed by their quick periodical and slow contemporary, rotatory motions, and which forms in future may become a subject for mathematical

investigation; or it may denote, in case geometrical researches should not countenance a sufficient deviation from the spherical form, that some part of the discs of these satellites reflects hardly any light, and therefore in certain situations of the satellite makes it appear of a smaller magnitude than in others.

Here then we see evidently that a considerable field for speculation, as well as observation, is opened to our view; and almost every attempt to enter upon the work must seem premature, for want of more extended observations. However, from those that have been given, such as they are, I will shew how far we may be authorized to say, that the satellites revolve on their axes in the same time that they perform a periodical revolution about the planet.

I shall take the usual method of throwing the observations of each satellite into a graduated circle. The zero of the degrees into which I suppose it divided, is in all observations assumed to be in the place of the geocentric opposition.

In order to bring these observations to the circle, the places of the satellites have been calculated from my own tables of the mean motion in degrees, and according to epochs continually assumed from the geocentric conjunctions pointed out in the configurations of the Nautical Almanac; and the nearest of these conjunctions have been always used. This method is fully sufficient for the purpose, as greater precision in the calculation is not required.

The observations extend from July 19, 1794, to November 3, 1796; and therefore include a period which takes in 470 rotations of the 1st satellite; 234 of the 2d; 116 of the 3d; and 50 of the 4th: that is, provided we admit that these rotations

are performed in the same time the satellites revolve in their orbits.

In the following table are the calculated places of the satellites; the correct sidereal times, given with the observations, having been turned into mean time.

Table of the Positions of the four Satellites of Jupiter at the Time of the Observations.

Time or Observ.	I	II	III	IV	Time of Observ.	I	II	III	IV
July 19 ^d 9 ^h 21'	127 ^o	346 ^o	179 ^o	46 ^o	Aug. 18 ^d 8 ^h 21'	115 ^o	o	o	191 ^o
July 21. 8. 57			278	89	Sept. 15. 7. 44	36	328	198	
July 26. 8. 56	124	333	169	205	Sept. 21. 7. 19	172	214	138	210
July 28. 8. 59	171	176	270	248	Sept. 24. 8. 38	74	163	305	275
July 30. 10. 27	231	25	13	292	Sept. 30. 7. 27	206	46	244	36
July 31. 8. 40	59	118	60	312	Oct. 15. 7. 44	28	130		5
Aug. 1. 8. 56	265	221	111	334	Oct. 15. 10. 15	49			
Aug. 9. 8 42	83	310	152	138	Oct. 16. 10. 39	256	243	334	
					Oct. 25. 7. 25	261	72	59	
Sept. 30. 7. 37	294	62	219	100	Nov. 3. 9. 0	306	270	151	58
Oct. 2. 7. 32	341	264	319	143					

It will be necessary now to explain in what manner, with the assistance of this table, the observations of the brightness and magnitudes of the satellites have been reduced to the expressions they bear in the four circles of the figures contained in Tab. VIII. and IX. By way of uniformity I judged it would be best to reduce the estimations of magnitude to those of brightness; as it may be justly supposed that when a satellite is at any given time larger in proportion to another than it was at another time, it will also be brighter than it was at that other time, due regard being had to the light of the satellite to which its magni-

tude has been compared. To manage the space allotted to the figure advantageously, I have used the abbreviations formerly employed in my catalogue of Nebulæ, *v* B, *c* B, B, *p* B, *p* F, F, *c* F, *v* F, for all the gradations of light that are necessary to express the brightness of the satellites at the time of observation. It will be easily remembered that B and F mean bright and faint; and *p*, *c*, *v*, stand for pretty, considerably, and very.

Now, when the observation mentions the brightness of the satellite, I place it in the figure as it is given. In that of the first, for instance, July 19, 1794, we find the satellite called very bright; I therefore put down in fig. 1. (Tab. VIII.) at 127 degrees, *v* B. But where the brightness is not expressed, I have recourse to the comparative magnitude, if that can be had. By fig. 3. (Tab. IX.) it appears that the 2d satellite is less subject to a change of brightness than either the 1st or 4th: it becomes, for that reason, a pretty good standard for the light of these other satellites. Therefore, in the observation of October 2, 1795, for instance, where the 1st satellite is described as undoubtedly less than the 2d, I put down very faint, or *v* F, at 341 degrees of the circle in fig. 1.; for in the observation of July 19, before mentioned, when the satellite was called very bright, it was at the same time described as undoubtedly larger than the 2d. In this case, as regard must be had to the relative state of the satellite we refer to, the four figures I have given will assist us in determining the condition of the light of the satellite we wish to admit as a standard.

In reducing the 2d satellite to the circle, I have generally used a reference to the magnitude of the 1st, where marks of

brightness were wanting; and sometimes also to the magnitude of the 4th, and even of the 3d.

The 3d satellite can hardly be ever compared to any but the 2d in magnitude; and this only in its degree of excess.

The magnitude of the 4th satellite has been generally compared with that of the 1st; and also sometimes with that of the 2d.

To make an application of the contents of the figures, will now require little more than a bare inspection of them.

The 1st satellite appears evidently to have a rotation upon its axis that agrees with its revolution in its orbit. It cannot be supposed that, in the course of 470 revolutions, all the bright observations could have ranged themselves in one half of the orbit, while the faint ones were withdrawn to the other. The satellite appears in the middle of the duration of its brightness, when it is nearly half way between its greatest eastern elongation, in the nearest part of its orbit; or when advancing towards its conjunction. I have pointed out this circumstance by a division with dotted lines, and the words bright and faint, inserted within the circle, fig. 1. This satellite, therefore, revolves on its axis in $1^d 18^h 26',6$.

The 2d satellite, though much less subject to change, on account, as we may suppose, of having only a small region on its body which reflects less light than the rest; has, nevertheless, its rotation directed by the same law with the 1st. It will hardly be necessary to take notice of a single deviation which occurs at 163 degrees, fig. 2.; as from the proximity of the satellite to the conjunction, a mistake in the estimation may easily take place. I generally made it a rule not to make

allowance for the influence of the superior light of the planet ; but it seems that we can hardly abstract sufficiently on such occasions. Two similar cases occur, in fig. 3. at 179 ; and fig. 4. at 5 degrees.

It is indeed not impossible but that occasional changes, on the bodies of the satellites themselves, may occasion some temporary irregularity of their apparent brightness : it will, however, not be necessary to make such an hypothesis, till we have better authority for it. The brightest side of this satellite is turned towards us when it is between the greatest eastern elongation and the conjunction. It revolves, consequently, on its axis in $3^d 18^h 17',9$.

The 3d satellite suffers but little diminution of its brightness, and is in full lustre at the time of both its elongations. It is however not impossible but that, after having recovered its light, on the return from the opposition, it may suffer a second defalcation of it in the nearest quadrant about half way towards the conjunction. The two independent observations at 151 and 152 degrees, fig. 3. seem to give some support to this surmise. It revolves on its axis in $7^d 3^h 59',6$.

The 4th satellite presents us with a few bright views when it is going to its opposition, and on its return towards the greatest eastern elongation ; but otherwise it is generally overcast. Its colour also is considerably different from that of the other three ; and it revolves on its axis in $16^d 18^h 5',1$.

It will not be amiss to gather into one view, all the observations that relate to the colour of the satellites.

The 1st is white ; but sometimes more intensely so than at others.

The 2d is white, bluish, and ash-coloured.

The 3d is always white; but the colour is of different intensity, in different situations.

The 4th is dusky, dingy, inclining to orange, reddish and ruddy at different times; and these tints may induce us to surmise that this satellite has a considerable atmosphere.

I shall conclude this paper with a result of the observation of the diameter of the second satellite, taken by its entrance upon the disc of the planet, July 28, 1794, and marked in fig. 2. at 176 degrees.

The duration by the observation is fixed at 4 minutes; in which time it passes over an arch in its orbit of $16' 52'',9$. Now as its distance from the planet is to its distance from the earth, so is $16' 52'',9$ to the diameter of the satellite; or the mean distance of the 2d satellite may be rated, with M. DE LA LANDE, at $2' 57''$, or $177''$. Then putting this equal to radius, we shall have the following analogy. Radius is to $177''$, as the tangent of $16' 52'',9$ is to the angle, in seconds, which the diameter of the second satellite subtends when seen from the earth. And by calculation, this comes out $0'',87$; that is less than nine-tenths of a second.

I have not been scrupulously accurate in this calculation, as the real distance of Jupiter at the time of observation should have been computed, whereas I have contented myself with the mean distance. Nor am I very confident that the angle of the greatest elongation, admitted to be $2' 57''$, is quite accurate; but I judged it unnecessary to be more particular, because the time of my observation in the beginning of the transit upon the disc, I find was only taken down in whole minutes of the clock. The end, however, is more accurately determined, by the observation which was made $45''$ after the immersion; when a



Fig. 3
Opposition

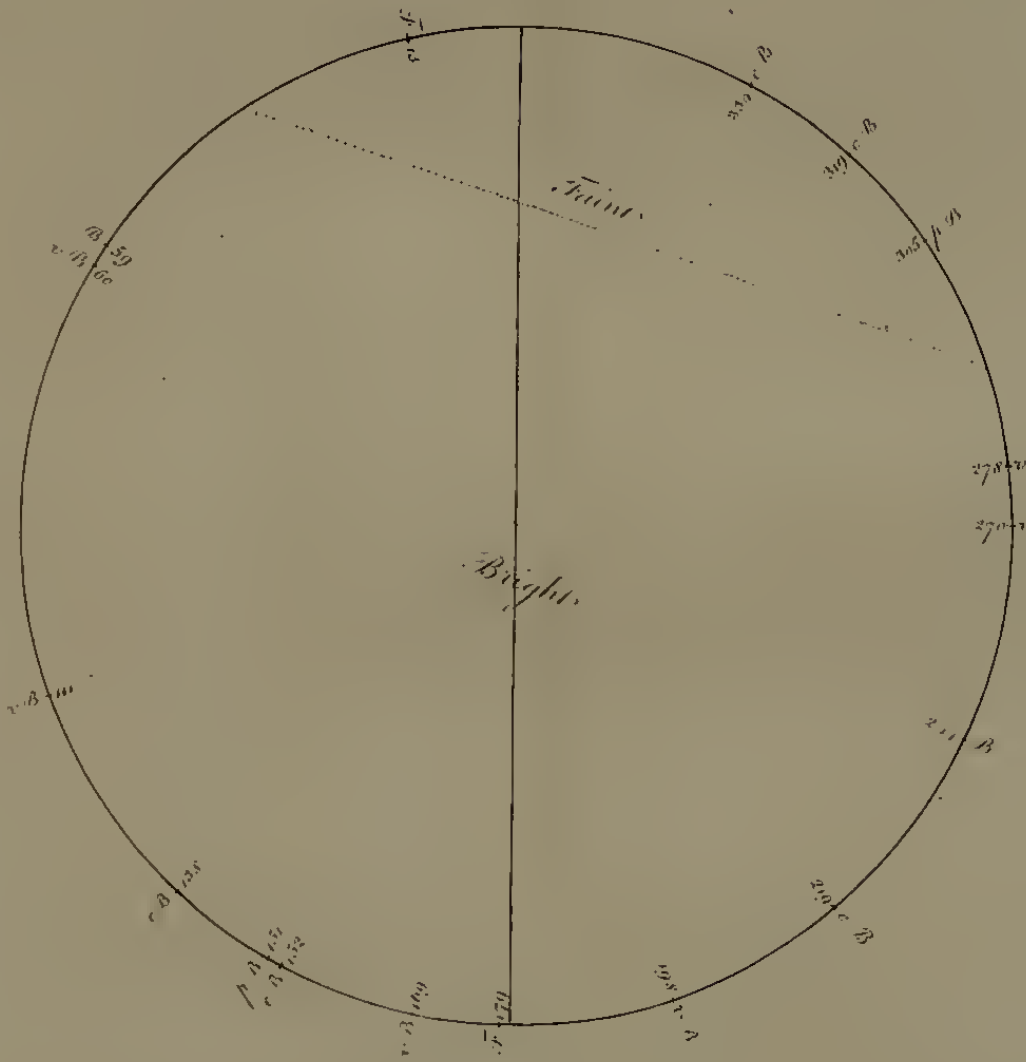
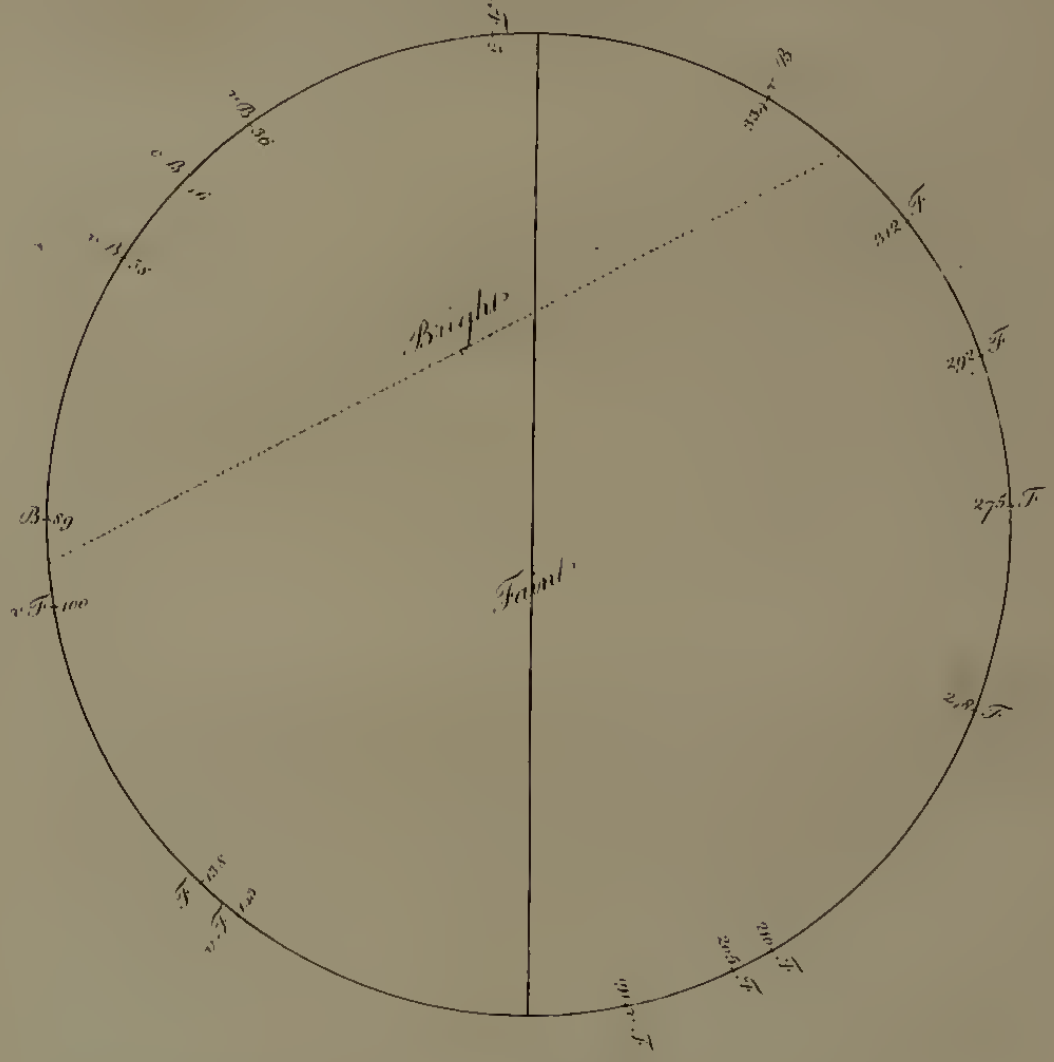


Fig. 4
Opposition





part of the disc, equal to about $\frac{1}{4}$ of the diameter of the satellite, is said to be visible. It seems that observations of this kind, made with very good telescopes, charged with high powers, are capable of great precision. For the remark that a margin of Jupiter, equal to about $\frac{1}{4}$ of the diameter of the satellite, became visible in $4.5''$ of time, adds great support to the accuracy of the observation of the foregoing 4 minutes: and, at all events, it is evidently proved, from the whole of the entrance upon the disc, that the diameter of this satellite is less, by one half at least, than what from the result of the measures of former observers it has been supposed to be.

A method has also been used, of deducing the diameter of the satellites from the time they employ to immerge into the shadow of the planet; but this must be very fallacious, and ought not to be used:

I should not pass unnoticed the apparent magnitude of the satellites. The expressions that have been given of them may be collected into the following narrow compass.

1, 4, 2 4; 1 3 -, 4; 1 - 2 4, 2, 1 3 -- 4; 1; 2
 1, 4, 2 3 - 2, 1, 4 3 -- 2 - 1, 4 1 $\bar{2}$ 2' 4. 1 - 2
 1 $\frac{1}{2}$ 2 - 4 3 -- 1, 2 2 - 1

From which we may conclude, that the 3d satellite is considerably larger than any of the rest; that the 1st is a little larger than the 2d, and nearly of the size of the 4th; and that the 2d is a little smaller than the 1st and 4th, or the smallest of them all.

WM. HERSCHEL.

Slough, near Windsor,

April 30, 1797.

XVI. *Farther Experiments and Observations on the Affections and Properties of Light.* By Henry Brougham, Jun. Esq. Communicated by Sir Charles Blagden, Knt. Sec. R. S.

Read June 15, 1797.

HAVING laid before the Royal Society an account of a course of experiments on light, in which I had been engaged, and also of the conclusions which these experiments had taught me to draw, I proceed in the following paper to relate the continuation of my observations; which I hope may not prove wholly uninteresting to such as honoured the former part with their attention. I am first to unfold a new and, I think, curious property of light, that may be indeed reckoned fourfold, as it holds, like the rest, equally with respect to refraction, reflexion, inflexion, and deflexion; thus preserving entire the same beautiful analogy in these four operations, which we have hitherto remarked. I shall then consider several phænomena connected either with this, or with the properties before described, and of which they afford some striking confirmations.

I.

Observation 1. The sun shining strongly into my darkened chamber, I placed, at a small hole in the window-shut, a prism with its refracting angle (of 65°) upwards, so that the spec-

trum was cast on a chart placed at right angles to the incident rays, and four feet from the prism.

In the rays, parallel to the chart, and two feet from it, I placed a pin, whose diameter was $\frac{1}{30}$ of an inch, and fixed it so, that the axis of its shadow on the spectrum might be parallel to the sides of the spectrum. A set of images by reflexion was formed (similar to those described above*), all inclining to the violet; but what I chiefly attended to at present was their shape. I had always observed that the part formed out of the red-making rays was broadest, and that the other parts diminished in breadth regularly towards the violet. I now delineated one or two, at about three inches from the shadow; and though (from the pin's irregularities) the sides were by no means smooth, yet the general shape was in every pin, and with every prism used, nearly as represented in fig. 1. (Tab. X.) divided in the direction R A, according to the colours of the spectrum in which they were formed; R O B A was red, and the broadest; that is, R A was broader than O B, the confines of the red and orange; and G D E V was the violet, narrowest of all.

Observation 2. Between the pin and the prism, $\frac{1}{10}$ of an inch from the pin, was placed a screen, through a small hole in which, of twice the pin's diameter, the rays of the spectrum passed, and were reflected into images by the pin; these were pretty distinct and well defined, when received on a chart $\frac{1}{2}$ a foot from the pin. They were oblong, having parallel sides and confused ends; they were wholly of the colour whose rays fell on the pin, unless when the white, mixed with those at the confines of the yellow and green, caused the images to be of all the

* Phil. Trans. for 1796, page 240.

colours. When the prism was turned round on its axis, so that different rays fell on the pin, the images changed their sizes as well as their positions; they were largest when red, and least when violet.

Observation 3. In case it may be thought that the sides of the hole, through which the rays passed in observation 2, by inflecting, might dispose them, before incidence, into beams of different sizes, I removed the screen, and placed the pin horizontally, the axis of the shadow being now at right angles to that of the prismatic spectrum; and moving the prism on its axis, again I observed the contraction and dilatation of the images by reflexion, though now they were rather less distinct, from the greater size of the incident beam; and to shew that there was both a change of size and of place, without any manner of deception, I placed one leg of a pair of compasses in a fixed point of the spectrum, and the other in the middle point of an image formed by the violet-making rays. The prism being then moved till the image became red, I again bisected it, and found its centre considerably beyond the point of the compasses, which was indeed evidently much nearer one end of the image than the other; besides, that the red image, when measured, was longer than the rest; and this satisfied me that there were two changes, one of place, with respect to the fixed point, the other of size, with respect to the centre of the image. Lastly, as far as I could judge, the dilatation and contraction appeared even and uniform.

Observation 4. I remarked that the fringes or images, by flexion, were always increased in size when formed out of red-making rays, and were less in every other colour, and least in violet (besides being moved farther from the edge of the shadow

in the former rays than in the latter); and this agrees with an observation of Sir ISAAC NEWTON, as far as he tried it, which was with respect to deflexion. In making several experiments with prisms, I hit on a very remarkable confirmation of this. I observed on each side of the spectrum four or five distinct fringes, like the images by reflexion, coloured in the order of the spectrum, but quite well defined at the edge, and even pretty distinct at the end; they were also much narrower than those images, but like them they inclined much to the violet, and were broadest in the red, growing narrower by degrees, and narrowest of all in the violet. I moved the prism and they disappeared, but when the prism was brought back to its former position, they also returned. I then observed the prism in open light, and saw that it had veins, chiefly opaque and white, running through it, and that there were several of these in the place where the light passed when the prism was held as before. But in case the inclination and shape of these images might be owing to the irregular order in which the veins were laid, I held another prism, which happened to have parallel veins; in many positions of this the fringes or images returned, not indeed always so regular nor always of the same kind; for some were confused and broader, formed (as I concluded from this and their position) by reflexion; others made by transparent veins and air-bubbles were also irregular, but inclined to the red, the violet being farthest from the perpendicular, and these were obviously caused by refraction; yet all agreed in this, that they were broadest in the red, and narrowest in the violet parts.

Observation 5. I held, in the direct rays of the sun at $\frac{1}{2}$ an inch from the small hole in the window-shut, a glass tube, free from

scratches and opaque veins, but like most glass that is not finely wrought, having its surface of a structure somewhat fibrous. When this tube was slowly introduced into the light, and so held that none of the rays might be refracted, a streak, chiefly white, was seen, similar in shape and position to those described before.* When narrowly inspected, it was found to contain many images by reflexion in it. But these were much diluted by the abundance of white light, reflected without decomposition in the manner above mentioned.† This streak lay wholly on one side of the tube; but I moved the tube onward a little, and another streak darted through the shadow, and extended all round on both sides: and now, when the tube was in the middle of the rays, there were two streaks on both sides, one a little separated from the other and continued through the shadow, the other on each side of the shadow; the former was evidently produced by refraction; it contained many images very like those by reflexion, only more vivid in the colours, which were all in the inverted order, the violet being outermost, and the rest nearest the point of incidence. Images similar to these are also producible on the retina, as mentioned before.‡

Observation 6. I now placed a prism at the hole, and made the same images by refraction, out of homogeneal light. These inclined to the red, not (like images by reflexion) to the violet; but they were broadest in the red, and grew narrower towards the violet parts. In short, when viewed beside the images by reflexion, except in point of brightness and inclination they differed from them in no respect.

The three first experiments shew, that when homogeneal

* Phil. Trans. 1796, page 236.

† Ibid. p. 237.

‡ Ibid. p. 243.

light is reflected, some rays are constantly disposed into larger images than others are, that is, into images more distended in length, though of the same breadth. The fourth experiment shews, that the same takes place when light is inflected and deflected; and the two last shew that the same happens when the rays are refracted in a way similar or analogous to that in which the other images were produced by reflexion and flexion.

We are now to shew, that this difference of size is not owing to the different reflexibilities and flexibilities of the rays. In order to this we shall both demonstrate, and then prove by experience, "that inflexion and deflexion do not decompose heterogeneous rays, whose direction is such, that they fall on the bending body." In fig. 2. let AB be the body; GH, EF, CD, the limits of its spheres of deflexion, inflexion, and reflexion, respectively; and let IP be a white ray of direct light entering at P the sphere of deflexion: through P draw LK at right angles to GH; IP will be separated into PR red, and PV violet, and the five other colorific rays according to their deflexibilities; at R and V draw the perpendiculars ST and QO; then the alternate angles PRT, RPL; and PVQ, VPL are equal each to each. But TRP and QVP are the angles of incidence, at which the red and violet enter the sphere of inflexion; and RPL, VPL are the angles of deflexion of the red and the violet; therefore the difference of the two latter, that is RPV, is likewise the difference of the two former. Suppose this difference equal to nothing; or that PV and PR are parallel; then rRS the angle of the red's inflexion will be less than vVO the angle of the violet's inflexion, by the angle RPV: (when not evanescent) add RPV to rRS ; then rRS will be equal to vVO : that is, the divergence will be destroyed,

and the rays enter the sphere of reflexion, parallel and undecomposed. It is evident, therefore, that the effect arising from the different deflexibilities of the rays is destroyed by the equal and opposite effect produced by their different inflexibilities; and the same thing may in like manner be shewn to happen in the return of the rays from the body after reflexion. But let the rays be so reflected that they shall pass by the body without entering any more than one sphere of flexion; then they will be separated by their flexibilities, as we before described. It appears, then, that if the rays of light were not differently reflexible, flexion could never produce the coloured images, by separating the compound light. And, indeed, this may be easily proved by fact. At 144 feet from the bending body, the greatest fringes by flexion are only half an inch in length, whereas the fourth or fifth images by reflexion are above half an inch at one foot from the reflecting surface: the one sort is therefore more than 144 times more distended than the other, whereas the flexion could, at the very farthest, only double them. Also the distinctness, and brightness, and regularity of the colouring, are quite different in the two cases; the supposed cause would neither account for the order of the colours, nor for their absence in common specular reflexion, and refraction through two prisms joined together with their angles the contrary ways. Lastly, if we suppose the images to be produced by flexion, and then reflected from the body, it would follow that light incident on a prism should be decomposed, formed into several coloured images, and then refracted, the violet being least and the red most bent; all which is perfectly the reverse of what actually happens. I have multiplied the proof of this proposition perhaps beyond what is

necessary; but its great importance to the whole theory will, I hope, plead my excuse.

Let us now suppose that a homogeneal beam passes through the spheres of flexion, it will follow that no divergence can take place from the bending power of the body; so that we have only to estimate the effect produced by the reflexion, and to inquire whether the different reflexibilities of the rays can cause the images to vary their sizes according as they are formed by different rays. In fig. 3. let AB be the body, CD the limit of its sphere of reflexion, and IP a beam of homogeneal rays, as red, incident at P and reflected to R , forming there the image Rr . It is evident that the greater reflexibility of the rays IP can only alter the position of the centre of Rr , making it nearer the perpendicular than the centre of an image formed by any other rays would be. But the greater length of Rr shews that a greater quantity of rays is reflected, or that the same quantity is spread over a greater space, and that in the following way. Let $IFfi$ be a beam of violet-making rays entering $ABCD$, and reflected so as to form the image Rv . The force exerted by AB decreasing according to some law (of which we are as yet ignorant) as the distance increases, is not sufficient to turn the rays back till they have come a certain length within $ABCD$. But for the same reason it turns back all that it does reflect before they come nearer than a certain distance; between these two limits, therefore, the rays are turned back. But the limits are not the same to all the rays; some begin to be turned at a greater distance from the body than others, and consequently are reflected to a greater distance from the middle ray of the incident beam. Thus if $IFfi$ be changed to a red-making beam, it begins to be turned back

at f , and the rays farthest from AB are reflected to r instead of to v , where they fell when I Ffi was violet-making; not but that the same quantity of rays is reflected, the only difference is, that the most reflexible are reflected farthest from the body by their greater reflexivity, and farthest from each other by this other property. Exactly the same happens in the case of refraction, *mutatis mutandis*; but there seems to be a slight variation in the *manner* in which the different rays are disposed into images of different sizes by flexion. In this case also the bending body's action reaches farther when exerted on some rays than when exerted on others: but then, the direction of the rays not passing through the body, those which are farthest off and at too great a distance to be bent, never coming nearer, are not bent at all; and consequently as the least flexible rays are in this predicament at the smallest distance, and the most flexible not till the distance is greater, the images formed out of the former must be less than those formed out of the latter. This difference in the way in which the phænomenon appears, does not argue the smallest difference in the cause: it only follows from the different position of the rays, with respect to the acting body, in the two cases. I infer then from the whole, that different sorts of rays come within the spheres of flexion, reflexion, and refraction, at different distances, and that the actions of bodies extend farthest when exerted on the most flexible. It may perhaps be consistent with accuracy and convenience to give a name to this property of light; we may, therefore, say that the rays of light differ in degree of refrangity, reflexivity, and flexity, comprehending inflexity and deflexity. From these terms (uncouth as, like all new words, they at first appear) no confusion can arise, if we always re-

member that they allude to the degree of distance to which the rays are subject to the action of bodies. I shall only add an illustration of this property, which may tend to convey a clearer idea of its nature. Suppose a magnet to be placed so that it may attract from their course a stream of iron particles, and let this stream pass at such a distance that part of it may not be affected at all; those particles which are attracted may be conceived to strike on a white body placed beyond the magnet, and to make a mark there of a size proportional to their number. Let now another equal stream considerably adulterated by carbonaceous matter, oxygene, &c. pass by at the same distance, and in the same direction. Part of this will also be attracted, but not so far from its course, nor will an equal number be affected at all; so that the mark made on the white body will be nearer the direction of the stream, and of less size than that made by the pure iron. It matters not whether all this would actually happen, even allowing we could place the subjects in the situation described; the thing may easily be conceived, and affords a good enough illustration of what happens in the case of light.

Pursuant to the plan I before followed, I now tried to measure the different degrees of reflexivity, &c. of the different rays; but though the measurements which I took agreed in this, that the red images were much larger than the rest, and the green appeared by them of a middle size, yet they did not agree well enough (from the roughness of the images, and several other causes of error), to authorize us to conclude with any certainty "that the action of bodies on the rays is in proportion to the "relative sizes of these rays." This, however, will most pro-

bably be afterwards found to be the case; in the mean time there is little doubt that the sizes are the cause of the fact.

II.

Several phænomena are easily explicable on the principles just now laid down.

1. If a pin, hair, thread, &c. be held in the rays of the sun refracted through a prism, extending through all the seven colours, a very singular deception takes place: the body appears of different sizes, being largest in the red and decreasing gradually towards the violet. This appearance seemed so extraordinary, that some friends who happened to see it as well as myself, suspected the body must be irregular in its shape. On inverting it, however, the same thing took place; and on turning the prism on its axis, so that the different rays successively fell on the same parts, the visible magnitude of the body varied with the rays that illuminated it. This appearance is readily accounted for by the different reflexivity of the rays, and follows immediately from Obs. 2. and 3.

2. Sir ISAAC NEWTON found that the rings of colours made by thin plates and by thick plates of glass (as he calls them), when formed of homogeneal light, varied in size with the rays that made them, being largest in the most flexible rays. I have had the pleasure of observing several other sorts of rings, so extremely similar, and formed by flexion, that I can no longer doubt of this being also the cause of the phænomena observed by NEWTON. I shall first describe a species, to prove " that the colours by thick and thin plates are one and the " same phænomenon, only differing in the thickness of the

“plates.” Happening to look by candle-light upon a round concave plate of brass, pretty well polished, so as to reflect light enough for shewing an image of the candle, I was surprised to see that image surrounded by several waves of colours, red, green, and blue, disposed in pretty regular order. This was so uncommon in a metallic speculum, that I examined the thing very minutely by a variety of experiments; these I shall not particularly now describe, but give a general idea of their results.

It must be observed, for the sake of clearness, that in the following inquiries concerning the formation of rings or fringes, the diameter of a ring or fringe means the line passing through the centre of that ring, and terminated at both ends by the circumference; whereas the breadth means that part of the diameter intercepted between the limits of the ring, or the distance between its extreme colours, red and violet.

In the first place, they were formed by the sun’s light in the figure of rings surrounding the centre of the sphere to which the plate was ground, at greater distances increasing their breadths, the colours pretty bright, though inferior in brilliancy to those of concave specula.

Secondly, the order of the colours was in all red outermost, and violet or blue innermost, with a greyish-blue spot in the common centre of the whole; and on moving the plate from the perpendicular position, the rings moved and broke exactly like those of specula.

In the third place, homogeneal light made them of simple colours; they were broadest when red, narrowest when blue and violet.

Fourthly, they decreased in breadth from the centre; and I

found, by a simple contrivance, that they were to one another in the very same ratio that the rays by specula follow.

In the fifth place, I compared the general appearance of the two sorts by viewing them at the same time, and was struck with their general appearance, unless that these of specula were most vivid and distinct.

These things made me suspect that they were actually caused by the thin coat of gums with which the surface of the plate was varnished, called lacker. Accordingly I took it off with spirit of wine, and found the rings disappear; on lackering it again they returned; and in like manner I caused a well finished concave metal speculum to form the rings of which we are speaking, by giving it a thin coat of lacker. This is a clear proof that these rings were exactly the same with those of thick plates (to use NEWTON'S expression), for the coat of gums is, when thin, pretty transparent, as may be seen by laying one on glass plates.

But this coat is extremely thin, and cannot exceed the 200th part of an inch; so that the colours of thick plates are in fact the very same with those of thin plates, except that the two kinds are made by different sized plates. We cannot, therefore, distinguish them, any more than we do the spectrum made by a prism whose angle is 90° from that made by one whose angle is 20° . This kind of colours is not the only one I have observed of nearly the same kind with those of plates; we shall presently see another much more curious and remarkable.

III.

In reflecting on the observations and conclusions contained in my former paper, several consequences seemed to follow,

which appeared so new and uncommon, that I began to doubt a little the truth of the premises; but at any rate was resolved to examine more minutely how far these inferences might be consistent with fact: and I am happy in being able to announce the completeness of that consistency, even beyond my expectations. The chief consequences were the following.

1. That a speculum should produce, by flexion and reflexion, colours in its reflected light wherever it has the least scratch or imperfection on its surface.

2. That on great inclinations to the incident rays all specula, however pure and highly polished, should produce colours by flexion.

3. That they should also in the same case produce colours by reflexion.

4. That lenses, having the smallest imperfections, should produce by flexion colours in their refracted light.

5. That there should be many more than three, or even four fringes by flexion, invisible to the naked eye. And,

6. That Iceland crystal should have some peculiarities with respect to flexion and reflexion; or if not, that some information should be acquired concerning its singular properties respecting refraction.

The manner in which the first of these propositions is demonstrated *a priori*, is evident from the 4th figure, where CD is the reflecting surface, $v o$ a concavity bearing a small ratio to CD , $A o$ and AB rays proceeding to CD . The one, AB , will be separated into Br red, and Bv violet, by deflexion from o , and will be reflected to $r'v'$, forming there the fringes. The other, $A o$, being reflected, will be separated into Bx and By , by deflexion from v , forming other fringes, xy , on the

side of $v o$'s shadow opposite to $r' v'$. Also when $v o$ is convex instead of concave, the like fringes will be produced by the rays being deflected in passing by its sides. Lastly, when $v o$ is a polished streak, images by reflexion will be produced, as described Phil. Trans. for 1796, p. 269. The same passage will also shew the reason why, on great inclinations, colours by reflexion should be produced. And the second proposition, with respect to flexion, follows from what was demonstrated in this paper (p. 357 and 358); it being that case where the rays either leave or fall on the speculum at such an inclination, as to come only within the sphere of inflexion, without being deflected. The fourth proposition is merely a simple case of flexion. And the two last require no illustration. I shall now relate how I inquired into the truth of these things *a posteriori*.

Observation 1. Looking at a plane glass mirror exposed to the sun's light, I observed that up and down its surface there were minute scratches (called hairs by workmen), and that each of these reflected a bright colour, some red, others green, and others blue. On moving the mirror to a different inclination, or my eye to a different position with respect to the mirror, I saw the species of the colours change; the red, for instance, became green, and the green blue. I applied my eye close to the mirror, and received on it the light reflected from one hair. I observed several distinct images of the sun much distended and regularly coloured, just like those described above; the same appearances were observable in all specula, metal and glass, which had these hairs, and I never saw any metal one without some: their size is exceedingly small, not above $\frac{1}{1000}$ of an inch. Rubbing a minute particle of grease on the surface of the speculum, images were seen on the fibrous surface:

and they always lay at right angles to that direction in which the grease was disposed by drawing the hand along it.

Observation 2. Besides these polished hairs, many specula have fewer or more small specks and threads, rough and black. Perhaps every polished surface is studded with a number of small ones, invisible to the naked eye from the quantity of regular light which it reflects. I took, from a reflecting telescope, a small concavespeculum not very well finished; its surface shewed several specks to the naked eye, and many with a microscope. Its diameter was $\frac{3}{5}$ of an inch, its focal distance two inches, and the sphere to which it was ground eight inches diameter. I placed it at right angles to the rays of the sun, coming through a small hole $\frac{1}{42}$ of an inch diameter, into a very well darkened room; I then moved it vertically, so that the rays might be reflected to a chart 12 inches from the speculum, and consequently 10 from the focus: and though the focus appeared white and bright, yet on the chart the broad image was very different. It was mottled with a vast number of dark spots; these were of two sorts chiefly, circular and oblong. Of the former a considerable number were distinct and large, the rest smaller and more confused, but so numerous that they seemed to fill the whole image. None were quite black, but rather of a bluish grey, and the oblong ones had a line of faint light in the middle, just as is the case in shadows of small bodies. But the chief thing which I remarked was the colours. Each oblong and round spot was bordered by a gleam of white, and several coloured fringes separated by small dark spaces. The fringes were exactly like those surrounding the shadows of bodies, of the same shape with the dark space, having the colours in the order, red on the outside, blue or violet in the inside; the in-

nermost fringe was broadest, the others decreasing in order from the first. I could sometimes see four of them, and when made at the edge of the large image, I could indistinctly discern the lineaments of a fifth: when two of the spots were very near one another, their rings or fringes ran into one another, crossing.

Observation 3. When the chart was removed to a greater distance, as six feet, the fringes were very distinct and large in proportion; also the smaller spots became more plain, and their rings were seen, though confusedly, from mixing with one another. When the speculum was turned round horizontally, so that its inclination to the incident rays might be greater, the distance of the chart remaining the same (by being drawn round in a circle), the spots and fringes evidently were distended in breadth. I have endeavoured to exhibit the sun's image, as mottled with fringes or rings and spots, in fig. 5.

Observation 4. I placed the speculum behind a screen with a hole in it, through which were let pass the homogeneous rays of the sun, separated by refraction through a prism; this being turned on its axis, the rays which fell on the speculum were changed; the fringes were now of that colour whose rays fell, and when the rays shifted, the fringes contracted or dilated, being broadest in the most flexible rays, and consequently in those whose flexity is greatest.

Observation 5. The direct light falling on the speculum, and part of the reflected light on the horizontal white stage of a very accurate micrometer, I measured the breadth of the fringes, spots, &c. These, with the distance of the speculum from the window and micrometer, and the size of the sun's image, are set down in the following table, all reduced to inches and decimals.

	Inches.	Parts.
Distance of the speculum from the hole in the window-shut - - - -	24.	
Distance of the speculum from the stage of the micrometer - - - -	18.	
Transverse axis of the sun's image - -	2.	6
Conjugate axis of the sun's image - -	1.	4
Length of the oblong dark spot - -		.4
Breadth of the oblong dark spot - -		.0074
Breadth of its first fringe - -		.0022
Elliptic spot's transverse axis - -		.0116
————— conjugate axis - -		.0068
Breadth of its first fringe - -		.0034
Transverse axis of a larger elliptic spot -		.013
Conjugate axis of the same spot - -		.0076.

In the image where these measures were taken, there were seven other elliptic spots, a little less and nearly equal; all the others were much smaller and more confused.

Observation 6. On viewing the surface of the speculum attentively in that place whence the rays formed the oblong and first mentioned elliptic spots, I saw a dark but very thin long scratch, and a dark dent, similar in shape to the dark spaces on the image; the dark spot measured less than $\frac{1}{250}$ of an inch; which makes its whole surface to the whole polished surface as 1 to 34225, supposing the former circular or nearly so. All these measures will be found to agree very well, for their smallness and delicacy: thus, the ratio last mentioned is nearly the same which we obtain by comparing the image and the spot; the like may be said of the two spots mentioned in the table,

i. e. their axes are proportional. I now could produce what spots I pleased, by gently scratching the speculum, or by making lines, dots, &c. with ink, and allowing it to dry; for these last formed convex fibres, which produced coloured fringes as well as the concavities, agreeably to what was deduced *a priori*.

Observation 7. The whole appearance which I have been describing bore such a close and complete resemblance to the fringes made round the shadows of bodies, that the identity of the cause in both cases could not be doubted. In order however to shew it still further, I measured the breadths of two contiguous fringes in several different sets; the measurements agreed very well, and gave the breadth of the first fringe .0056, and of the second .0034; or of the first .0066, and of the second .0034. The ratio of the breadths by the first is 28 to 17; by the second 30 to 17; of which the medium is 29 to 17, and this is precisely the ratio of the two innermost fringes made by a hair, according to Sir ISAAC NEWTON'S measurement: the first being, according to him, $\frac{1}{170}$ of an inch; the second $\frac{1}{290}$ of an inch.* Farther, the two innermost rings made by plates have their diameters (not breadths) in the ratio of $1\frac{1}{16}$ to $2\frac{3}{8}\dagger$, and the distance between the middle of the innermost fringes (made by a hair), on either side the shadow, is to the same distance in the second fringes as $\frac{1}{38}$ to $\frac{2}{47}$; therefore the diameters of the two first rings made by the specks in the speculum, are as $\frac{209}{663}$ to $\frac{627}{1363}$; which ratio differs exceedingly little from that of $1\frac{1}{16}$ to $2\frac{3}{8}$, the ratio of the diameters of rings made by plates, either those called by NEWTON thick, or those which

* Optics, Book 3. Obs. 3.

† Book 2. Parts 1 and 4.

he names thin : for suppose this difference nothing, $2\frac{3}{8} \times \frac{2}{6}\frac{0}{6}\frac{2}{3} = 1\frac{1}{6} \times \frac{6}{3}\frac{2}{6}\frac{2}{3}$; and the difference between these two products (now stated equal) is not much above $\frac{2}{3}$ in reality.

Observation 8. The last thing worth mentioning in these phænomena was this: I viewed the fringes through a prism, holding the refracting angle upwards, and the axis parallel to that of the dark space; then moving it till the objects ceased descending, I saw in that posture the fringes much more distinct and numerous; for I could now see five with ease, and several more less distinctly. This led me to try more minutely the truth of the 5th proposition, with respect to the number of the fringes surrounding the shadows of bodies in direct light. Having produced a bright set of these by a blackened pin $\frac{1}{25}$ of an inch in diameter, I viewed them through a well made prism, whose refracting angle was only 30° , and held this angle upwards, when the fringes were on the side of the shadow opposite to me; I then moved the prism round on its axis, and when it was in the posture between the ascent and descent of the objects, I was much pleased to see five fringes plainly, and a great number beyond, decreasing in size and brightness till they became too small and confused for sight. In like manner those formed by a double flexion of two bodies, and those made out of homogeneous light, were seen to a much greater number when carefully viewed through the prism. And this experiment I also tried with all the species of fringes by flexion which I could think of.

Observation 9. The same appearances which were occasioned by the metal speculum, might be naturally expected to appear when a glass one was used. But I also found the like rings or fringes of colours and spots in the image beyond the focus of

a lens; nor was a very excellent one belonging to a DOLLOND'S telescope free from them. The rings with their dark intervals resembled those floating specks so often observed on the surface of the eye, and called "*muscæ volitantes*," only that the *muscæ* are transparent in the middle, because formed by drops of humor: they will, however, be found to be compassed by rings of faint colours, which will become exceedingly vivid if the eyes be shut and slowly opened in the sun's light, so that the humor may be collected; they also appear by reflexion, mixed with the colours described in *Phil. Trans.* for 1796, p. 268.

Observation 10. The sun shining strongly on the concave metal speculum, placed at such a distance from the hole in the window that it was wholly covered with the light; upon inclining it a little, the image on the chart was bordered on the inside with three fringes similar to those already described; on increasing the inclination these were distended, becoming very bright and beautiful; when the inclination was great, and when it was still increased, another set of colours emerged from the side next the speculum, and was concave to that side. Here I stopped the motion, and the image on both sides of the focus had three sets of fringes, and four fringes in each set; but when viewed through a prism (as before described), the numbers greatly increased, both the fringes and the dark intervals decreasing regularly. The appearance to the naked eye is represented in fig. 6. where A D C being the image, A and C are the sets of fringes at the edges, and B the third set, there being none at E and D the sides, since the light which illuminates these quarters comes not from the edges of the speculum in so great inclinations. I now viewed the surface of the speculum, and saw it, in the place answering to B in the image, covered with

fringes exactly corresponding with those at B; and on changing the figure of that part of the speculum's edge between them and the sun, the fringes likewise had their figure altered in the very same way. On moving the speculum farther round, B came nearer to A in the image, according as the fringes on the speculum receded from that side which formed them; and before they vanished alike from the speculum and image, they mixed with the colours at A in the image, and formed in their motion a variety of new and beautiful compound colours; among these I particularly remarked a brown chocolate colour, and various other shades and tinges of brown and purple. Just before the fringes at B appeared, the space between A and C was filled with colours by reflexion, totally different in appearance from the fringes; but I could not examine them so minutely as I wished in this broad image, I therefore made the following experiment.

Observation 11. At the hole in the window-shut I held the speculum, and moved it to such an inclination that the colours by reflexion might be formed in the image; they were much brighter and far more distended than the fringes, and were in every respect like the images by reflexion in the common way, only that the colours were a little better and more regular. They were also seen on the speculum as the third set of fringes had before been in *Obs. 10.*; but by letting the rays fall on the half next the chart, and inclining that half very much, I could produce them, though less distinctly, by a single reflexion. I now held a plain metal speculum so that the rays might be reflected to form a white image on a chart. On inclining the speculum much, I saw the image turn red at the edge; it then became a little distended; and lastly, fringes

emerged from it well coloured, and in regular order, with their dark intervals. This may easily be tried by candle-light with a piece of looking-glass, and those who without much trouble would satisfy themselves of the truth of the whole experiment contained in this and the last observation, may easily do it in this way with a concave speculum; but the beauty of the appearance is hereby quite impaired. After this detail it is almost superfluous to add, that the fringes at B, fig. 6. are formed by deflexion from the edge of the speculum next the sun, and then falling on it are reflected to the chart; that the images by reflexion are either formed by the light being decomposed at its first reflexion, and then undergoing a second, or, in other instances, without this second reflexion; and that the other fringes are produced exactly as described above, from the necessary consequences of the theory. I shall only add, that nothing could have been more pleasing to me than the success of this experiment; not only because in itself it was really beautiful from its variety, but also because it was the most peremptory confirmation of what followed from the theory *a priori*, and in that point where the singularity of its consequences most inclined me to doubt its truth.

Let us now attend to several conclusions to which the foregoing observations lead, independently of the propositions (*viz.* the five first) which they were made to examine.

I. We must be immediately struck with the extreme resemblance between the rings surrounding the black spots on the image made by an ill polished speculum, and those produced by thin plates observed by NEWTON; but perhaps the resemblance is still more conspicuous in the colours surrounding the image made by any speculum whatever, and fully described in

Obs. 10. and 11. The only difference in the circumstances is now to be reconciled. The rings surrounding the black spot on the top of a bubble of water, and those also surrounding the spot between two object glasses,* have dark intervals (exactly like those rings I have just now described, and the fringes surrounding the shadows of bodies); but these intervals transmit other fringes of the same nature, though with colours in the reverse order; from which Sir ISAAC NEWTON justly inferred, that at one thickness of a plate the rays were transmitted in rings, and at another reflected in like rings. Now it is evident, that neither reflexibility nor refrangibility will account for either sort of rings, because the plate is far too thin for separating the rays by the latter, and because the colours are in the wrong order for the former; and also because the whole appearance is totally unlike any that refrangibility and reflexibility ever produce. To say that they are formed by the thickness of the plates, is not explaining the thing at all. It is demanded in what way? and indeed we see the like dark intervals and the same fringes formed at a distance from bodies by flexion, where there is no plate through which the rays pass. The state of the case then seems to be this: “when a phænomenon is produced in a particular combination of circumstances, and the same phænomenon is also produced in another combination, where some of the circumstances, before present, are wanting; we are intitled to conclude that the latter is the most general case, and must try to resolve the other into it.” In the first place, the order of the colours in the NEWTONIAN rings is just such as flexion would produce; that is, those which are transmitted have the red inner-

* Optics, Book 1.1. P. 1.

most, those which are reflected have the red outermost; the former are the colours arranged as they would be by inflexion, the latter as they would be by deflexion; and here by outermost and innermost must be understood relative position only, or position with respect to the thickness of the plate, not of the central spot. Secondly, the thinnest plate makes the broadest ring (the diameter of the rings being in the inverse subduplicate ratio of the plate's thickness); just so is it with fringes by flexion; nearer the body the fringes are broadest, and their diameters increase in the same ratio with the diameters of the rings by plates whose thickness is uniform; each distance from the bending body therefore corresponds with a ring or fringe of a particular breadth, and the alternate distances correspond with the dark intervals: the question then is, what becomes of the light which falls on or passes at these alternate distances? In the case of thin plates, this light is transmitted in other rings; we should therefore be led to think that in the case of the light passing by bodies, it should be at one distance inflected, and at another deflected; and in fact the phænomena agree with this, for fringes are formed by inflexion within the shadows of bodies; they are separated by dark intervals; the fringes and the intervals without the shadow decrease in breadth according to the same law; so that the fringes and intervals within the shadow correspond with the intervals and fringes without, respectively. Nor will this explanation at all affect the theory formerly laid down; it will only (if found consistent with farther induction) change the definite spheres of inflexion and deflexion into alternate spheres. At any rate, the facts here being the same with those described by NEWTON, but in different circumstances, teach us to reconcile the difference, which

we have attempted to do, as far as is consistent with strictness; and what we have seen not only entitles us to conclude that the cause is the same, but also inclines us to look for farther light concerning that cause's general operation: and I trust some experiments which I have planned, with an instrument contrived for the purpose of investigating the ratio of the bending power to the distances at which it acts, will finally settle this point.

II. Another conclusion follows from the experiments now related, *viz.* that we see the great importance of having specula for reflectors delicately polished; not only because the more dark imperfections there are on the surface, the more light is lost, and the more colours are produced by flexion (these colours would be mostly mixed and form white in the focus), but also because the smallest scratches or hairs, being polished, produce colours by reflexion, and these diverging irregularly from the point of incidence are never collected into a focus, but tend to confuse the image. Indeed it is wonderful that reflectors do not suffer more from this cause, considering the almost impossibility of avoiding the hairs we speak of: however, that they do actually suffer is proved by experience. I have tried several specula from reflecting telescopes, and found that though they performed very well, from having a good figure, yet from the focus (when they were held in the sun's light) several streaks diverged, and were never corrected; others had the hairs so small, that it was very difficult to perceive the colours produced by them, unless they fell on the eye. Glass concaves were freer from these hairs, but they were much more hurt by dark spots, &c. In general the hairs are so small in well wrought metals, that they do little hurt; but when en-

larged by any length of exposure to the light and heat in solar observations, they produce irregularities round the image. Such at least I take to be the explanation of the phenomenon, observed at Paris by M. DE BARROS during the transit of Mercury in 1743, and recorded in Phil. Trans. for 1753. But there is another more serious impediment to the performance of reflectors, and which it is to be feared we have no means of removing. In making the experiments of which the history has been given, on viewing attentively the surface of the speculum, every part of it was seen covered with points of colours, formed by reflexion from the small specular particles of the body. I never saw a speculum free in the least from these, so that the image formed in the focus must be rendered much more dim and confused by them, than it otherwise would be.

III. The last conclusion which may be drawn from these experiments, is a very clear demonstration in confirmation of what was otherwise shewn, concerning the difference between coloured images produced by reflexion, and those made by flexion. This complete diversity is most evident in the experiments with specula, the colours produced by which, in the form of fringes and rings, ought, as well as the others described as images by reflexion in *Obs.* 11, to be the same in appearance with those formed by pins; whereas no two things can be more dissimilar.

It remains to examine the 6th proposition: for this purpose I made the following observations.

Observation 1. Having procured a good specimen of Iceland crystal, I split it into several pieces, and chose one whose surface was best polished. I exposed this to a small cone of the sun's light, and received the reflected rays on a chart; nothing

was observable in the image, farther than what happens in reflexion from any other polished body. Some pieces, indeed, doubled and tripled the image, but only such as were rough on the surface, and consequently presented several surfaces to the rays. When smooth and well polished, a single image was all that they formed. The same happened if I viewed a candle, the letters of a book, &c. by reflexion from the Iceland crystal.

Observation 2. I ground a small piece of Iceland crystal round at the edge, and gave it a tolerable polish here and there by rubbing it on looking-glass, and sometimes by a burnisher (it would have been next to impossible to polish it completely). I then placed the polished part in the rays near the hole in the window-shut, and saw the chart illuminated with a great variety of colours by reflexion, irregularly scattered, as described above; * I therefore held the edge in the smoke of a candle and blackened it all over, then rubbed off a very little of the soot, and exposed it again in the rays. I now got a pretty good streak of images by reflexion, in no respect differing from those made in the common way. Nor could I ever produce a double set, or a single set of double images, by any specimen properly prepared, either on a chart by the rays of the sun, or on my eye by those of a candle.

Observation 3. I ground to an even and pretty sharp edge two pieces of Iceland crystal, and placed one in the sun's rays. At some feet distance I viewed the fringes with which its shadow was surrounded, and saw the usual number in the usual order. I then applied the other edge so near that their spheres of flexion might interfere in the manner before described, † and

* Phil. Trans. for 1796, p. 270.

† Ibid. p. 256.

thus the fringes might be distended; still no uncommon appearance took place; nor when other bodies were used with one edge of crystal, nor when polished pieces of different shapes and sizes were employed. The same things happened by candle-light, and also by refracted homogeneous light. In short, I repeated most of my experiments on flexion with Iceland crystal, and found that they were not changed at all in their results.

Observation 4. Having great reason to doubt the accuracy of an experiment tried by Mr. MARTIN, and in which, by a prism of Iceland crystal, he thought six spectra were produced, I was not much surprised to find, that a prism made by polishing the two contiguous sides of a parallelepiped of Iceland crystal produced only two equal and parallel images, in whatever position the prism was held. But though, from the imperfect account which MARTIN gives of this appearance, it was impossible to discover his error from his own words, yet chance led me to find out what most probably had misled him; for looking at a candle through the opposite sides of a specimen of Iceland crystal, I saw four coloured images (besides two white ones) of the candle. These were parallel to one another, and in the same line, as represented in fig. 7. where E represents the two regular images, G and F two others coloured very irregularly, and changing colours as the crystal was moved horizontally, sometimes appearing each two-fold, and its two parts of the same or different colours. A and B were regularly coloured, and evidently formed by refraction, and reflected back from the sides. On turning the crystal round, so that its position might be at right angles to its former position, the images moved round, and were in a line perpendicular to AB, as CD. All this happened in like manner in the sun's rays; and on

viewing the specimen, I found it was split and broken in the inside, so as to be lamellated in directions parallel, or nearly so, to the sides; on these plates there were colours in the day time by the light of the clouds: and it is evident that it was these fractures which caused the irregular images G and F, for other specimens shewed no such appearance. I would therefore conclude, that Iceland crystal separates the rays of light into two equal and similar beams by refraction, and no more.*

As to the cause of the separation, I would hope that some information may be obtained from the experiments I have related: for from them it appears, that this singular property extends no farther than to the action of the particles of Iceland crystal on the particles of light in their passage through the body; and from *Obs.* 4. it is farther evident, that it is not owing to the different properties which Sir ISAAC NEWTON conjectures the different sides of rays to have; for if this were the cause, when the rays pass between two pieces of crystal, an uncommon flexion would take place. Lastly, another fact (mis-stated by BARTOLIN † and ROME' DE LISLE) ‡ shews, that the unusual refraction takes place within the body, while the

* Mentioning this account of MARTIN'S mistake to Professor ROBISON, of this university, I was pleased to find a full confirmation of it. It was that excellent philosopher who shewed the appearance to MARTIN; but he not understanding it, took the liberty of publishing the observation as his own, after first mangling it in such a way as to give him, indeed, some pretext for the appropriation. The Professor merely mentioned his having communicated it to Mr. MARTIN; how the latter used it we have shewn in the text: the theory of the appearance is somewhat more complex than appears by my observations. I was therefore pleased to find that the Professor was in possession of the true account of it; which is, however, foreign to the present purpose.

† *Experimenta Crystalli*, abridged in *Phil. Trans.* Vol. V.

‡ *Cristallographie*, Vol. I.

other, like all refractions, begins at some small distance before the rays enter.

The writers just now quoted assert, that if the crystal be turned round so as to assume different positions, there is one in which the line appears single. The fact is very different, as follows. When the crystal is turned round, the unusual image moves round also, and appears above the other; the greatest distance between the two images is when they are parallel to the line bisecting one of the acute angles of the parallelogram through which the rays pass; when the images are parallel to a line bisecting one of the obtuse angles they seem to coincide; but they will be found, if observed more nearly, to coincide only in part. Thus (in fig. 9.) AB and CD are the two black lines at their greater distance, and their extremities A and C , B and D are even with one another; that is, the figure formed by joining A and C , B and D is a rectangle. But in the other case (fig. 8.) AB and CD being the lines, the space CB (equal in depth of colour to the real line on the paper), is the only place in which the lines (or images) coincide. The space AC of AB , and BD of CD are still of a light colour, and the two lines AB and CD do not coincide, by the difference AC or BD ; that is, by the difference OP , the greatest distance (fig. 9.). In short, the unusual line's extremities describe circles (in the motion of the crystal) whose centres are the extremities of the usual line, and whose radii are the greatest distance. From this it appears evident, that the unusual image is formed within the crystal, and turns round with the side of the particle, or rhomboidal mass of particles, which forms it. Farther, it is evident that the power which produces the division of the incident light, is very different from common refraction, from the motion, and

the effect taking place when the rays are perpendicular. Suspecting, therefore, that it might be owing to flexion, I made the following experiment, which undeceived me.

Observation 5. I covered one side of a specimen of Iceland crystal, three inches deep, with black paper, all but a small space $\frac{1}{30}$ of an inch in diameter, and placed a screen with a hole of the same size, six feet from the hole in the window-shut of my darkened chamber, so that the rays might pass through the screen, and fall on a prism placed behind, to refract them into a small and well defined spectrum, which was received on a chart two feet from the prism. This spectrum I viewed through the crystal, and of course saw it doubled; but the two images were by no means parallel; the unusual one inclined to the red, and its violet was considerably farther removed from the violet of the other, than the two reds were from one another; which shews, that the most refrangible or least flexible rays were farthest moved from their course by the unusual action, and proves this to be very different from flexion.*

From all these observations this conclusion follows; that the remarkable phenomenon in question arises from an action very different from either refraction or flexion; and whose nature well deserves to be farther considered. It may possibly belong to the particles of Iceland crystal, and in a degree to those of rock crystal, from the form and angles of the rhomboidal masses, whereof these bodies are composed. Nor is this conjecture at all disproved by the fact that glass shaped like these bodies wants the property; for we cannot mould the *particles* of glass, we can only shape large *masses* of these; whereas we

* When a candle or line is viewed through a deep specimen, the unusual image is tinged with colours.

cannot doubt that in crystallization the smallest masses assume the same form with the largest: but then other hypotheses may perhaps also account for the fact, such as atmospheres, electric fluid, &c. &c.; so that till farther observations are made we ought to rest contented with barely suggesting the query. In the mean time, reserving to a future opportunity some inquiries concerning the chemical properties of light, and the nature of the forces which bodies exert on it internally, I conclude at present with a short summary of propositions. But first, may I be permitted to express a hope, that what has been already attempted (and for which no praise can be claimed farther than what is due to attentive observation, according to the rules of the immortal BACON), may prove acceptable to such as love to admire the beautiful regularity of nature, or more particularly to trace her operations, as exhibited in one of the most pleasing, most important, and most unerring walks of physical science.

Proposition I. The sun's light consists of parts which differ in degree of refrangity, reflexivity, inflexity, and deflexity; and the rays which are most flexible have also the greatest refrangity, reflexivity, and flexity; or are most *refrangile, reflexile, and flexile*.

Proposition II. Rays of compound light passing through the spheres of flexion and falling on the bending body, are not separated by their flexibility, either in their approach to, or return from the body.

Proposition III. The colours of thin and those of thick plates are precisely of the same nature; differing only in the thickness of the plate which forms them.

Proposition IV. The colours of plates are caused by flexion, and may be produced without any transmission whatever.

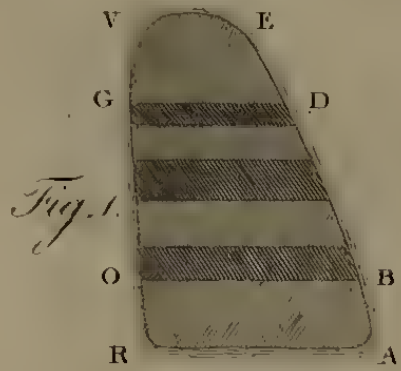


Fig. 1.

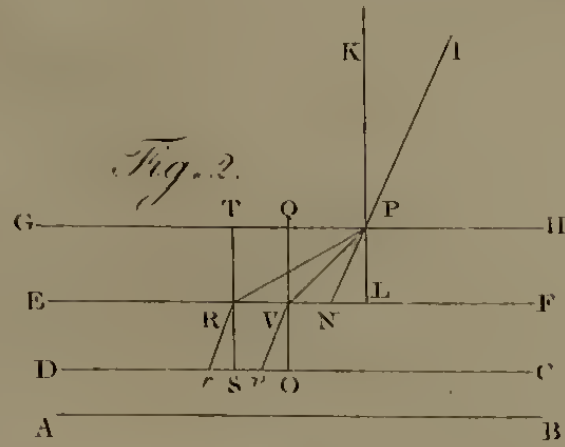


Fig. 2.

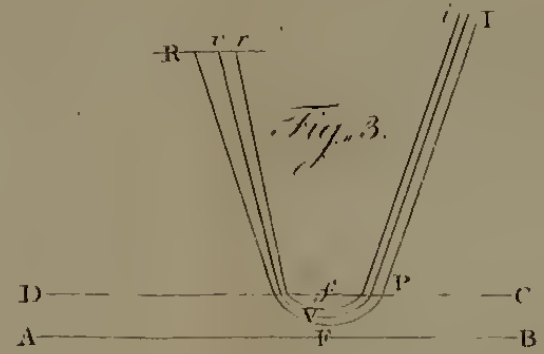


Fig. 3.

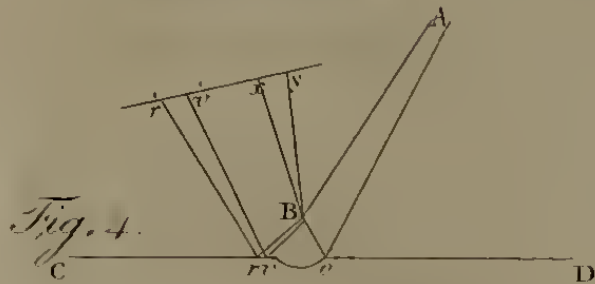


Fig. 4.



Fig. 5.

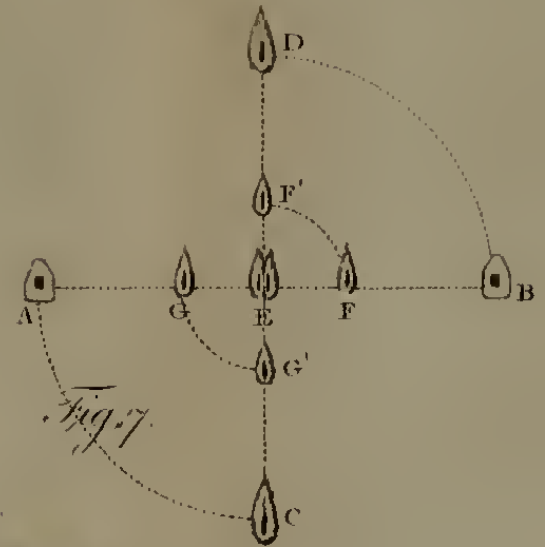


Fig. 7.



Fig. 8.

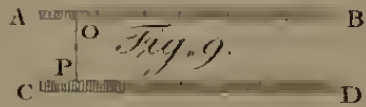


Fig. 9.

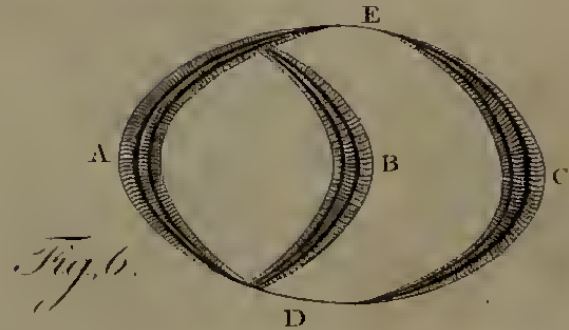


Fig. 6.



Proposition V. All the consequences deducible from the theory *a priori* are found to follow in fact.

Proposition VI. The common fringes by flexion (called hitherto the “*three fringes*”), are found to be as numerous as the others.

Proposition VII. The unusual image by Iceland crystal is caused by some power inherent in its particles, different from refraction, reflexion, and flexion.

Proposition VIII. This power resembles refraction in its degree of action on different rays; but it resembles flexion within the body, in not taking place at a distance from it, in acting as well on perpendicular as on oblique rays, and in its sphere or space of exertion moving with the particles which it attends.

XVII. *On Gouty and Urinary Concretions.* By William Hyde Wollaston, M. D. F. R. S.

Read June 22, 1797.

IF in any case a chemical knowledge of the effects of diseases will assist us in the cure of them, in none does it seem more likely to be of service than in the removal of the several concretions that are formed in various parts of the body. Of these one species from the bladder has been thoroughly examined by SCHEELE, who found it to consist almost entirely of a peculiar concrete acid, which, since his time, has received the name of lithic.

In the following paper I purpose giving an account of the analysis of gouty concretions, and of four new urinary calculi.

The gouty matter, from its appearance, was originally considered as chalk; but from being found in an animal not known to contain or secrete calcareous earth uncombined with phosphoric acid, it has since been supposed to resemble earth of bones. Dr. CULLEN has even asserted, that it is 'very entirely' soluble in acids. The assertion, however, is by no means generally true, and I think he must, in all probability, have used the nitrous acid, for I find no other that will dissolve it.

Another opinion, and, I believe, at this time the most prevalent is, that it consists of lithic acid, or matter of the calculus described by SCHEELE. But this idea is not, I believe, founded

on any direct experiments, nor is it (to my knowledge) more ably supported than by Mr. FORBES, who defends it solely by pathological arguments from the history of the disease. Had he undertaken an examination of the substance itself, he would have found that, instead of a mere concrete acid, the gouty matter is a neutral compound, consisting of lithic acid and mineral alkali; as the following experiments will prove.

(1.) If a small quantity of diluted vitriolic acid be poured upon the chalk-stone, part of the alkali is extracted, and crystals of GLAUBER'S salt may be obtained from the solution. Common salt may still more easily be procured by marine acid. The addition of more acid will extract the whole of the alkali, leaving a large proportion of the chalk-stone undissolved; which exhibits the following characteristic properties of lithic matter.

(a.) By distillation it yields a little volatile alkali, Prussic acid, and an acid sublimate, having the same crystalline form as the sublimate observed by SCHEELE.

(b.) Dissolved in a small quantity of diluted nitrous acid it tinges the skin with a rose colour, and when evaporated leaves a rose-coloured deliquescent residuum.

(c.) It dissolves readily in caustic vegetable alkali, and may be precipitated from it by any acid, and also by mild volatile alkali; first as a jelly, and then breaking down into a white powder.

(2.) In distillation of the chalk-stone the lithic acid is decomposed, and yields the usual products of animal substances, *viz.* a fetid alkaline liquor, volatile alkali, and a heavy fetid oil, leaving a spongy coal; which when burnt in open air fuses into a white salt, that does not deliquesce, but dissolves

entirely in water, is alkaline, and when saturated with nitrous acid gives rhomboidal crystals.

These characteristic properties prove it to be mineral alkali.

(3.) Caustic vegetable alkali poured upon the chalk-stone, and warmed, dissolves the whole without emitting any smell of volatile alkali. From which it appears, that the volatile alkali obtained by distillation is a product arising from a new arrangement of elements, not so combined in the substance itself.

(4.) Water aided by a boiling heat dissolves a very small proportion of the gouty concretion, and retains it when cold. The lithic acid thus dissolved in combination with the alkali, is rather more than would be dissolved alone; so that by addition of marine acid it may be separated. While the solution continues warm no precipitate is formed; but as it cools, the lithic acid crystallizes on the sides of the vessel, in the same manner as the crystals called red sand do, when an acid is added to recent urine.

The gouty concrete may be easily formed by uniting the ingredients of which I have found it to consist.

(5.) If a fragment of lithic acid be triturated with some mineral alkali and a little warm water, they unite, and after the superfluous alkali has been washed out, the remainder has every chemical property of gouty matter.

The acid will not sublime from it, but is decomposed (2.) by heat: the alkali may be extracted by the vitriolic or marine (1.) or indeed by most acids. The compound requires a large quantity of water for its solution (4.), and while warm the solution yields no precipitate by the addition of an acid; but upon its

cooling the lithic crystals form, as in the preceding experiment.

In each case the crystals are too small for accurate examination, but I have observed, that by mixing a few drops of caustic vegetable alkali to the solution previous to the decomposition, they may be rendered somewhat larger. At the first precipitation, the crystals from gouty matter were not similar to those of lithic acid; but by redissolving the precipitate in water with the addition of a little caustic vegetable alkali, and decomposing the solution as before, while hot, the crystals obtained were perfectly similar to those of lithic acid procured by the same means.

Such then are the essential ingredients of the gouty concretion. But there might probably be discovered, by an examination of larger masses than I possess, some portion of common animal fibre or fluids intermixed; but whatever particles of heterogeneous matter may be detected, they are in far too small proportion to invalidate the general result, that 'gouty matter is lithiated soda.'

The knowledge of this compound may lead to a further trial of the alkalies which have been observed by Dr. CULLEN to be apparently efficacious in preventing the returns of this disease (First Lines, DLVIII.); and may induce us, when correcting the acidity to which gouty persons are frequently subject, to employ the fixed alkalies, which are either of them capable of dissolving gouty matter, in preference to the earths (termed absorbent) which can have no such beneficial effect.

Fusible Calculus.

My next subject of inquiry has been a species of calculus, that was first ascertained to differ from that of SCHEELÉ by Mr. TENNANT; who found that when urged by the heat of a blow-pipe, instead of being nearly consumed, it left a large proportion fused into an opaque white glass, which he conjectured to be phosphorated lime united with other phosphoric salts of the urine, but never attempted a more minute analysis.

Stones of this kind are always whiter than those described by SCHEELÉ, and some specimens are perfectly white. The greater part of them have an appearance of sparkling crystals, which are most discernible where two crusts of a laminated stone have been separated from each other.

I lately had an opportunity of procuring these crystals alone, voided in the form of a white sand, and thence of determining the nature of the compound stone, in which these are cemented by other ingredients.

The crystals consist of phosphoric acid, magnesia, and volatile alkali: the stone contains also phosphorated lime, and generally some lithic acid.

The form of the crystals is a short trilateral prism, having one angle a right angle, and the other two equal, terminated by a pyramid of three or six sides.

(6.) By heat the volatile alkali may be driven off from the crystals, and they are rendered opaque (or may be partially fused). The phosphorated magnesia may then be dissolved in nitrous acid; and by addition of quicksilver dissolved in the same acid, a precipitate of phosphorated quicksilver is obtained,

from which the quicksilver may be expelled by heat, and the acid procured separate. By addition of vitriolic acid to the remaining solution, Epsom salt is formed, and may be crystallized, after the requisite evaporation of the nitrous acid, and separation of any redundant quicksilver.

(7.) These crystals require a very large quantity of water for their solution, but are readily soluble in most if not all acids; *viz.* vitriolic, nitrous, marine, phosphoric, saccharine, and acetic; and when precipitated from them re-assume the crystalline form.

(8.) From the solution in marine acid, sal ammoniac may be obtained by sublimation.

(9.) Although the analysis is satisfactory, the synthetic proof is (if possible) still more so. After dissolving magnesia in phosphoric acid, the addition of volatile alkali immediately forms the crystalline precipitate, having the same figure and properties as the original crystals.

(10.) If volatile alkali be cautiously mixed with recent urine, the same compound will be formed; the first appearance that takes place when a sufficient quantity of alkali has been gradually added, is a precipitate of these triple crystals.

These constitute the greater part of the fusible stone; so that a previous acquaintance with their properties is necessary, in order to comprehend justly the nature of the compound stone in which they are contained.

The most direct analysis of the compound stone is effected by the successive action of distilled vinegar, marine acid, and caustic vegetable alkali.

(11.) Distilled vinegar acts but slowly upon the calculus when entire; but when powdered, it immediately dissolves the

triple crystals, which may be again precipitated from it as crystals by volatile alkali; and if the solution has not been aided by heat, scarcely any of the phosphorated lime will be found blended with them.

In one trial the triple crystals exceeded $\frac{6}{10}$ of the quantity employed; but it seemed unnecessary to determine the exact proportion which they bear to the other ingredients in any one instance, as that proportion must vary in different specimens of such an assemblage of substances not chemically combined.

Marine acid, poured on the remainder, dissolves the phosphorated lime, leaving a very small residuum.

This is soluble in caustic vegetable alkali entirely, and has every other property of mere lithic acid.

The presence of volatile alkali in the compound stone may be shewn in various ways.

(12.) In the distillation of this stone there arises, first volatile alkali in great abundance, a little fetid oil, and lithic acid. There remains a large proportion charred. Water poured upon the remaining coal dissolves an extremely small quantity of a salt, apparently common salt, but too minute for accurate examination. Distilled vinegar dissolves no part of it even when powdered. Marine acid dissolves the phosphorated lime and phosphorated magnesia, leaving nothing but a little charcoal. From this solution vitriolic acid occasions a precipitate of selenite, after which triple crystals may be formed by addition of volatile alkali.

(13.) Marine acid also acts readily upon a fragment of the stone, leaving only yellowish laminæ of lithic acid. When the solution has been evaporated to dryness, sal ammoniac may be sublimed from it; and the two phosphorated earths are found

combined with more or less of marine acid, according to the degree of heat applied. If the proportion of the earth is wished to be ascertained, acid of sugar will separate them most effectually, by dissolving the phosphorated magnesia, and forming an insoluble compound with the lime.

(14.) Caustic vegetable alkali has but little effect upon the entire stone; but if heated upon the stone in powder, a strong effervescence takes place from the escape of alkaline air, and the menstruum is found to contain lithic acid precipitable by any other acid. Some phosphoric acid also, from a partial decomposition of the triple crystals, is detected by nitrated quicksilver.

(15.) The triple crystals alone are scarcely fusible under the blow-pipe; phosphorated lime proves still more refractory; but mixtures of the two are extremely fusible, which explains the fusibility of the calculus.

The appearance of the lithic strata, and the small proportion they bear to the other ingredients, shews that they are not an essential part, but an accidental deposit, that would be formed on any extraneous substance in the bladder, and which probably in this instance concretes during any temporary interval that may occur in the formation of the crystals.

I come now to what has been called

Mulberry Calculus.

This stone, though by no means overlooked, and though pointed out as differing from other species, has not, to my knowledge, been subjected to any farther analysis than is given, in the Second Volume of the Medical Transactions, by Dr. DAWSON, who found that his lixivium had little or no effect

upon it; and in the Phil. Trans. by Mr. LANE, who, among other simple and compound stones, gives an account of the comparative effects of lixivium and heat upon a few specimens of mulberry calculus (*viz.* No. 7, 8, 9, 10.); but neither of these writers attempted to ascertain the constituent parts.

Though the name has been confined to such stones as, from their irregularly knotted surface and dark colour, bear a distant resemblance to that fruit, I find the species, chemically considered, to be more extensive, comprehending also some of the smoothest stones we meet with; of which one in my possession is of a much lighter colour, so as to resemble in hue, as well as smoothness, the surface of a hemp-seed. From this circumstance it seems not improbable, that the darkness of irregular stones may have arisen from blood voided in consequence of their roughness.

The smooth calculus I find to consist of lime united with the acids of sugar and of phosphorus. The rougher specimens have generally some lithic acid in their interstices.

(16.) Caustic vegetable alkali acquires a slight tinge from a fragment of this kind of stone, but will not dissolve it. When powdered it is thereby purified from any quantity of lithic acid that it may contain. Phosphoric acid will then dissolve out the phosphorated lime, and the remainder, after being washed, may be decomposed by the vitriolic. The affinity of this acid for a certain proportion of lime is superior even to that of acid of sugar; selenite is formed, and the acid of sugar may be crystallized, and by the form of its crystals recognized, as well as by every other property. It is easily soluble, occasions a precipitate from lime water, and from a solution of selenite,

and with mineral alkali forms a salt that requires a large quantity of water for its solution.

(17.) When the stone has been finely powdered, marine acid will slowly dissolve all but any small quantity of lithic matter which it may contain. After the solution has been evaporated to dryness no part is then soluble in water, the marine acid being wholly expelled. When the dried mass is distilled with a greater heat, the saccharine acid is decomposed, and a sublimate formed, still acid and still crystallizable, but much less soluble in water, and which does not precipitate lime from lime water. After distillation the remainder contains phosphorated lime, pure lime, and charcoal; and when calcined in the open air, the charcoal is consumed and the whole reduced to a white powder. The two former may be dissolved in marine acid, which when evaporated to dryness will be retained only by the lime; so that water will then separate the muriated lime, and the phosphorated may afterwards be submitted to the usual analysis.

Bone-earth Calculus.

Beside that of SCHEELE, and the two already noticed, there is also a fourth species of calculus, occasionally formed in the bladder, distinct in its appearance, and differing in its component parts from the rest; for it consists entirely of phosphorated lime.

Its surface is generally of a pale brown, and so smooth as to appear polished; when sawed through, it is found very regularly laminated; and the laminæ in general adhere so slightly to each other, as to separate with ease into concentric crusts. In a specimen with which I was favoured by Dr. BAILLIE, each

lamina is striated in a direction perpendicular to the surface, as from an assemblage of crystallized fibres.

This calculus dissolves entirely, though slowly, in marine or nitrous acid, and, consisting of the same elements as earth of bones, may undergo a similar analysis, which it cannot be necessary to particularize.

By the blow-pipe it is immediately discovered to differ from other urinary calculi: it is at first slightly charred, but soon becomes perfectly white, still retaining its form, till urged with the utmost heat from a common blow-pipe, when it may at length be completely fused. But even this degree of fusibility is superior to that of bones. The difference consists in an excess of calcareous earth contained in bones, which renders them less fusible. This redundant portion of lime in bones renders them also more readily soluble in marine acid, and may, by evaporation of such a solution, be separated, as in the last experiment upon mulberry calculus. The remaining phosphorated lime may be re-dissolved by a fresh addition of marine acid; and being now freed from redundant lime, will, upon evaporation of the marine acid, assume a crystalline form. As the laminated calculus contains no excess of lime, *that* will at once yield such crystals: their appearance will be described in the succeeding experiment.

Calculus from the Prostate Gland.

There is still another calculus of the urinary passages, though not of the bladder itself, which deserves notice, not from the frequency of its occurrence, but from having been supposed to give rise to stone in the bladder. I mean the small stones which are occasionally found in the prostate gland. Those

that I have seen, and which, by favour of Mr. ABERNETHY, I have had an opportunity of examining, were from the size of the smallest pin's head to that of pearl barley, in colour and transparency like amber, and appeared originally to have been spherical; but from contiguity with others, some had flattened surfaces, so as at first sight to appear crystallized.

These I find to be phosphorated lime in the state of neutralization, tinged with the secretion of the prostate gland.

(18.) A small fragment being put into a drop of marine acid, on a piece of glass over a candle, was soon dissolved; and upon evaporation of the acid, crystallized in needles, making angles of about 60° and 120° with each other.

Water dropped on the crystals would dissolve no part of them; but in marine acid they would re-dissolve, and might be re-crystallized.

(19.) Vitriolic acid forms selenite with the calcareous earth.

(20.) By acid of nitrated quicksilver, phosphoric acid is readily obtained.

(21.) When heated this calculus decrepitates strongly; it next emits the usual smell of burnt animal substances, and is charred, but will not become white though partially fused. It still is soluble in marine acid, and will in that state crystallize more perfectly than before. Hence I conclude, that these stones are tinged with the liquor of the prostate gland, which in their original state (18.) somewhat impedes the crystallization.

This crystallization from marine acid is so delicate a test of the neutral phosphorated lime, that I have been enabled by that means to detect the formation of it, although the quantities were very minute. The particles of sand which are so

generally to be felt in the pineal gland, have this for their basis; for I find that after calcination they crystallize perfectly from marine acid.

I have likewise met with the same compound in a very pure state, and soft, contained in a cyst under the pleura costalis.

On the contrary, ossifications (properly so called) of arteries and of the valves of the heart, are similar to earth of bones, in containing the redundant calcareous earth; and I believe also those of veins, of the bronchiæ, and of the tendinous portion of the diaphragm, have the same excess; but my experiments on these were made too long since for me to speak with certainty.

To these I may also add the incrustation frequently formed upon the teeth, which, in the only two specimens that I have examined, proved to be a similar compound, with a very small excess of lime.

Though I do not at present presume to draw conclusions with regard to the treatment of all the diseases in question, some inferences cannot pass unobserved.

The sand from the pineal gland, from its frequency hardly to be called a disease, or when amounting to disease most certainly not known by its symptoms, would, at the same time, if known, be wholly out of the reach of any remedy.

The calculi of the prostate are too rare, perhaps, to have been ever yet suspected in the living body, and are but indirectly worthy of notice. For if by chance one of them should be voided with the urine, a knowledge of its source would guard us against an error we might otherwise fall into, of proposing the usual solvents for urinary calculi.

The bone-earth calculus, although so nearly allied to the

last, is still manifestly different, and cannot be supposed to originate from that source ; but if ever the drinking of water impregnated with calcareous earth gave rise to a stone in the bladder, this would most probably be the kind generated, and the remedy must evidently be of an acid nature.

With respect to the mulberry calculus, I fear that an intimate knowledge of its properties will leave but small prospect of relief from any solvent ; but by tracing the source of the disease we may entertain some hopes of preventing it. As the saccharine acid is known to be a natural product of a species of oxalis, it seems more probable that it is contained in some other vegetables or their fruits taken as aliment, than produced by the digestive powers, or secreted by any diseased action of the kidneys. The nutriment would therefore become a subject of minute inquiry, rather than any supposed defect of assimilation or secretion.

When a calculus is discovered, by the evacuations, to be of the fusible kind, we seem to be allowed a more favourable prospect in our attempts to relieve : for here any acid that is carried to the bladder will act upon the triple crystals, and most acids will also dissolve the phosphorated lime ; while alkalies, on the contrary, would rather have a tendency to add to the disease.

Although, from want of sufficient attention to the varieties of sediment from urine, and want of information with regard to the diversity of urinary calculi, the deposits peculiar to each concretion are yet unknown ; it seems probable that no long course of observation would be necessary to ascertain with what species any individual may be afflicted.

The lithic, which is by far the most prevalent, fortunately

affords us great variety of proofs of its presence. Particles of red sand (as they are called) are its crystals. Fragments also of larger masses, and small stones, are frequently passed; and it is probable that the majority of appearances in the urine called purulent, are either the acid itself precipitated too quickly to crystallize, or a neutral compound of that acid with one of the fixed alkalies.

Beside this species, the fusible calculus has afforded decisive marks of its presence in the case which furnished me with my specimen of triple crystals; and by the description given by Mr. FORBES (in his *Treatise upon Gravel and Gout*, ed. 1793, p. 65.) of a white crystallized precipitate, I entertain no doubt that his patient laboured under that variety of the disease.

XVIII. *Experiments on carbonated hydrogenous Gas; with a View to determine whether Carbon be a simple or a compound Substance, By Mr. William Henry. Communicated by Mr. Thomas Henry, F. R. S.*

Read June 29, 1797.

THE progress of chemical science depends not only on the acquisition of new facts, but on the accurate establishment, and just valuation, of those we already possess: for its general principles will otherwise be liable to frequent subversions; and the mutability of its doctrines will but ill accord with the unvaried order of nature. Impressed with this conviction, I have been induced to examine a late attempt to withdraw from its rank among the elementary bodies, one of the most interesting objects of chemistry. The inferences respecting the composition of charcoal, deduced by Dr. AUSTIN from his experiments on the heavy inflammable air,* lead to changes so numerous in our explanations of natural phænomena, that they ought not to be admitted without the strictest scrutiny of the reasoning of this philosopher, and an attentive repetition of the experiments themselves. In the former, sources of fallacy may, I think, be easily detected; and in the latter, there is reason to suspect that Dr. AUSTIN has been misled by inattention to some collateral circumstances. Several chemists, however, of distinguished rank have expressed themselves satisfied with the

* Phil. Trans. Vol. LXXX. p. 51.

evidence thus produced in favour of the composition of charcoal; and amongst these it may be sufficient to mention Dr. BEDDOES, who has availed himself of the theory of Dr. AUSTIN, in explaining some appearances that attend the conversion of cast into malleable iron.*

The heavy inflammable air, having been proved to consist of a solution of pure charcoal in light inflammable air, is termed, in the new nomenclature, carbonated hydrogenous gas. By repeatedly passing the electric shock through a small quantity of this gas, confined in a bent tube over mercury, Dr. AUSTIN found that it was permanently dilated to more than twice its original volume. An expansion so remarkable could not, as he observes, be occasioned by any other known cause than the evolution of light inflammable air.

When the electrified air was fired with oxygenous gas, it was found that more oxygen was required for its saturation than before the action of the electric fluid; which proves that, by this process, an actual addition was made of combustible matter.

The light inflammable air disengaged by the electrization, proceeded, without doubt, from the decomposition of some substance within the influence of the electric fluid, and not merely from the expansion of that contained in the carbonated hydrogenous gas: for had the quantity of hydrogen remained unaltered, and its state of dilatation only been changed, there would not, after electrization, have been any increased consumption of oxygen.

The only substances in contact with the glass tube and mercury, in these experiments, besides the hydrogen of the dense

* Phil. Trans. Vol. LXXXI.

inflammable gas, were carbon and water; which last, though probably not a constituent of gases, is, however, copiously diffused through them. If the evolved hydrogen proceeded from the decomposition of the former of these two substances, it is evident that a certain volume of the carbonated hydrogenous gas must yield, after electrization, on combustion with oxygen, less carbonic acid than an equal volume of non-electrified gas; or, in other words, the inflammation of 20 measures of carbonated hydrogen, expanded by electricity from 10, should not afford so much carbonic acid as 10 measures of the unelectrified.

From the fact which has been before stated, respecting the increased consumption of oxygen by the electrified air, it follows, that in determining the quantity of its carbon by combustion, such an addition of oxygen should be made, to that necessary for the saturation of the gas before exposure to the electric shock, as will completely saturate the evolved hydrogen. For if this caution be not observed, we may reasonably suspect that the product of carbonic acid is diminished, only because a part of the heavy inflammable air has escaped combustion. It might, indeed, be supposed, that in consequence of the superior affinity of carbon for oxygen, the whole of the former substance, contained in the dense inflammable gas, would be saturated, and changed into carbonic acid, before the attraction of hydrogen for oxygen could operate in the production of water. But I have found that the residue, after inflaming the carbonated hydrogenous gas with a deficiency of oxygen, and removing the carbonic acid, is not simply hydrogenous but carbonated hydrogenous gas.

In the 2d, 5th, and 6th of Dr. AUSTIN'S experiments, in which the quantity of carbon, in the electrified gas, was ex-

mined by deflagrating it with oxygen, the combustion was incomplete, because a sufficiency of oxygen was not employed; and Dr. AUSTIN himself was aware that, in each of them, "a small quantity of heavy inflammable air might escape unaltered." It is observable, also, that the product of carbonic acid, from the electrified gas, increased in proportion as the combustion was more perfect. We may infer, therefore, that if it had been complete, there would have been no deficiency of this acid gas, and consequently no indication of a decomposition of charcoal. A strong objection, however, is applicable to these, as well as to most of Dr. AUSTIN'S experiments, that the residues were not examined with sufficient attention. In one instance we are told, that the remaining gas was inflammable, and in another, that it supported combustion like vital air. I need hardly remark, that a satisfactory analysis cannot be attained of any substance, without the most scrupulous regard not only to the qualities, but to the precise quantities of the products of our operations.

To the 8th and 9th experiments, the objection may be urged with additional weight, which has been brought against the preceding ones, that the quantity of oxygen, instead of being duly increased in the combustion of the electrified gas, was, on the contrary, diminished. Thus, in the 8th experiment, 2,83 measures of carbonated hydrogen were inflamed with 4,58 measures of oxygenous gas; but in the 9th, though the 2,83 measures were dilated to 5,16, and had therefore received a considerable addition of combustible matter, the oxygen employed was only 4,09. To the rest of Dr. AUSTIN'S experiments either one or both of the above objections are applicable.

The first and most important step, therefore, in the repetition

of these experiments, is to determine, whether the carbonated hydrogenous gas really sustains, by the process of electrization, a diminution of its quantity of carbon; because, should this be decided in the negative, we derive from the fact a very useful direction in ascertaining the true source of the evolved hydrogen. The following experiments were therefore made with a view to decide this question, and the error of Dr. AUSTIN, in employing too little oxygen, was carefully avoided.*

Experiment 1. In a bent tube, standing inverted over mercury, 94,5 measures of carbonated hydrogenous gas from acetite of pot-ash, were mixed with 107,5 of oxygen. The total, 202, was reduced by an explosion to 128,5, and was farther contracted by lime water to 54. A solution of hepar sulphuris left only 23 measures.

The diminution by lime water, *viz.* 74,5 measures, makes known to us the quantity of carbonic acid afforded by the combustion of 94,5 measures of carbonated hydrogenous gas: and the residue after the action of hepar sulphuris, *viz.* 23 measures, gives the proportion of azotic gas contained in the carbonated hydrogen; for the oxygenous gaz employed, which was procured

* The apparatus employed in these experiments, was the ingenious contrivance of Mr. CAVENDISH, and is described in the LXXV. Vol. of the Philosophical Transactions. In dilating the gas, I sometimes used a straight tube, furnished with a conductor, in the manner of Dr. PRIESTLEY, (see his Experiments on Air, Vol. I. plate I. fig. 16.). The bulk of the gases introduced, and their volume after the various experiments, were ascertained by a moveable scale, and by afterwards weighing the mercury which filled the tube to the marks on the scale; by which means I was spared the trouble of graduating the syphons. Each grain of mercury indicates one measure of gas; and though the smallness of the quantities submitted to experiment may be objected to, yet this advantage was gained, that the electrified gas could be fired at one explosion, as was done in the 4th, 6th, and 8th experiments. Errors, from variations of temperature and atmospherical pressure, were carefully avoided.

from oxygenated muriate of pot-ash, was so pure, that the small quantity used in this experiment could not contain a measurable portion of azotic gas.

Experiment 2. The same quantity of carbonated hydrogen was expanded by repeated electrical shocks to 188 measures. The addition of hydrogenous gas, therefore, amounted to 93,5. The gas, thus dilated, was fired, at different times, with 392,5 measures of oxygenous gas; and the residue, after these several explosions, was 203 measures. Lime water reduced it to 128,5, and sulphure of pot-ash to 19,5. In this instance, as in the former one, the product of carbonic acid is 74,5 measures.

Finding, from the first experiment and other similar ones, that the carbonated hydrogenous gas, which was the subject of them, contained a very large admixture of azotic gas, I again submitted to distillation a quantity of the acetite of pot-ash, with every precaution to prevent the adulteration of the product with atmospherical air. Such an adulteration, I have observed, impedes considerably the dilatation of the gas, and for a time even entirely prevents it. This explains the failure, which some experienced chemists have met with, in their attempts to expand the carbonated hydrogenous gas by electricity. Gas which is thus vitiated becomes, however, capable of expansion, after exposure to the sulphure of pot-ash.

Experiment 3. Carbonated hydrogen 340 measures were exploded with the proper proportion of oxygenous gas. The carbonic acid produced amounted to 380 measures, and the residue of azotic gas was 20 measures.

Experiment 4. The same quantity, when expanded to 690, gave on combustion 380 measures of carbonic acid, and 19,8 of azotic gas.

Experiment 5. Three hundred and fifteen measures of carbonated hydrogen yielded 359 measures of carbonic acid, and 18,5 measures of azote.

Experiment 6. The same quantity, after expansion to 600, afforded the same products of carbonic acid and azotic gases.

Experiments 7 and 8. As much carbonic acid was obtained by the combustion of 408 measures of carbonated hydrogenous gas, expanded from 200, as from 200 measures of the non-electric fired gas; and the residues of azotic gas were the same in both cases.

It is unnecessary to state the particulars of several other experiments, similar to those above related, which were attended with the same results. They sufficiently prove that the action of the electric spark, when passed through carbonated hydrogenous gas, is not exerted in the decomposition of carbon; for the same quantity of this substance is found after as before electrization. Even granting that charcoal is a compound, the constituents of which are held together by a very forcible affinity, it does not appear likely that the agency of the electric shock, which seems, in this instance, analogous to that of caloric, should effect its decomposition under the circumstances of these experiments. For it is a known property of charcoal to decompose water, when aided by a high temperature; and its union with oxygen is a much more probable event, when this body is present, than a separation into its constituent principles. As an argument, also, that water is the source of the light inflammable air in this process, it may be observed, that the dilatation in Dr. AUSTIN'S experiments could never be carried much farther than twice the

original bulk of the gas.* This fact evidently implies that the expansion ceased only in consequence of the entire destruction of the matter, whose decomposition afforded the light inflammable air, and this substance could not be carbon, because Dr. AUSTIN admits that a large portion, and I have shewn that the whole of it, still remains unaltered.

If the dilatation of the carbonated hydrogenous gas arose from the decomposition of water, the effect should cease when this fluid is previously abstracted. To ascertain whether this consequence would really follow, I exposed a portion of the gas, for several days before electrization, to dry caustic alkali. On attempting its expansion, I found that it could not be carried beyond one-sixth the original bulk of the gas. By 160 very strong explosions it attained this small degree of dilatation, but 80 more produced not the least effect; though the former number would have been amply sufficient to have dilated the gas, in its ordinary state, to more than twice its original volume. A drop or two of water being admitted to this portion of gas, the expansion went on as usual; and I may here observe, that when a little water gained admission into the tube along with the gas, in any experiment, which often happened before I had acquired sufficient expertness in transferring the air from water to mercury, the dilatation went on with remarkable rapidity.

* "After the inflammable air has been expanded to about double its original bulk," says Dr. AUSTIN, "I do not find that it increases further by continuing the shocks. "Conceiving that the progress of the decomposition was impeded by the mixture of the "other airs with the heavy inflammable, I passed the spark through a mixture of the "heavy inflammable air and light inflammable; but the expansion succeeded nearly as "well as when the heavy inflammable was electrified alone." *Phil. Trans.* Vol. LXXX. p. 52.

Carbonic acid gas, according to the discovery of M. MONGE,* undergoes, when submitted to the electric shock, a change similar to that effected on the carbonated hydrogen; and the expansion has been shewn, by MESSRS. LANDRIANI and VAN MARUM,† to be owing to the same cause, *viz.* the extrication of light inflammable air. The added gas, M. MONGE ably contends, cannot proceed from any other source than the water held in solution by all aeriform bodies, the oxygen of which he supposes to combine with the mercury. That the decomponent of the water, however, in the experiments which I have described, is not a metallic body, will appear highly probable when we reflect that there is present in them a combustibile substance, *viz.* charcoal, which attracts oxygen much more strongly than metals; and the following experiments evince that the mercury, by which the air was confined, had no share in producing the phænomena.

Experiment 9. A portion of carbonated hydrogenous gas was introduced into a glass tube closed at one end, into which a piece of gold wire was inserted that projected both within and without the cavity of the tube. The open end of the tube was then closed by a stopper perforated also with gold wire, so that electric shocks could be passed through the confined air, without the contact of any metal that has the power of decomposing water. On opening the tube with its mouth downwards, under water, a quantity of air immediately rushed out.

Experiment 10. The dilatation of the gas was found to proceed very rapidly when standing over water, and exposed to the action of the electric fluid, conveyed by gold conductors.

We have only, therefore, in the two preceding experiments,

* 29 *Journal de Physique*, 277.

† 2 *Annales de Chimie*, 273.

one substance in contact with the gas which is capable of decomposing water, *viz.* charcoal. The union of this body with the oxygen of the water would be rendered palpable by the formation of carbonic acid; but Dr. AUSTIN did not observe that any precipitation was occasioned in lime water, by agitating it with the electrified gas. On passing up syrup of violets to the electrified air, with the expectation of its indicating the volatile alkali, as in the experiments of Dr. AUSTIN, no change of colour took place, though the test was of unexceptionable purity. On examining, however, whether any alteration of bulk had been produced in the air by the contact of this liquid, it appeared that of 709 measures, 100 had been absorbed. Suspecting that the absorption was owing to the presence of carbonic acid, I introduced some lime water to a volume of the expanded gas amounting to 556 measures, when they were immediately reduced to 512. The contraction would probably have been still more remarkable if the gas had been farther expanded before the admission of the liquid. The change in the lime water was very trifling; but my friend Mr. RUPP, who witnessed this as well as several of the other experiments, and who is much conversant in the observation of chemical facts, was satisfied that, after a while, he saw small flocculi of a precipitate on the surface of the mercury. This contraction of bulk cannot be ascribed to any other cause than the absorption of carbonic acid; for besides the fact that the colour of syrup of violets and of turmeric, which I also tried, were not affected by exposure to the electrified gas, I have this objection to the absorbed gas being ammoniac, that no diminution either of bulk or transparency occurred on the admixture of muriatic acid gas with the electrified air; whereas ammoniac would

have been exhibited under the form of a neutral salt. When water was passed up to this mixture of the two gases, there was an absorption not only of the muriatic gas, but of something more.

Conceiving that the demolition of charcoal, by the action of the electric fluid, was sufficiently proved by his experiments, Dr. AUSTIN assigns the evolved hydrogen as one of its constituents, and the other he concludes to be azote. This inference, however, rests almost entirely upon estimates, in which material errors may be discovered. Some of these it may be well to point out, for the satisfaction of such as have acquiesced in Dr. AUSTIN's opinion.

The carbonated hydrogenous gas submitted to Dr. AUSTIN's experiments clearly appears, from his own account, to have been largely adulterated with azotic gas. One source of its impurity he has disclosed, by informing us that the gas "had been very long exposed to water;"* for Dr. HIGGINS has somewhere shewn that the heavy inflammable air, after standing long over water, leaves a larger residue of azote, on combustion, than when recently prepared.† It is probable also, that the proportion of azote derived from the water, would increase with the time of its exposure; and thus a fertile source of error is suggested, which appears wholly to have escaped Dr. AUSTIN's attention. In repeating his experiments, I was careful that comparative ones, on two equal quantities of the

* 80 Phil. Trans. 54.

† Similar facts respecting the deterioration of other gases, by standing over water, may be seen in Dr. PRIESTLEY's Experiments on Air, Vol. I. p. 59, 158. I found that oxygenous gas, from oxygenated muriate of pot-ash, acquired, by exposure a few weeks to water, .125 its bulk of azotic gas.

electrified and unelectrified gas, should be made without the intervention of any time that could vary the proportion of azote in either of the gases.

To the 9th experiment, in which the quantity of azote seems to have been increased by electrization, I must repeat the objection, that a sufficiency of oxygenous gas was not used in the combustion. In the 8th experiment, 2,83 of the unelectrified air were fired with 4,17 oxygenous gas, and only 0,15 of the latter remained above what was sufficient for saturation; but in the 9th, though the 2,83 measures were expanded to 5,16, the quantity of oxygen employed was 0,08 less than in the former experiment; and it may therefore be presumed that a small quantity of inflammable air might escape unaltered, and might add apparently to the product of azote. In the 8th experiment also, the portion of oxygenous gas that was more than sufficient to saturate the carbonated hydrogen, would probably combine, in part, with the remaining azote, as in the experiments of Dr. HIGGINS* and Dr. PRIESTLEY.† But in the 9th, the quantity of oxygenous gas was hardly sufficient to saturate both kinds of inflammable air after electrization, and could not therefore diminish the azotic gas. When the proportion of oxygen is duly increased, and the inflammation of the electrified air is performed in small portions, there is no augmentation, but on the contrary a decrease of the quantity of the azote, as will appear on comparing the 1st and 2d of the experiments which I have related.

Two circumstances were observed, in the experiments of Dr. AUSTIN, which have not been noticed in the preceding account

* Experiments and Observations on acetous Acid, &c. p. 295.

† 79 Phil. Trans. 7.

of the repetition of them, *viz.* the appearance of a deposit from the carbonated hydrogenous gas during its electrization, and the formation of ammoniac by the same process. In some experiments, which I made on the first portion of gas, both these facts were sufficiently apparent; but neither of them occurred on electrifying the gas which was afterwards procured. Suspecting that the cessation of them arose from the superior purity of the latter portion from azotic gas, I passed the electric shock through a mixture of carbonated hydrogen with about one-fourth its bulk of azote, and thus again produced the precipitate, which would have been of a white colour, if it had not been obscured by minute globules of mercury, that were driven upwards by the force of the explosion. An infusion of violets was tinged green when admitted to the electrified gas; but the change of colour did not occur instantly, as happens from the absorption of ammoniacal gas; and required for its production that the liquid should be brought extensively into contact with the inner surface of the tube. From this effect on a blue vegetable colour, we may infer that the precipitate was an alkaline substance, and probably the carbonate of ammoniac; but the quantity was much too minute to be the subject of more decisive experiment.

I shall conclude this memoir, with a brief summary of the facts that are established by the preceding experiments.* Those included under the first head are deducible from the experiments of Dr. AUSTIN.

* Since this paper was written I have extended the inquiry to phosphorated hydrogenous gas, which expands equally with the carbonated hydrogen; loses its property of inflaming when brought into contact with oxygenous gas; and affords evident traces of a production of phosphorous or phosphoric acid.

1. Carbonated hydrogenous gas, in its ordinary state, is permanently dilated by the electric shock to more than twice its original volume; and as light inflammable air is the only substance we are acquainted with, that is capable of occasioning so great an expansion, and of exhibiting the phænomena that appear on firing the electrified gas with oxygen, we may ascribe the dilatation to the production of hydrogenous gas.

2. The hydrogenous gas evolved by this process does not arise from the decomposition of charcoal; because the same quantity of that substance is contained in the gas after, as before electrization.

3. The hydrogenous gas proceeds from decomposed water; because when this fluid is abstracted as far as possible from the carbonated hydrogenous gas, before submitting it to the action of electricity, the dilatation cannot be extended beyond one-sixth its usual amount.

4. The decomposition of the water is not a metallic substance, because carbonated hydrogenous gas is expanded when in contact only with a glass tube and gold, a metal which has no power of separating water into its formative principles.

5. The oxygen of the water (when the electric fluid is passed through carbonated hydrogenous gas, that holds this substance in solution), combines with the carbon, and forms carbonic acid. This production of carbonic acid, therefore, adds to the dilatation occasioned by the evolution of hydrogenous gas.

6. There is not, by the action of the electric matter on carbonated hydrogenous gas, any generation of azotic gas.

7. Carbon, it appears, therefore, from the united evidence of these facts, is still to be considered as an elementary body; that is, as a body with the composition of which we are unacquainted, but which may nevertheless yield to the labours of some future and more successful analyst.

XIX. *Observations and Experiments on the Colour of Blood.*
 By William Charles Wells, M. D. F. R. S.

Read July 6, 1797.

DR. PRIESTLEY is, I believe, the only person who has hitherto attempted to shew by what means common air brightens the colour of blood, which has been for some time exposed to it.* His opinion is, that the air produces this effect by depriving the blood of its phlogiston; for blood, according to the same author, is wonderfully fitted both to imbibe and to part with phlogiston, becoming black when charged with that principle, but highly florid when freed from it. Various arguments may be brought to prove that this opinion is erroneous, even upon the admission of such a principle of bodies as phlogiston. It may be said, for instance, that it is contrary to the laws of chemical affinity, that the same mass should, at one time, convert pure into phlogisticated air, by giving out its phlogiston, and immediately after reconvert phlogisticated into pure air, by imbibing that principle; both which changes are supposed by Dr. PRIESTLEY to be induced by blood upon those airs. Again; it may be urged, that, since the neutral salts, and the different alkalis, when saturated with fixed air, produce the same effect as common air upon the colour of blood, if common air acts by attracting phlogiston, those other bodies must have a similar operation. But surely it cannot be

* Phil. Trans. for 1776.

thought, that the mild volatile alkali, which has been supposed by chemists to superabound with phlogiston, can yet attract it from blood. It appears to me, however, unnecessary to bring any further arguments of this kind against the opinion of Dr. PRIESTLEY, since the following experiments will, I expect, be thought sufficient to shew, in opposition to what is taken for granted by him in the whole of his inquiry, that the alteration induced upon the colour of blood, both by common air and the neutral salts, is altogether independent of any change effected by them upon its colouring matter.

I infused a piece of black crassamentum of blood in distilled water, and immediately after covered the containing vessel closely, to prevent the access of air. Having obtained by this means a transparent solution of the red matter of blood, nearly free from serum and coagulable lymph, I exposed a quantity of it to the open air, in a shallow vessel, and poured an equal quantity into a small phial, which was then well closed. When the first portion of the solution had been exposed to the air for several hours, I decanted it into a phial, of the same size and shape as that which contained the second portion, and having added to it as much distilled water as was sufficient to compensate the loss it had suffered by evaporation, I now compared the two together, and found them to be exactly of the same colour, with regard both to kind and degree. I afterwards poured two other equal quantities of the red solution into two phials of the same size and shape. To one I added a little of a solution of nitre in water, and to the other as much distilled water. Upon comparing the two mixtures together, I found that they also possessed precisely the same colour. Lastly, I cut a quantity of dark crassamentum

of blood into thin slices, and exposed them to common air. When they became florid, I put them into a phial containing distilled water. I then took as much of the same crassamentum, which was still black, and infused it in an equal quantity of distilled water, contained in a phial similar in size and shape to the former. The two solutions which were thus obtained, one from florid blood, the other from black blood, were, notwithstanding, of precisely the same colour. These experiments were frequently repeated, and were attended with the same results, as often as I used certain precautions, which shall be mentioned hereafter, as the reasons for them will then be more readily understood than they can be at present.

Assuming therefore as proved, that neither common air, nor the neutral salts (for all those I have tried are similar to nitre in this respect) change the colour of the red matter of blood; I shall now attempt to explain the manner in which those substances give, notwithstanding, to black blood a florid appearance; premising, however, some observations upon the colours of bodies in general.

It was the opinion of KEPLER,* that light is reflected without colour from the surfaces of bodies; which he says is easily proved, by exposing to the sun's light a number of cups filled with transparent liquors of different colours, and receiving the reflexions from them upon a white ground in a dark place. ZUCCHIUS, who was younger than KEPLER, but for some time his cotemporary, taught more explicitly, † that the colours of bodies depend, not upon the light which is reflected from their anterior surfaces, but upon that portion of it which is received into their

* *Paralipomena in VITELLIONEM*, p. 23 et 436.

† *Optica Philosophia, Pars I.* p. 278 et seq.

internal parts, and is thence sent back through those surfaces. The following are some of the experiments, upon which he founded this doctrine. He exposed small round pieces of transparent glass, tinged with various colours, to the light of the sun, and received what was reflected from them upon white paper, in a darkened part of his room. He then found, that each glass produced two luminous circles, which, when the paper was sufficiently remote, were entirely separate from each other; and that the circle which proceeded from the upper surface of the glass was altogether without colour, while that which arose from the under surface, was of the same colour as the glass exhibited, when held between the light and the eye. From these experiments ZUCCHIVS also concluded, first, that every coloured body must be in some degree transparent, since a body absolutely impenetrable to light, could only reflect the colours of other bodies, but possess none of its own; and, secondly, that all bodies, which appear coloured when seen by reflected light, must be in some measure opaque; for as the light which is reflected from their surfaces comes untinged to the eye, if that part of it which penetrates their substance were afterwards to proceed in it without impediment, no colour could be exhibited by them.*

* The works of ZUCCHIVS seem very little known, though they contain a considerable number of original experiments, and though it is probable that he was the inventor of the reflecting telescope. For he says (Pars 1. p. 126.) it had occurred to him so early as 1616, that the same effect which is produced by the convex object-glass of a telescope, might be obtained by reflexion from a concave mirror; and that, after many attempts to construct telescopes with such mirrors, which proved fruitless from imperfections in their figure, he at length procured a concave mirror very accurately wrought, by means of which, and a *concave* eye-glass, he was enabled to prove his theory to be just. He does not mention at what precise time he constructed this

When Sir ISAAC NEWTON began his experiments upon light and colours, it was generally believed, that colours in opake bodies arise from some modification given to light, by the surfaces which reflect it. In opposition to one part of this opinion, our great philosopher maintained, that such bodies are seen coloured, from their acting differently upon the different colorific rays, of which white light is composed; but having established this point beyond dispute, he seems to have admitted, without inquiry, that colours are produced at the surfaces of the opake bodies to which they belong. For his experiments do not necessarily lead to such a conclusion; on the contrary, they are not more consistent with it, than they are with the opinion of KEPLER and ZUCCHIUS. This opinion, indeed, he appears not to have known; since he has taken for granted, what is contradicted by the experiments upon which it is founded, that the tinging particles of transparent bodies reflect coloured light.*

The very splendour of Sir ISAAC NEWTON'S discoveries in optics, has probably done some injury to this branch of knowledge; for soon after they were made public, it became a common opinion, that the subject of light and colours had been exhausted by that great man, and that no writer upon it before him, was now worthy of being read. The former part of this opinion has long been generally acknowledged to be unjust; but the latter part of it is still maintained by many,

telescope; but his book was printed in 1652, eleven years before the publication of the "*Optica Promota*" of JAMES GREGORY. I have not met with any account of ZUCCHIUS, in MONTUCLA'S or PRIESTLEY'S histories; in the article "telescope," in the French Encyclopedia; or in any biographical dictionary which I have consulted.

* Optics, Book i. Part II. Prop. 10.

among whom may be placed the learned Mr. DELAVAL. This gentleman has lately published * a very elaborate treatise to prove, that the colours of opake bodies do not arise from the rays of light which they reflect from their anterior surfaces; but from that portion of it, which, having penetrated their anterior surfaces, is reflected by the opake particles which are diffused through their substance. But had the learned author not believed, that no European writer upon colours, before Sir ISAAC NEWTON, contained any valuable information upon that subject, he would probably have discovered, that both KEPLER and ZUCCHIUS had long ago maintained the very opinion which he now advances, and that they had built it upon experiments similar to his own. The merit of the invention of this theory belongs, therefore, to the great KEPLER; but still much praise is due to Mr. DELAVAL, both for reviving and confirming it; since, though it be not free from defects in some of its parts, it affords solutions of several optical difficulties, which, as far as I know, admit of an explanation from no other source. Among these I regard the phænomenon which is the subject of the present inquiry.

To shew then, from the theory of KEPLER, ZUCCHIUS, and DELAVAL, how common air and the neutral salts may brighten the appearance of blood, without producing any change upon its colouring matter, I shall first suppose that all its parts have the same reflective power. The consequence will be, that a mass sufficiently thick to suffocate the whole of the light which enters it, before it can proceed to the posterior surface, and be thence returned through the first surface, must appear black:

* Manchester Memoirs, Vol. II.

for the rays which are reflected from the first surface are without colour, and, by hypothesis, none can be reflected from its internal parts. In the next place, let there be dispersed through this black mass a small number of particles, differing from it in reflective power, and it will immediately appear slightly coloured; for some of the rays, which have penetrated its surface, will be reflected by those particles, and will come to the eye obscurely tinged with the colour, which is exhibited by a thin layer of blood, when placed between us and the light. Increase now by degrees the number of those particles, and in the same proportion as they are multiplied, must the colour of the mass become both stronger and brighter.

Having thus shewn that a black mass may become highly coloured, merely by a considerable reflexion of light from its internal parts; if I should now be able to prove, that both common air and the neutral salts increase the reflexion of light from the internal parts of blood, at the same time that they brighten it, great progress would certainly be made in establishing the opinion, that the change of its appearance, which is occasioned by them, depends upon that circumstance alone. But the following observations seem to place this point beyond doubt.

I compared several pieces of crassamentum of blood, which had been reddened by means of common air and the neutral salts, with other pieces of the same crassamentum, which were still black, or nearly so; upon which I found, that the reddened pieces manifestly reflected more light than the black. One proof of this was, that the minute parts of the former could be much more distinctly seen than those of the latter. Now this increased reflection of light, in the reddened pieces,

could not arise from any change in the reflective power of their surfaces; for bodies reflect light from their surfaces in proportion to their density and inflammability; and neither of those qualities, in the reddened pieces of crassamentum, can be supposed to have been augmented by common air, or a solution of a neutral salt in water. The increased reflection must, consequently, have arisen from some change in their internal parts, by means of which much of the light which had formerly been suffocated, was now sent back through their anterior surfaces, tinged with the colour of the medium through which it had passed.

The precise nature of the change which is induced upon blood by the neutral salts, is made manifest by the following experiment. I poured upon a piece of printed card as much serum, rendered very turbid with red globules, as barely allowed the words to be legible through it. I next dropped upon the card a little of a solution of nitre in water; when I observed, that, wherever the solution came in contact with the turbid serum, a whitish cloud was immediately formed. The two fluids were then stirred together; upon which the mixture became so opaque, that the printed letters upon the card could no longer be seen. I have not hitherto been able to devise any experiment, which shews the exact change induced by common air; but it is evident that air must also, in some way, increase the opacity of blood, since it can, by no other means, increase the reflection of light from the interior parts of that body.

This theory explains another fact respecting the colour of blood, which might otherwise seem unaccountable. If a small quantity of a concentrated mineral acid be applied to a piece of dark crassamentum, the parts touched by it will for an instant

appear florid; but the same acids, added to a solution of the red matter in water, do nothing more than destroy its colour. Upon examining the crassamentum, a reason for this difference of effect is discovered; for the spots, upon which the acid was dropped, are found covered with whitish films. From which it seems evident, that the acid had occasioned an increase of opacity in the crassamentum, more quickly than it had destroyed its colour; and that the red matter, from having been in consequence seen by a greater quantity of light, had in that short interval appeared more florid than formerly.

The change which, I think, I have proved to take place in blood, when its colour is brightened by common air and the neutral salts, is similar to that which occurs to cinnabar, in the making of vermilion. This pigment, it is known, is formed from cinnabar, merely by subjecting it to a minute mechanical division. But the effect of this division is, to interpose among its particles, an infinite number of molecules of air, which, now acting as opaque matter, increase the reflection of light from the interior parts of the heap, and by this means occasion the whole difference of appearance which is observed between those two states of the same chemical body.

I expect, however, it will be said, in opposition to what I have advanced, that, granting an increased reflection of light takes place from the interior parts of blood, in consequence of the application of common air and the neutral salts, still this is not a sufficient cause for the production of the colour which they occasion; for the colour of blood, after those substances have acted upon it, is a scarlet, which, agreeably to the observation of a learned and ingenious Fellow of this Society, Dr. G. FORDYCE,*

* Elements of the Practice of Physic, p. 13.

differs not only in brightness, but also in kind, from the ordinary colour of that fluid, which is a Modena red.

My answer is, that there are examples, beside that to which the objection is made, of dark blood appearing florid, merely from its colouring matter being seen by means of an increased quantity of light. One is afforded by rubbing a piece of the darkest crassamentum with a proper quantity of serum; for a mixture is thus formed, in a few seconds, possessing a colour similar to that which is given to crassamentum by common air. But here we certainly do nothing more, than interpose among the red globules a number of the less dense particles of serum; which, in their present situation, act as opaque matter, and consequently increase the internal reflections. A second example occurs, when we view, by transmitted light, the fine edges and angles of a piece of crassamentum in water; for, in this situation, their colour appears to be a bright scarlet, though all the other parts of the same mass are black. These facts seem sufficient to prove, that the immediate cause I have assigned for the production of the florid appearance in blood, which has been exposed to the action of common air and neutral salts, is adequate to the effect; but I shall advance a step further, and shew how the Modena red is converted into a scarlet.

Blood, as I have found by experiment, is one of those fluids which Sir ISAAC NEWTON has observed appear yellow,* if viewed in very thin masses. When, therefore, a number of opaque particles are formed in it, by the action of common air and the neutral salts, many of them must be situated immediately beneath the surface. The light reflected by these will consequently be yellow; and the whole effect of the newly-

* Book i. Part II. Prop. 10.

formed opaque particles, upon the appearance of the mass, will be the same, as if yellow had been added to its former colour, a Modena red. But Modena red and yellow are the colours which compose scarlet.*

I shall now relate the cautions to be observed in making the experiments, which are described in the beginning of this paper.

The first is, that the blood should be newly drawn, and the weather cool. For as the solution of the red matter is not to be filtered, but must become transparent by the gradual subsiding of whatever may render it turbid, if the blood be old, or the weather warm, it will often assume, before it be clear, a dark and purplish hue. When exposed in this state to the atmosphere in a broad and shallow vessel, its colour changes to a bright red, which, however, is not brighter than the proper colour of the solution. The dark purplish hue seems owing to some modification of sulphur; for the solution possessing it smells like hepatic air, particularly when agitated, and tarnishes silver which is held over it. Neutral salts produce no change upon this colour.

The second caution is, that the neutral salts be not added to the red solution, except when perfectly transparent; for if it be not so, the salts will render it more turbid, and the mixture will appear brighter, if seen by reflected light.

The last I shall note is, that the red solution ought to be poured gently from the vessel in which it has been made. If it be not, as it is a mucilaginous liquor, it is apt to entangle small particles of air, which by acting as opaque matter, will for some time alter the appearance of the solution.

* FORDYCE'S Elements of the Practice of Physic, p. 14.

I proceed next to offer a few observations upon the cause of the red colour of blood.

It has of late been very generally supposed, that blood derives its colour from iron. As far as I know, however, no other argument has been given in support of this opinion, than that the red matter is found to contain that metal. But there is certainly no necessary connection between redness and iron; since this metal exists in many bodies of other colours, and even in various parts of animals without colour, as bones and wool. More direct reasons, however, may be given for rejecting this opinion.

1. I know of no colour, arising from a metal, which can be permanently destroyed by exposing its subject, in a close vessel, to a heat less than that of boiling water. But this happens with respect to the colour of blood.

2. If the colour from a metal, in any substance, be destroyed by an alkali, it may be restored by the immediate addition of an acid; and the like will happen from the addition of a proper quantity of alkali, if the colour has been destroyed by an acid. The colour of blood, on the contrary, when once destroyed, either by an acid or an alkali, can never be brought back.

3. If iron be the cause of the red colour of blood, it must exist there in a saline state, since the red matter is soluble in water. The substances, therefore, which detect almost the smallest quantity of iron in such a state, ought likewise to demonstrate its presence in blood; but upon adding Prussian alkali, and an infusion of galls, to a very saturate solution of the red matter, I could not observe, in the former case, the slightest blue precipitate, or in the latter, that the mixture had acquired the least blue, or purple tint.

Upon the whole it appears to me, that blood derives its colour from the peculiar organization of the animal matter of one of its parts; for whenever this is destroyed, the colour disappears, and can never be made to return; which would not, I think, be the case, if it depended upon the presence of any foreign substance whatsoever.

I shall conclude this paper with relating several miscellaneous facts respecting the colour of blood, and some conclusions which may be formed from them.

Dr. PRIESTLEY has mentioned,* that the only animal fluid, beside serum, which he found to transmit the influence of common air to blood, was milk. But I have observed, that the white of an egg possesses the same property, notwithstanding its great tenacity. Now as serum contains an animal substance very similar to the white of eggs, it occurred to me as a question, whether, in transmitting the influence of air to blood, it acts by its salts only, or partly by means of the substance of which I have just spoken. I took therefore a quantity of urine, which is known to contain nearly the same salts as serum, and having added to it as much distilled water as rendered its taste of the same pungency as that of serum, I poured the mixture upon a piece of dark crassamentum of blood. I then put to another piece of the same crassamentum an equal quantity of serum, and exposed both parcels to the atmosphere. The result was, that the blood in the diluted urine did not become nearly so florid as that in the serum. I have found also, that a solution of sugar in water conveys the influence of air to blood; from which it seems probable, that milk owes its similar property to the saccharine matter which it contains. Black blood

* Phil. Trans. for 1776, p. 246.

exposed to the atmosphere under mucilage of gum arabic, does not become florid.

It has been said,* that neither serum, nor solutions of the neutral salts, dissolve the red matter of blood. But this induction has been made from too small a number of experiments. For saturate solutions of all the neutral salts, which I have tried, will extract, though slowly, red tinctures from blood, some of which are very deep; and neither they, nor serum, added in any proportion to a solution of the red matter in water, alter its colour or transparency, except by diluting it. The following experiments, however, will place this point in a clearer light.

I added a drachm of distilled water to an ounce of serum, and poured the mixture upon a small piece of crassamentum. Upon an equal piece of crassamentum I poured a drachm of water, and after some time added an ounce of serum. Each parcel, therefore, contained the same quantity of crassamentum, serum, and water; but the crassamentum upon which the mixture of serum and water had been poured, communicated no tinge to it; while the other piece, to which water had been first applied, and afterwards serum, gave a deep colour to the fluid above it. I made similar experiments with crassamentum, water, and a dilute solution of a neutral salt, which were attended with the same results.

Since then neither serum, nor a dilute solution of a neutral salt, will extract colour from blood, though they are both capable of dissolving the red matter, when separated by water from the other parts of the mass, it follows, in my opinion, that what are called the red globules consist of two parts, one

* FORDYCE'S Elements of the Practice of Physic, p. 14.

within the other, and that the outer, being insoluble in serum or dilute solutions of neutral salts, defends the inner from the action of those fluids. It is remarkable, that microscopical observations led Mr. HEWSON to the same conclusion, namely, that the red globules consist of two parts,* which, according to him, are an exterior vesicle, and an interior solid sphere. But the same writer, upon the authority of other microscopic experiments, asserts that the vesicles are red. If they be so, there must exist two red matters in the blood, possessing different chemical properties; which is certainly far from being probable.

The exterior part of the globule appears to be that ingredient of the blood upon which common air and the neutral salts produce their immediate effect, when they render the whole mass florid; for I have shewn they do not act upon the red matter itself, and I have not found that they occasion any change in coagulated lymph or serum. The only matter then which remains to be operated upon, is that which I have mentioned. It seems evident also, from what has been just stated, that there exists an animal matter in the blood, different from the coagulable lymph, the coagulable part of the serum, the putrescent mucilage, and the red particles, which, I believe, are all the kinds it has hitherto been supposed to contain.

The microscopical observations of Mr. HEWSON appear likewise to furnish a reason, why both water, and a saturate solution of a neutral salt, can extract colour from the red globules, though a mixture of these fluids be incapable of the same effect. For water applied to the red globules, separates the exterior vesicles from the red particles, which are therefore now

* HEWSON'S Works, Vol. III. p. 17.

open to the action of any solvent.* The addition, however, of a small quantity of a neutral salt to the water enables the vesicles to preserve their shape, and to retain the inner spherules.† Upon the addition of a greater quantity of salt, the vesicles contract, and apply themselves closely to the red particles within.‡ Thus far Mr. HEWSON'S observations extend. Let it now be supposed that the vesicles contract still more, from a further addition of salt to the water; the consequence must be, that, as the internal particles are incompressible, the sides of the vesicles will be rent, and their contents exposed to the action of the surrounding fluid. Both water and a strong solution of a neutral salt may, therefore, destroy the organization of the vesicles, though in different ways, and thus agree in bringing the red matter in contact with a solvent; while a mixture of those two fluids, namely, a dilute solution of a neutral salt, will, by hardening the vesicles, increase the defence of the red matter against the action of such substances as are capable of dissolving it. But all reasoning founded upon experiments with microscopes, ought perhaps to be regarded as, in great measure, conjectural.

* HEWSON'S Works, Vol. III. p. 17. † Ibid. p. 40. ‡ Ibid. p. 31.

XX. *An Account of the Trigonometrical Survey, carried on in the Years 1795, and 1796, by Order of the Marquis Cornwallis, Master General of the Ordnance. By Colonel Edward Williams, Captain William Mudge, and Mr. Isaac Dalby. Communicated by the Duke of Richmond, F. R. S.*

Read May 11, 1797.

P A R T F I R S T.

PREAMBLE.

ACCORDING to the resolution expressed in the account of the Trigonometrical Survey, printed in the Philosophical Transactions for the year 1795, we now communicate to the public, through the same channel, a farther relation of its progress.

On referring to the above paper, it will be found that, for the prosecution of this undertaking, a design was formed of proceeding to the westward, with a series of triangles, for the survey of the coast. This intention has been carried into effect; and as the small theodolite, or circular instrument, announced in our former communication as then in the hands of Mr. RAMSDEN, was finished early in the summer of 1795, we are enabled to give a series of triangles, extending, in conjunction with those before given, from the Isle of Thanet, in Kent, to the Land's End.

In the composition of the following account, we have adhered to the plan adopted in the last, of giving the angles of

the great triangles, with their variations; and we have, with as much brevity as possible, inserted a narrative of each year's operations. This will be found, however, to extend only to the First Part, or that containing the particulars of the survey in which the great instrument alone was used. The remaining contents of this portion of the work, are necessarily confined to the angles of the principal, and secondary triangles, with the calculations of their sides, in feet; and likewise such *data* as have no connection with the computations of latitudes and longitudes.

Part the Second contains an account of a survey carried on in Kent, in the years 1795 and 1796, with the small instrument, by order of the Master General, for completing a map of the eastern and southern parts of that county, for the use of the Board of Ordnance, and the military commanders on the coast.

In Part the First will be found an article, for which we are indebted to Dr. MASKELYNE, the Astronomer Royal. It contains his demonstration of M. DE LAMBRE'S formula, in the *Connoissance des Temps* of 1793, for reducing a distance on the sphere to any great circle near it, or the contrary. The *practical rule* thence derived, for reducing the angles in the plane of the horizon, to those formed by the chords, is very useful, and will considerably abridge the trouble which must necessarily arise in computing the chord corrections by any former method.

SECTION FIRST.

ARTICLE I. *Of Particulars relating to the Operations of the Year 1795.*

In an early part of this season, from the necessity which existed of completing the map of Kent, mentioned in the preamble, we had conceived that our former intentions, of continuing the survey towards the west, would for the present be relinquished; as it was not imagined that the telescope of the small circular instrument, then in the hands of Mr. RAMSDEN, could be applied, with good effect, in observing staffs erected on very distant stations.

From the obvious importance, however, of adhering to the first resolution, it was determined that a trial should be made of the excellence of this instrument, in the construction of which extraordinary pains had been taken, by operating with it in Kent, and using it for those purposes to which, if the object before spoken of had not been in view, the great theodolite would have been necessarily applied.

This smaller theodolite, therefore, as a substitute, was in May taken into Kent by Mr. DALBY, and Mr. GARDNER, chief draughtsman in the Tower; the assistance of the former being necessary, as the stations in the series of 1787 were for the most part unknown to the latter gentleman.

As the former paper, relating to the trigonometrical survey, could not be presented to the Royal Society before the 4th of June, the business did not commence till the 12th of the same month. The party then left London, and the instrument was taken to Bull Barrow, in Dorsetshire.

On a reference to the account of 1795, it will be seen, that a station was chosen near Lulworth, and observed both from Nine Barrow Down and Black Down. It was also intended to be observed from Bull Barrow; by which means the great triangle, formed by the stations Black Down, Nine Barrow Down, and Bull Barrow, would be divided into, and made to consist of, two smaller triangles. This, however, it was now found could not be done, as a signal house had been erected near the station at Lulworth, subsequent to the operations in 1794, which prevented that spot from being afterwards seen at Bull Barrow: but no consequences very injurious can have arisen from the impracticability of making use of this station in the manner originally proposed, since the stations formerly chosen in Portland, with which that of Lulworth was also intended to connect, have not been visited with the instrument. The stations in that island were selected with a view of observing from them, and Charton Common, some point in the vicinity of Torbay, which might be a proper station in the series intended to be carried along the coast. Such a situation, however, could not be conveniently found, as the view of Devonshire from Charton Common is much interrupted by trees and other obstacles; and it would have been highly improper to shorten the side between Pilsden Hill and the coast, by choosing a station more remote from the latter than Charton Common.

As from an inspection of the plan of the triangles annexed to this account, a doubt may be entertained as to the propriety of carrying on so very extensive a series from the short side connecting the stations on Black Down and Mintern Hill; it must be observed that, admitting the necessity of adopting Bull Barrow for a station, those on Pilsden and Mintern Hills were

naturally chosen; the first, because it connected with Dumpdon (a station that could not be dispensed with); and the second, because it was the point most remote from Black Down, being on the brow of the high land overlooking the general surface of Somersetshire.

To connect with the station formerly chosen near Maiden Bradley, two others were selected whilst the party were at Bull Barrow; one on Ash Beacon, near Sherborne, and the other on the Quantock Hills. Both these have very commanding views, and will hereafter easily unite with any stations which may be chosen to the northward.

From Bull Barrow, the instrument was successively taken to the following stations, before any other new ones were chosen, *vi.* Mintern, Pilsden, and Charton Common; and whilst the party were at the latter, nearly all the stations were selected in Devonshire. In the choice of these, much difficulty occurred, as the face of this county is particularly unfavourable for operations of this kind. Around Honiton and Chard, there are several small ranges of hills, nearly of an equal height, running in parallel directions. Near the former are three, thus circumstanced; *viz.* Hembury Fort, Combe Raleigh, and Dumpdon. From the first and second of these, the station on Charton Common is not visible; and it is from the last only, that both Pilsden and the Quantock Hills can be seen. This station, however, has a disadvantage: Combe Raleigh, which is to the west of it, takes off all view round Tiverton and Silferton; so that it became indispensably necessary to select a spot on the northern extremity of Dartmoor, called Cawsand Beacon.

To those who are acquainted with the interior of Dartmoor,

it will be unnecessary to assign the reason for not having chosen any station towards its centre. It may be sufficient to observe, that two spots were found on its circumference, which render the want of it trifling in its consequences.

Independent of the stations to which, as we have before observed, the instrument was taken this year, the following were visited, *viz.* Dumpdon, Little Haldon, Furland, and Butterton. From the latter, the party returned to London in the month of October.

ART. II. *Angles taken in the Year 1795.*

At Bull Barrow.

Between	°	'	"	Mean.
Mintern Hill and Black Down -	46	54	33	} 34
			34,75	
			34	
Black Down and Nine Barrow Down	84	31	22,25	} 23,25
			24	
Nine Barrow Down and Wingreen	93	33	0,5	} 0,25
		32	59,75	

At Mintern Hill.

Bull Barrow and Black Down -	101	39	30	} 30,5
			31,25	
Black Down and Pilsden - -	68	30	45,75	} 46,5
			47	

On Charton Common.

Little Haldon and Dumpdon -	68	12	49,75	} 51,25
			51,25	
			52,75	
Dumpdon and Pilsden - -	93	54	36,25	} 37,25
			37,5	
			38	

Between				Mean.
Pilsden and Black Down	-	-	47 39 17,5 19,25	} 18,5

On Pilsden Hill.

Mintern and Black Down	-		44 37 51,5 52,5 53 54,25 55,5	} 53,25
Black Down and Charton Common	105	5	25,75 26 26	} 26
Charton Common and Dumpdon	47	32	0,25 1,25 2,5	} 1,25

At Dumpdon.

Charton Common and Little Haldon	86	39	7 7,25 8,5 8,75 9,25	} 8,25
Little Haldon and Cawsand Beacon	35	7	6,5 6,75 8,25	} 7,25
Pilsden and Charton Common	-	38 33	22 22,25 22,25 23 23,5 23,5	} 22,75

At Little Haldon.

Furland and Rippin Tor	-	-	84 58 42 43	} 42,5
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	.	'	"	Mean.
Between				
Rippin Tor and Cawsand Beacon	29	30	9,25	} 10,5
			11	
			11	
Dumpdon and Charton Common	25	8	0,75	} 1,25
			2	
Dumpdon and Furland -	143	52	32,75	} 33,25
			33	
			34	

At Furland.

The Bolt Head and Butterton -	53	15	34,25	} 35
			35,75	
Butterton and Rippin Tor -	43	38	4	} 4,5
			5,25	
Rippin Tor and Little Haldon -	39	24	36,75	} 37,25
			37,75	

At Butterton.

Rippin Tor and Furland - -	74	21	56	} 57,25
			56,5	
			57,25	
			58	
			58,5	
Furland and the Bolt Head -	63	47	50,75	} 50,75
			50,75	
The Bolt Head and Kit Hill -	127	37	36,5	} 36,5
			36,75	
Maker Heights and Kit Hill -	42	11	38,75	} 38,75
			38,75	
Maker Heights and Carraton Hill	35	30	28	} 28,75
			28,75	
			29,75	

ART. III. *Of Particulars relating to the Operations of the
Year 1796.*

In the account of this Survey, published in the Philosophical Transactions for 1795, page 473, it is stated, that large stones were sunk in the ground at the extremities of the base of verification on Salisbury Plain. To render these points permanent, two iron cannon (selected from among the unserviceable ordnance in Woolwich Warren) were, towards the end of February, sent to Salisbury, and in the beginning of March inserted at the ends of the base. The same methods were adopted, for the purpose of fixing these cannon in their proper positions, as those made use of when similar *termini* were sunk in the ground on Hounslow Heath. This operation having been completed on the 10th of March, the instrument was shortly after carried to Kit Hill, in Cornwall; a station, like that on Bindown, chosen rather for the purpose of a secondary, than a principal place of observation.

It would be tedious, and perhaps unnecessary, to enumerate the names of all the stations selected this year, as many of them do not form any part of the series now given to the public. We shall, therefore, confine ourselves to such remarks on the subject as may serve to abridge this article.

We have before stated, that a station was chosen on Cawsand Beacon, the northern extremity of Dartmoor, for the purpose of connecting with Dumpdon. It should have been observed, that to the westward of the former eminence, and near it, there is a hill considerably higher, which in point of situation has many advantages, but which cannot be made

use of on account of the ruggedness of its surface, which seems to render the carrying of the instrument to its top almost impossible. From this circumstance, and similar impediments, which the high lands remote from the circumference of Dartmoor offer to our operations, it results, that the body of this moor cannot have any great triangles carried over it: such stations were therefore selected this year as may serve, in conjunction with others, to include this tract of country in a polygon of a small number of sides.

To make observations for the purpose of hereafter determining the longitude and latitude of the Lizard, was a principal object in this year's operations; and as this headland seems to offer itself as very convenient for a station, it will be right to assign our reasons for not having chosen one upon it.

As no other spot but Hensbarrow Beacon could be found in that part of Cornwall proper for a station, it became necessary to fix on the Deadman, or Dodman, for another point in the series. From this place no part of the land within four miles of the Lizard can be seen, as the high ground about Black Head, which is to the eastward of the latter, is nearly in a line between them, and is also much higher than both. It will be perceived, however, that no evil can result from the want of such a station, as the light-houses and the naval-signal-staff at the Lizard, have been intersected from several stations. The precise spot on which Mr. BRADLEY made his observations in the year 1769, for ascertaining the longitude and latitude of this headland, was pointed out by the person having the care of the light-houses, who well remembered the common particulars relating to his operations: such measurements were made from the light-houses to this spot, as may enable us, at a future

period, to compare the results from the *data* afforded by the trigonometrical operation, with those deduced from the astronomical observations made by the above gentleman. It may be also mentioned, that angles were at the same time taken at the western light-house and signal-staff, for the purpose of finding the situation of the Lizard Point.

We are now to speak of the most important business performed this year; that of making observations to determine the distance of the Scilly Isles from the Land's End.

To do this as accurately as possible, it became necessary to find stations affording the longest *base*. The hill near *Rosemergy*, called the *Watch*, and the station near St. Buryan, are certainly the most advantageous places, because all the islands can be seen from both; but we could not avail ourselves of the former, as difficulties almost insuperable would have attended an attempt to get the instrument upon it. Another station was therefore selected, on Karnminnis, near *St. Ives*; a spot as well situated as the place spoken of, provided all the islands could be seen: this, however, does not prove to be the case, *St. Martin's Day-Mark* being the only object in the Scilly Islands visible from Karnminnis.

From the stations near the Land's End (Sennen and Pertinney), as well as that above mentioned (St. Buryan), St. Agnes' Light-house, and two objects in St. Mary's, were observed; and as the means by which all their distances are determined, except those of the Day-Mark, from the shortness of the bases (which were, however, the longest that could be found) are exceptionable, it will be right to mention, that while we were engaged in that part of the operation now spoken of, the air was so unusually clear, that we could sometimes, with the

telescope of the great theodolite, discover the soldiers at exercise in St. Mary's island.

Under this article, it will be convenient to state, that we have endeavoured to find some spot to the westward, on which a base might be measured. Had we been fortunate in this respect, it undoubtedly would be eminently advantageous; as those triangles, now extended to the Land's End, would, in that case, be verified in some part of the new series. In Devonshire and Cornwall, however, no place has been discovered by any means fit for the purpose; so that our communicating this work, under the circumstances attending it, is a matter of necessity.

In the present and former seasons, such stations were selected and observed, as were judged to be proper for the future use of the small instrument; and as we had experienced, in the early stage of this Survey, much delay and disappointment from the white lights not being always seen when fired on distant stations, we have since substituted lamps and staffs in their stead. The operations of the present year were continued till October, when the party returned to London.

ART. IV. *Angles taken in the Year 1796.*

At Kit Hill.

Between		o	'	"	Mean.
Butterton and Maker Heights	-	48	36	45	} 46,5
				47,75	
Maker Heights and Bindown	-	53	21	13,75	
Carraton Hill and Bindown	-	50	45	31	

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*The Account of a**On Maker Heights.*

Between		°	'	"	Mean.
Lansallos and Carraton Hill	-	48	39	54,75 } 54,75	
Carraton and Butterton	-	112	18	7,75 } 8,75	
				9,75 }	
Butterton and the Bolt Head	-	45	54	35,75 } 37	
				38,5 }	
Bindown and Carraton Hill	-	28	22	50,75	
Bindown and Kit Hill	-	51	29	20,5 } 22,5	
				24,5 }	
Kit Hill and Butterton	-	89	11	33,25 } 34,75	
				36 }	

At the Bolt Head.

Maker Heights and Butterton	-	48	39	24,5 } 24,75	
				24,75 }	
Butterton and Furland	-	62	56	36,5	

At Rippin Tor.

Cawsand Beacon and Little Haldon	124	59	12,75 } 13		
			13,5 }		
Little Haldon and Furland	-	55	36	39 } 40,5	
			41,75 }		
Furland and Butterton	-	61	59	59,25 } 59,5	
			59,5 }		

On Cawsand Beacon.

Dumpdon and Little Haldon	-	43	14	20 } 21,25	
			22,5 }		
Little Haldon and Rippin Tor	-	25	30	39,5 } 39,75	
			40,25 }		

On Carraton Hill.

Between		°	'	"	Mean.
Maker Heights and Lansallos	-	67	12	20,25 23,5	} 21,75
Lansallos and Bodmin Down	-	56	21	16,75 17	} 17
Lansallos and Hensbarrow Beacon		37	28	57,75 58	} 58
Butterton and Maker Heights	-	32	11	22,5 23,5	} 23
Kit Hill and Bindown	-	91	45	22,5	
Maker Heights and Bindown	-	38	58	38,5	

On Bindown.

Lansallos and Carraton Hill	-	119	9	36,25	
Carraton Hill and Kit Hill	-	37	29	5,75	
Kit Hill and Maker Heights	-	75	9	24,5	

At Lansallos, or Polvinton Farm.

Deadman and Hensbarrow Beacon		52	34	2 2,5 5	} 3
Hensbarrow Beacon and Bodmin Down		45	1	10,75 12,75	} 11,75
Bodmin Down and Carraton Hill		54	57	43,25 44,75	} 44
Carraton Hill and Bindown	-	32	36	43,25	
Carraton Hill and Maker Heights		64	7	43,5 43,75 45,75	} 44,25

On Bodmin Down.

Carraton Hill and Lansallos	-	68	40	57,75 41 0,75 40 58,5	} 59
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Between	°	'	"	Mean.
Lansallos and Hensbarrow Beacon	67	59	27,5 28	} 27,75

On Hensbarrow Beacon.

Carraton Hill and Lansallos	-	42	32	8,5	
Bodmin Down and Lansallos	-	66	59	21,75 25	} 23,25
Lansallos and Deadman	-	71	13	35 35,25 35,5	} 35,25
Deadman and St. Agnes' Beacon		77	20	28,5 28,75 31,5	} 29,5

On St. Agnes' Beacon.

Hensbarrow Beacon and Deadman		34	31	17 21 23	} 20,25
Deadman and Karnbonellis	-	75	51	53 53,75	} 53,25
Karnbonellis and Karnminnis	-	57	46	31 31,5	} 31,25

On Karnminnis.

St. Agnes' Beacon and Karnbonellis		32	30	0,25 02,5	} 0,25
Karnbonellis and St. Buryan	-	111	53	15,5 16,5	} 16
St. Buryan and Pertinney	-	13	48	16,75 17 20,75	} 18

At St. Buryan.

Karnminnis and Karnbonellis	-	41	43	45,25 45,5 45	} 45,25
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Between				Mean.
Pertinney and Karnminnis	-	52 31	27,5 27,5	} 27,5
Sennen and Pertinney	- -	75 36	11 11,75 12	} 11,5

At Sennen.

Pertinney and St. Buryan	-	36 39	18,5 19,25	} 18,75
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On Pertinney.

Karnminnis and St. Buryan	-	113 40	15,25 16	} 15,5
St. Buryan and Sennen	- -	67 44	30,5 31,25	} 31

At Karnbonellis.

St. Buryan and Karnminnis	-	26 22	59,25 59,5	} 59,25
Karnminnis and St. Agnes' Beacon		89 43	27,25 28,75 31,25	} 29
St. Agnes' Beacon and the Deadman		78 16	39,75 40,5 43	} 41

On the Deadman, or Dodman Point.

Karnbonellis and St. Agnes' Beacon		25 51	24,5 24,75	} 24,75
St. Agnes' Beacon and Hensbarrow Beacon		68 8	12,5 13,75	} 13
Hensbarrow Beacon and Lansallos		56 12	22,5 22,75	} 22,75

ART. V. *Situations of the Stations.*

Mintern, or Revel's Hill. This station is in Dorsetshire, and situated on Revel's Hill, which is not far from Mintern. It is 17 feet N. E. from the corner of the hedge.

Pilsden. This station is also in Dorsetshire, and near Broadwindsor. The point is on the S. E. corner of the old parapet.

Charton Common. The station is in the field adjoining to, and also to the westward of, the Common, and is about two miles from Lyme: it is 50 yards from the eastern hedge, and may be easily found, as Black Down is only visible from that spot, being seen between two trees.

Dumpdon; about three miles N. E. of Honiton. The station is 10 feet northward of the hedge of the plantation, and nearly on the highest part of the hill.

Little Haldon; near Teignmouth, in Devonshire. The station is 80 yards from the *Direction Post*, and in a line with it and the Obelisk on *Great Haldon*.

Cawsand Beacon; near South Zeal. The station is about 200 feet north of the Karn, or great heap of stones.

Rippin Tor. This station is also on Dartmoor, and about 5 miles from Ashburton. The point is mid-way between the two heaps of stones.

Furland; a field near the turnpike-gate between Brixen and Dartmouth. The station is near the stone, erected in the middle of the field.

Butterton. The station is 45 feet S. W. of the Karn, on the hill called by this name, and about 1 mile from Ivy Bridge.

The Bolt Head. The station is on the spot called *White Soar*, above the Bolt; it is 95 feet in the line produced, north-

ward, from the west side of the signal-house, and about 90 feet from the nearest corner of it.

Maker Heights. This spot is near Cawsand, and the station is 45 feet from the great flag-staff, in the line produced from Statten Battery passing by the side of the staff.

Kit Hill, near Callington. The station is on the S. W. bastion of a work, similar to an Indian fortification.

Carraton Hill. This station is about 4 miles north of Liskeard; and the point 150 yards south of the highest Karn on the top of the hill.

Bindown, near Looe. The station is 50 yards eastward of the barrow on this hill.

Lansallos. The station is in a field belonging to *Polvinton Farm*, which is near that town. The point is 159 feet from the western bank, and $90\frac{1}{2}$ from the southern one.

On Bodmin Down. The station 120 yards south of the high road, and about a quarter of a mile east of the turnpike gate. The point is in the centre of a remarkable ring.

Hensbarrow Beacon, near St. Roach. The station is on the top of the barrow.

The Deadman, or Dodman Head. The station is about 40 feet south of the bank, and nearly 100 yards to the east of the entrance into the inclosure.

St. Agnes' Beacon. The station is on the southern brow of the beacon, and about 80 yards from the tower.

Karnbonellis. The station is 90 yards south of the northern Karn, or heap of stones. The hill called *Karnbonellis* is near *Porcillis*.

Pertinney. The station is in the middle of the ring on its top. This hill is about 2 miles eastward of *St. Just*.

Sennen. This station is in the north-west corner of a field belonging to Mr. WILLIAMS. The field may be easily found, as there is no other spot near the town of *Sennen*, from which the Longship's Light-house, Pertinney, and St. Buryan, can be seen.

Karnminnis, near St. Ives. The station on the top of this hill, may be found from the following measurements :

	Feet.	In.		
The station from 3 large	8	8	from the south	}
moor-stones, south of	11	0	— north	
the hedge.	14	1	— west	

St. Buryan. The station is in a field adjoining the town, and by the side of the *Penzance* road. It is $84\frac{1}{2}$ feet from the stile, and 48 feet from a large stone in the northern hedge. This stone is 81 feet from the stile; the station, this stone, and Chapel Karnbury, being in a right line.

ART. VI. *Demonstration of M. de Lambre's Formula in the Connoissance des Temps of 1793, for reducing a Distance on the Sphere to any great Circle near it, or the contrary.* By Nevil Maskelyne, D. D. F. R. S. and Astronomer Royal.

Put A = angle subtended by two terrestrial objects; a = the same reduced to the horizon; H, b the two apparent altitudes: if either is a depression, it must be taken negative.

By spherics, $c, A = c, a.c, H.c, b + s, H.s, b.$

Put $A = a + d a$, where $d a$ signifies $A - a$, and not their differential.

By trigonometry $c, A = c, a.c, d a - s, a.s, d a = c, a \times \sqrt{1 - v s}, d a - s, a.s, d a = c, a - c, a \times 2 s^2, \frac{1}{2} d a - s, a.s, d a$ (by theo-

rem above) = $c, a \cdot c, H \cdot c, b + s, H \cdot s, b \cdot \cdot s, da + 2s^2,$
 $\frac{1}{2} da \cdot 't, a = 't, a - 't, a \cdot c, H \cdot c, b - s, H \cdot s, b \times \text{cosec. } a$

$$= 't, a - 't, a \times \frac{1}{2} c, \overline{H - b} + \frac{1}{2} c, \overline{H + b} - \text{cosec. } a$$

$$\times \frac{1}{2} c, \overline{H - b} - \frac{1}{2} c, \overline{H + b} \text{ (because } 't, a = \frac{1}{2} 't, \frac{1}{2} a - \frac{1}{2} t, \frac{1}{2} a;$$

$$\text{and } \text{cosec. } a = \frac{1}{2} 't \frac{1}{2} a + \frac{1}{2} t, \frac{1}{2} a) = \frac{1}{2} 't', \frac{1}{2} a - \frac{1}{2} t, \frac{1}{2} a$$

$$\times 1 - \frac{1}{2} c, \overline{H - b} - \frac{1}{2} c, \overline{H + b} - \frac{1}{2} 't, \frac{1}{2} a + \frac{1}{2} t, \frac{1}{2} a$$

$$\times \frac{1}{2} c, \overline{H - b} - \frac{1}{2} c, \overline{H + b} = \frac{1}{2} 't, \frac{1}{2} a \times 1 - c, \overline{H - b}$$

$$- \frac{1}{2} t, \frac{1}{2} a \times 1 - c, \overline{H + b} = \frac{1}{2} 't, \frac{1}{2} a \times vs, \overline{H - b} - \frac{1}{2} t, \frac{1}{2} a$$

$$\times vs, \overline{H + b} = 't, \frac{1}{2} a \cdot s^2, \frac{1}{2} \overline{H - b} - t, \frac{1}{2} a \cdot s^2, \frac{1}{2} \overline{H + b}.$$

Put $n = 't, \frac{1}{2} a \cdot s^2, \frac{1}{2} (H - b) - t, \frac{1}{2} a \cdot s^2, \frac{1}{2} (H + b),$

We shall have

$$s, da + 2s^2, \frac{1}{2} da \cdot 't, a = n;$$

$$\text{and } s, da = n - 2s^2, \frac{1}{2} da \cdot 't, a.$$

$$\text{But } s, da = 2s, \frac{1}{2} da \cdot c, \frac{1}{2} da$$

$$\therefore s, \frac{1}{2} da = \frac{s, da}{2c, \frac{1}{2} da} = \frac{n - 2s^2, \frac{1}{2} da \cdot 't, a}{2c, \frac{1}{2} da},$$

$$\text{and } s, da = n - 2s^2, \frac{1}{2} da \cdot 't, a = n - 2t', a \left(\frac{n - 2s^2, \frac{1}{2} da \cdot 't, a}{2c, \frac{1}{2} da} \right)^2,$$

$$\text{because } \left. \frac{n - 2s^2, \frac{1}{2} da \cdot 't, a}{2c, \frac{1}{2} da} \right|^2 = \frac{n - 4n \cdot s^2, \frac{1}{2} da \cdot 't, a + 4s^4, \frac{1}{2} da \cdot 't^2, a}{4 \times 1 - s^2, \frac{1}{2} da}$$

$$= \frac{n^2}{4} + \frac{n^2 s^2, \frac{1}{2} da}{4} - n \cdot s^2, \frac{1}{2} da \cdot 't, a - n \cdot s^4, \frac{1}{2} da \cdot 't, a$$

$$+ s^4, \frac{1}{2} da \cdot 't^2, a) = n - \frac{1}{2} n^2 \cdot 't, a - \frac{1}{2} n^2 \cdot 't, a \cdot s^2, \frac{1}{2} da$$

$$+ 2n \cdot 't^2, a \cdot s^2, \frac{1}{2} da + 2n 't^2, a \cdot s^4, \frac{1}{2} da - 2 't^3, a \cdot s^4 + \frac{1}{2} da,$$

by substituting for $s, \frac{1}{2} da$ its near value $n,$

$$= n - \frac{1}{2} n^2 't, a - \frac{n^4 't, a}{8} + \frac{1}{2} n^3 't^2, a + \frac{1}{8} n^5 't^2, a - \frac{1}{8} n^4 't^3, a,$$

where the last term but one containing the 5th power of n may be rejected, as it has been omitted by M. DE LAMBRE.

As da is always very small, the arc da in parts of the radius, unity, = s, da in parts of the same radius, therefore

$s, 1'' : 1'' : s, da$ (in parts of radius unity) : $\frac{1}{s, 1''} \times s, da = da$ in seconds,

$$\begin{aligned} &= \frac{1''}{s, 1''} \times n - 2s^2, \frac{1}{2} da . 't, a = \frac{1''}{s, 1''} \times n - da . s, \frac{1}{2} da . 't, a = \frac{1'' \times n}{s, 1''} \\ &- \frac{1'' \times da . s, \frac{1}{2} da . 't, a}{s, 1''} \cdot \cdot \text{if we put } n = \frac{1''}{s, 1''} \times t', \frac{1}{2} a . s, \frac{1}{2} (H - b) \\ &- t, \frac{1}{2} a . s^2, \frac{1}{2} (H + b), \text{ and } da = a \text{ number of seconds, we} \\ &\text{shall have} \end{aligned}$$

$da = n - da . s, \frac{1}{2} da . 't, a$; and, for the most part, without any sensible error, $da = n - n . s, \frac{1}{2} n . 't, a$.

Table I. contains $\frac{1'' \times t, \frac{1}{2} a}{10000}$, and $\frac{1'' \times t, \frac{1}{2} a}{10000}$; Table II. contains $10000 \times s^2, \frac{1}{2} (H \mp b)$. Table III. contains the term $-n . s, \frac{1}{2} n . 't, a$. The argument on the side is a , and that on the top is n or the result found by the help of the two first tables. If this correction should be considerable, with the value of da , found after this correction has been applied, enter Table III. again at the top, and with a on the side as before; the number now found subtracted from n will give the correct value of da .

By the investigation,

$da = \frac{1}{2} 't, \frac{1}{2} a . vs \overline{H} \simeq b - \frac{1}{2} t, \frac{1}{2} a . vs, \overline{H} \pm b - vs, da . 't, a$, where the upper or lower signs are to be used, according as the objects are on the same, or on contrary sides of the great circle to which they are referred; the third term will be negative or positive, according as a is less or more than 90° .* If da should come out negative, A will be less than a , or a greater than A . In the case of reducing a spheric angle to the angle

* Compute the two, which will give the approximate value of da , and make use of them in computing the third term; and join the three terms together according to their signs, which will give da still nearer; and, if this should prove considerable, compute the third term a second time with the new value of da .

between the chords, the spheric angle will be represented by a , and the angle between the chords by $A = a + da$; and $da = \frac{1}{2}t, \frac{1}{2}a \cdot vs, \overline{H \sim b} - \frac{1}{2}t, \frac{1}{2}a \cdot vs, \overline{H + b} - vs, da \cdot t, a$ (if D, d represent the arcs to the chords) $= \frac{1}{2}t, \frac{1}{2}a \cdot vs, \frac{1}{2}(D \sim d) - \frac{1}{2}t, \frac{1}{2}a \cdot vs, \frac{1}{2}(D + d) - vs, da \cdot t, a$;
 $A = a - (\frac{1}{2}t, \frac{1}{2}a \cdot vs, \frac{1}{2}\overline{D + d} - \frac{1}{2}t, \frac{1}{2}a \cdot vs, \frac{1}{2}D \sim d) - vs, da \cdot t, a$; where the last term will change its sign to affirmative, if a is greater than 90° . If the answer is required in seconds, the correction must be multiplied by 206265, the number of seconds in an arc = radius. The calculation will be easily made by logarithms.

Practical Rule.

The practical rule deduced from the above conclusions is the following, and given in the words of the Astronomer Royal.

“ To the constant logarithm 5,0134 add $L. t, \frac{1}{2}a$ and $L. vs \overline{D + d}$; the sum diminished by 20 in the index is the logarithm of the first part of the value of da in seconds, which is always negative. To the constant logarithm 5,0134 add $L. t, \frac{1}{2}a$, and $L. vs, \frac{1}{2}\overline{D \sim d}$, the sum diminished by 20 in the index, is the logarithm of the second part in seconds, which is always affirmative. These two joined together, according to their proper signs, will give the approximate value of da . To its logarithmic versed sine, add $L. t, a$ and constant logarithm 5,3144, the sum, diminished by 20 in the index, will be the logarithm of the third part in seconds, which will be negative or affirmative, according as a is less or more than 90° . This applied according to its sign, to the

“ approximate value of $d a$, will give the correct value of $d a$.
 “ If the third part comes out considerable, it should be com-
 “ puted anew with the last value of $d a$. The value of $d a$,
 “ finally corrected, applied to a , will give A , the angle between
 “ the chords.”

In the application of the above rule, to the computation of such corrections as may be applied to the angles of any triangles in this survey, it is manifest that the last step may be entirely neglected on account of the smallness of the *approximate* value of $d a$, whose versed sine is one of the arguments. Being, therefore, confined to the use of the two first steps, the operation is very short. An example is here given in the computation of the correction for reducing the angle at Chanctonbury Ring in the 39th triangle, given in the last account (see Phil. Trans. for 1795, p. 492), to that formed by the chords.

EXAMPLE.

Constant logarithm	-	-	5,0134	-	-	-	-	5,0134
Log. tang. $\frac{1}{2} a = 78^\circ 56'$	-	-	10,7112	Log. co. tang. $\frac{1}{2} a$	-	-	-	9,2887
Log. vs. $\frac{1}{2} \cdot \overline{H + b} = 19' 53''{,}5$			5,2237	Log. vs. $\frac{1}{2} \cdot \overline{H - b} = 5' 53''{,}5$				4,1669
			0,9483					- 2,4690 + 0'',03
								- 8,88
								+ 0,03
								- 8,85 the correction required.

SECTION SECOND.

Calculation of the Sides of the great Triangles, carried on from the Termination of the Series, published in the Philosophical Transactions of the Year 1795, along the Coasts of Dorsetshire, Devonshire, and Cornwall, to the Land's End.

Distance from Wingreen to Nine Barrow Down, 130224,5 Feet (see Phil. Trans. for 1795).

No. of triangles	Names of stations.	Observed angles.	Diff.	Spherical excess.	Error.	Angles corrected for calculation.	Distances.
XLIII.	Wingreen -	54 29 36,5	-0,4	"	"	54 29 36	Feet.
	Bull Barrow -	93 33 0,25	-0,91			93 32 59	
	Nine Barrow Down	31 57 25,5	-0,4			31 57 25	
		180 0 2,25		1,72	+0,53		
	Bull Barrow from { Wingreen - - Nine Barrow Down - -						69058 106213
XLIV.	Black Down -	56 30 18,75	-0,53			56 30 18,5	
	Nine Barrow Down	38 58 19,25	-0,89			38 58 19	
	Bull Barrow -	84 31 23,25	-0,57			84 31 22,5	
		180 0 1,25		1,99	-0,74		
	Black Down from { Nine Barrow Down - Bull Barrow - -						126782 80103,8
XLV.	Mintern -	101 39 30,5	-0,36			101 39 30	
	Bull Barrow -	46 54 34	-0,09			46 54 33,5	
	Black Down -	31 25 57,5	-0,11			31 25 56,5	
		180 0 2		0,59	+1,41		
	Mintern from { Bull Barrow - - Black Down - -						42653,4 59730

No. of triangles	Names of stations.	Observed angles.	Diff.	Spherical excess.	Error.	Angles corrected for calculation.	Distances.
XLVI.	Pilsden - -	0 1 #				0 1 #	Fect.
	Mintern Hill - -	44 37 53,25	-0,29	"	"	44 37 53	
	Black Down - -	68 30 46,5	-0,36			68 30 46	
		66 51 21,25	-0,36			66 51 21	
		180 0 1		1	-0,02		
	Pilsden from {						
		Mintern Hill	-	-			78177
		Black Down	-	-			79110,7
XLVII.	Charton Common	47 39 18,5	-0,10			47 39 18,5	
	Black Down - -	27 15 14	-0,21			27 15 16	
	Pilsden - - -	105 5 26	-0,60			105 5 25,5	
		179 59 58,5		0,88	-2,38		
	Charton Common from {						
		Black Down	-	-			103345
		Pilsden	-	-			49106,3
XLVIII.	Dumpdon - -	38 33 22,75	-0,12			38 33 22,25	
	Pilsden - - -	47 32 1,25	-0,14			47 32 1	
	Charton Common	93 54 37,25	-0,36			93 54 36,75	
		180 0 1,25		0,66	+0,59		
	Charton Common from {						
		Dumpdon	-	-			49016,3
		Pilsden	-	-			78459,3
XLIX.	Little Haldon -	25 8 1,25	-0,45			25 8 1	
	Charton Common	68 12 51,25	-0,48			68 12 51	
	Dumpdon - -	86 39 8,5	-0,78			86 39 8	
		180 0 1		0,66	+0,34		
	Little Haldon from {						
		Charton Common	-	-			136353
		Dumpdon	-	-			126831

No. of triangles	Names of stations.	Observed angles.	Diff.	Spherical excess.	Error.	Angles corrected for calculation.	Distances.
L.	Cawsand Beacon	0' 43" 14"	-0,57	"	"	0' 43" 14"	Fct. 181334 106508
	Dumpdon -	21,25 35' 7"	-0,64			21,25 35' 7"	
	Little Haldon -	33,75 101' 38"	-1,93			33,75 101' 38"	
		180 0 2,25		3,12	-0,87		
	Cawsand Beacon from { Dumpdon - - Little Haldon - -						
L I.	Rippin Tor -	124 59 13	-0,08			124 59 11,75	64020,5 55988,7
	Cawsand Beacon	25 30 39,75	+0,01			25 30 38,75	
	Little Haldon -	29 30 10,5	+0,05			29 30 9,5	
		180 0 3,25		0,69	+2,56		
	Rippin Tor from { Cawsand Beacon - - Little Haldon - -						
L II.	Furland - -	39 24 37,25	-0,26			39 24 37	72776 87851
	Little Haldon -	84 58 43	-0,44			84 58 42,75	
	Rippin Tor -	55 36 40,5	-0,25			55 36 40,25	
		180 0 0,75		0,96	-0,21		
	Furland from { Little Haldon - - Rippin Tor - -						
L III.	Furland -	43 38 4,5	-0,32			43 38 4	62951 80547,8
	Rippin Tor -	61 59 59,5	-0,38			61 59 59,25	
	Butterton -	74 21 57,25	-0,44			74 21 56,75	
		180 0 1,25		1,15	+0,1		
	Butterton from { Rippin Tor - - Furland - -						

No. of triangles	Names of stations.	Observed angles.	Diff.	Spherical excess.	Error.	Angles corrected for calculation.	Distances.
LIV.	Bolt Head -	62 56 30,5	-0,41	"	"	62 56 35,25	Feet. 81152 72479,8
	Furland -	53 15 35	-0,38			53 15 34,75	
	Butterton -	63 47 50,75	-0,43			63 47 50	
		180 0 2,25		1,23	+ 1,02		
	Bolt Head from {	Furland -	-	-	-	-	81152
		Butterton -	-	-	-	-	72479,8
LV.	Maker Heights	45 54 37	-0,42			45 54 37,5	100591 75760,8
	Bolt Head -	48 39 24,5	-0,33			48 39 24,5	
	Butterton -	85 25 58	-0,59			85 25 58	
		179 59 59,5		1,29	- 1,79		
	Maker Heights from {	Bolt Head -	-	-	-	-	100591
		Butterton -	-	-	-	-	75760,8
LVI.	Maker Heights	112 18 8,75	-1,09			112 18 8	131576 82600,3
	Butterton -	35 30 28,75	-0,17			35 30 29	
	Carraton Hill -	32 11 23	-0,10			32 11 23	
		180 0 0,5		1,36	- 0,86		
	Carraton Hill from {	Butterton -	-	-	-	-	131576
		Maker Heights -	-	-	-	-	82600,3
LVII.	Lansallos -	64 7 44,25	-0,44			64 7 44	84631,4 68929,7
	Maker Heights -	48 39 54,75	-0,36			48 39 54,5	
	Carraton Hill -	67 12 21,75	-0,43			67 12 21,5	
		180 0 0,75		1,24	- 0,49		
	Lansallos from {	Maker Heights -	-	-	-	-	84631,4
		Carraton Hill -	-	-	-	-	68929,7

By the latter triangle we get the distance from Lansallos to Carraton Hill 68929,7 feet; which being obtained from the least number of triangles, we shall make use of in the calculations of the sides farther to the westward. The same conclusion, however, is nearly obtained by making the computations pass through the triangles connected with Kit Hill and the station on Bindown.

Distance from Butterton to Maker Heights 75760,8 feet.

No. of triangles.	Names of stations.	Observed angles.	Diff.	Spherical excess.	Error.	Angles corrected for calculation.	Distances.
LVIII.	Kit Hill - -	48 36 46,75	-0,26	"	"	48 36 46,75	Feet. 100969 67822,3
	Butterton - -	42 11 38,75	-0,20			42 11 38,75	
	Maker Heights	89 11 34,5	-0,75			89 11 34,5	
		180 0 0		1,21	-1,21		
	Kit Hill from	{	Butterton - - -	-	-	-	100969
			Maker Heights - - -	-	-	-	67822,3
LIX.	Bindown - -	75 9 24,5	-0,28			75 9 24,25	56294,8 54902,7
	Maker Heights	51 29 22,5	-0,17			51 29 22,25	
	Kit Hill - -	53 21 13,75	-0,22			53 21 13,5	
		180 0 0,75		0,70	+0,05		
	Bindown from	{	Maker Heights - - -	-	-	-	56294,8
			Kit Hill - - -	-	-	-	54902,7
LX.	Carraton Hill	91 45 22,5				91 45 23	33427 42541,4
	Kit Hill - -	50 45 31				50 45 31	
	Bindown - -	37 29 5,75				37 29 6	
		179 59 59,25		0,42	-1,17		
	Carraton Hill from	{	Kit Hill - - -	-	-	-	33427
			Bindown - - -	-	-	-	42541,4
LXI.	Lansallos - -	32 36 43,25				32 36 42,25	37335,3
	Bindown - -	119 9 36,25				119 9 35,25	
	Carraton Hill	28 13 43,25				28 13 42,5	
		180 0 2,75		0,33	+2,42		
	Lansallos from Bindown	-	-	-	-	-	37335,3

By the last triangle we get the distance from Lansallos to Carraton 68931 feet. We shall, however, as before observed, use the distance between those stations as derived from the LVII. triangle.

No. of triangles	Names of stations.	Observed angles.	Diff.	Spherical excess.	Error.	Angles corrected for calculation.	Distances.
LXII.	Lansallos -	0 1 44	-0,26	"	"	0 1 44	Feet.
	Carraton Hill	54 57 44	-0,27			54 57 44	
	Bodmin Down	56 21 17	-0,30			56 21 17	
		68 40 59				68 40 59	
		180 0 0		0,82	-0,82		
	Bodmin Down from { Carraton Hill Lansallos - - -						60582,7 61597,1
LXIII.	Hensbarrow Beacon	66 59 23,25	-0,23			66 59 22,25	
	Bodmin Down	67 59 27,75	-0,21			67 59 26,75	
	Lansallos -	45 1 11,75	-0,15			45 1 11	
		180 0 2,75		0,63	+1,12		
	Hensbarrow Beacon from Bodmin Down -						47337,2

By this last triangle, the distance from Hensbarrow Beacon to Lansallos is found to be 62044,8 feet, and by the following triangle

LXIV.	Hensbarrow Beacon	42 32 8,5	-0,20			42 32 8	
	Carraton Hill	37 28 58	-0,18			37 28 57,5	
	Lansallos -	99 58 55,75	-0,59			99 58 54,5	
		180 0 2,25		0,99	+1,26		
	Hensbarrow Beacon from Carraton Hill -						100416

we get 62044,7 feet for the same distance.

LXV.	Deadman -	56 12 22,75	-0,25			56 12 22,5	
	Lansallos - -	52 34 3	-0,24			52 34 2,5	
	Hensbarrow Beacon	71 13 35,25	-0,35			71 13 35	
		180 0 1		0,82	+0,18		
	Deadman from { Lansallos - - Hensbarrow Beacon - -						70686,8 59284,2

No. of triangles	Names of stations.	Observed angles.	Diff.	Spherical excess.	Error.	Angles corrected for calculation.	Distances
LXVI.	St. Agnes' Beacon	0 1 20,25	-0,31	"	"	0 1 19,25	Fect. 97084,8 102066
	Hensbarrow Beacon	77 20 29,5	-0,54			77 20 28,75	
	Deadman -	68 8 13	-0,63			68 8 12	
		180 0 2,75		1,32	+1,43		
	St. Agnes' Beacon from { Hensbarrow Beacon - Deadman - -						
LXVII.	St. Agnes' Beacon	75 51 53,75	-0,40			75 51 53,5	101084 45461,9
	Deadman -	25 51 24,75	-0,30			25 51 25,25	
	Karnbonellis -	78 16 41	-0,40			78 16 41,25	
		179 59 59,5		1,06	-1,56		
	Karnbonellis from { Deadman - - - St. Agnes' Beacon - - -						
LXVIII.	Karnminnis -	32 30 0,25	-0,22			32 30 0,25	84610,6 71578,3
	St. Agnes' Beacon	57 46 31,25	-0,35			57 46 31	
	Karnbonellis -	89 43 29	-0,53			89 43 28,75	
		180 0 0,5		0,77	-0,27		
	Karnminnis from { St. Agnes' Beacon - Karnbonellis - - -						
LXIX.	St. Buryan -	41 43 45,5	-0,03			41 43 45,25	99786 47786,7
	Karnbonellis -	26 22 59,25	-0,09			26 22 59,25	
	Karnminnis -	111 53 16	-0,65			111 53 15,5	
		180 0 0,75		0,75	0,0		
	St. Buryan from { Karnbonellis - - - Karnminnis - - -						

No. of triangles	Names of stations.	Observed angles.	Diff.	Spherical excess.	Error.	Angles corrected for calculation.	Distances.
LXX.	Pertinney -	113 40 15,5	"	"	"	113 40 15	Feet. 41407,7 12450,2
	Karnminnis -	13 48 18				13 48 18	
	St. Buryan -	52 31 27,5				52 31 27	
		180 0 1		0,16	+0,84		
			Pertinney from { Karnminnis - - St. Buryan - -				
LXXI.	Sennen - -	36 39 18,75				36 39 18,25	19300,8 20199,9
	St. Buryan - -	75 36 11,5				75 36 11	
	Pertinney - -	67 44 31				67 44 30,75	
		180 0 1,25		0,08	+1,17		
			Sennen from { St. Buryan - - - Pertinney - - -				

The angles in the above series of triangles, are those arising from taking the means of the several observations: and the same rules have been adopted for their corrections, which were laid down in the account of the trigonometrical operation, published in the *Philosophical Transactions* for 1795. The angle at Blackdown in the XLVII. triangle (for the triangles of the present series are numbered in order from those of the former), is considered to be nearly 2" in defect, and has been augmented for calculation accordingly: the angle at that station was observed under circumstances less favourable, than those which attended the observations made on Pilsden, and Charton Common.

SECTION THIRD.

*Heights of the Stations. Terrestrial Refractions.*ART. I. *Elevations and Depressions.**At Wingreen.*

The ground at Bull Barrow - - depressed 6' 3"

At Nine Barrow Down.

The ground at Black Down - - depr. 3 29
 at Bull Barrow - - elevated 1 25

At Black Down.

The ground at Nine Barrow Down - - depr. 13 26
 at Charton Common - - depr. 15 11
 at Mintern Hill - - 0 0
 at Bull Barrow - - depr. 1 16
 at Pilsden - - - - depr. 0 50

At Pilsden Hill.

The ground at Black Down - - depr. 11 0
 at Charton Common - - depr. 28 39
 The horizon of the sea on the 6th of June,
 at 6 P. M. in a S. E. direction, nearly, depr. 29 23

At Bull Barrow.

The ground at Wingreen - - - - depr. 4 53
 at Mintern - - - - - - - - depr. 6 5
 at Black Down - - - - - - - - depr. 10 39

On Charton Common.

The ground at Black Down	-	-	0	0
at Pilsden	-	-	<i>elev.</i>	20 37
at Haldon	-	-	<i>depr.</i>	3 33

At Dumpdon.

The ground at Pilsden	-	-	<i>depr.</i>	3 45
at Charton	-	-	<i>depr.</i>	22 12
The bottom of the Karn, or heap of stones, (nearly on a level with the axis of the tele- scope) on Cawsand Beacon	-	-	} <i>elev.</i>	4 49

At Haldon.

The ground at Charton	-	-	<i>depr.</i>	15 59
at Cawsand Beacon	-	-	<i>elev.</i>	24 3
at Rippin Tor	-	-	<i>elev.</i>	40 49
at Furland	-	-	<i>depr.</i>	16 6
The horizon of the sea on the 27th of July, at 6 P. M. in a S.-W. direction, nearly,			<i>depr.</i>	27 24

On Cawsand Beacon.

The ground at Rippin Tor	-	-	<i>depr.</i>	17 42
at Haldon	-	-	<i>depr.</i>	38 57
The lamp at Dumpdon	-	-	<i>depr.</i>	29 36
<i>N. B.</i> The lamp was about $5\frac{1}{2}$ feet from the ground.				

On Rippin Tor.

The ground at Butterton	-	-	<i>depr.</i>	23 38
at Cawsand Beacon	-	-	<i>elev.</i>	8 3
at Haldon	-	-	<i>depr.</i>	49 31

At Furland.

The ground at Haldon	-	-	elev.	5	¹ / ₂	27
at Butterton	-	-	elev.	20		15

At Butterton.

The ground at Kit Hill	-	-	-	depr.	10	49
at Carraton	-	-	-	depr.	9	0
at Maker Heights	-	-	-	depr.	41	48
at the Bolt Head	-	-	-	depr.	41	48
at Furland	-	-	-	depr.	32	18
at Rippin Tor	-	-	-	elev.	13	54

On Maker Heights.

The ground at Lansallos	-	-	-	depr.	1	27
at Bindown	-	-	-	elev.	11	32
at Carraton Hill	-	-	-	elev.	27	36
at Kit Hill	-	-	-	elev.	29	45
at Butterton	-	-	-	elev.	30	45
at the Bolt Head	-	-	-	depr.	5	47

At the Bolt Head.

The ground at Maker	-	-	-	depr.	7	42
at Butterton	-	-	-	elev.	31	6

At Kit Hill.

The ground at Butterton	-	-	-	depr.	1	42
at Maker Heights	-	-	-	depr.	37	38
at Bindown	-	-	-	depr.	3	0
at Carraton Hill	-	-	-	elev.	9	38

On Carraton Hill.

The ground at Lansallos	-	-	-	<i>depr.</i>	41	18
at Hensbarrow	-	-	-	<i>depr.</i>	13	27
at Maker Heights	-	-	-	<i>depr.</i>	39	30
at Bindown	-	-	-	<i>depr.</i>	47	48
at Butterton	-	-	-	<i>depr.</i>	9	48
at Kit Hill	-	-	-	<i>depr.</i>	15	19

On Bindown.

The ground at Maker Heights	-	-	-	<i>depr.</i>	19	41
at Carraton Hill	-	-	-	<i>elev.</i>	41	20
at Lansallos	-	-	-	<i>depr.</i>	16	24
at Hensbarrow	-	-	-	<i>elev.</i>	7	10
at Kit Hill	-	-	-	<i>elev.</i>	22	51

At Lansallos.

The ground at Carraton Hill	-	-	-	<i>elev.</i>	30	18
at Bindown	-	-	-	<i>elev.</i>	10	46
at Kit Hill	-	-	-	<i>elev.</i>	15	27
at Bodmin Down	-	-	-	<i>elev.</i>	2	56
at Hensbarrow	-	-	-	<i>elev.</i>	23	57
at the Deadman	-	-	-	<i>depr.</i>	11	39
at Maker Heights	-	-	-	<i>depr.</i>	10	30

On Bodmin Down.

The ground at Hensbarrow	-	-	-	<i>elev.</i>	24	3
at Lansallos	-	-	-	<i>depr.</i>	12	9

On Hensbarrow Beacon.

The ground at Carraton	-	-	<i>depr.</i> 0 36
at Lansallos	-	-	<i>depr.</i> 33 23
at the Deadman	-	-	<i>depr.</i> 42 8
at St. Agnes' Beacon	-	-	<i>depr.</i> 21 53
at Bodmin Down	-	-	<i>depr.</i> 31 21

At the Deadman.

The ground at Karnbonellis	-	-	<i>elev.</i> 7 51
at St. Agnes' Beacon	-	-	<i>elev.</i> 0 19
at Hensbarrow	-	-	<i>elev.</i> 33 30
at Lansallos	-	-	<i>elev.</i> 1 30

At St. Agnes' Beacon.

The ground at Karnminnis	-	-	<i>elev.</i> 2 11
at Karnbonellis	-	-	<i>elev.</i> 12 45
at Hensbarrow	-	-	<i>elev.</i> 8 8
at the Deadman	-	-	<i>depr.</i> 14 15

On Karnbonellis.

The ground at St. Agnes' Beacon	-	-	<i>depr.</i> 19 51
at Karnminnis	-	-	<i>depr.</i> 5 51
at St. Buryan	-	-	<i>depr.</i> 20 56
at the Deadman	-	-	<i>depr.</i> 22 18

On Karnminnis.

The ground at St Buryan	-	-	<i>depr.</i> 32 9
at Karnbonellis	-	-	<i>depr.</i> 4 30
at St. Agnes' Beacon	-	-	<i>depr.</i> 14 12
at Pertinney Hill	-	-	<i>depr.</i> 9 14

At St. Buryan.

The ground at Karnminnis	-	-	elev. 24 32
at Karnbonellis	-	-	elev. 6 50

N. B. 6'' must be subtracted from the elevations, and added to the depressions, on account of the error in the parallelism of the line of collimation of the telescope, and the rod attached to its side, upon which the level is hung.

The axis of the telescope was about $5\frac{1}{2}$ feet from the ground at all the above stations.

ART. II. *Terrestrial Refractions.*

Between		Mean Refraction.
Maker and Kit Hill	-	$\frac{1}{7}$ of the contained arc.
Butterton and Kit Hill	-	$\frac{1}{8}$
Bindown and Lansallos	-	$\frac{1}{9}$
Nine Barrow Down and Black Down		$\frac{1}{10}$
Maker and Lansallos	-	$\frac{1}{10}$
Maker and the Bolt Head	-	$\frac{1}{10}$
Carraton Hill and Bindown	-	$\frac{1}{11}$
Karnbonellis and St Buryan		$\frac{1}{11}$
Maker and Bindown	-	$\frac{1}{12}$
Hensbarrow and the Deadman		$\frac{1}{12}$
St. Agnes' Beacon and the Deadman		$\frac{1}{12}$
St. Agnes' Beacon and Karnminnis		$\frac{1}{12}$
Dumpdon and Cawsand Beacon		$\frac{1}{13}$
Haldon and Cawsand Beacon		$\frac{1}{13}$
Kit Hill and Bindown	-	$\frac{1}{13}$
Carraton Hill and Hensbarrow		$\frac{1}{13}$

Between	Mean Refraction.
Lansallos and the Deadman -	$\frac{1}{13}$ of the contained arc.
Hensbarrow and St. Agnes' Beacon	$\frac{1}{13}$
Karbonellis and Karnminnis	$\frac{1}{14}$
Furland and Haldon - -	$\frac{1}{15}$
Butterton and Maker -	$\frac{1}{15}$
Butterton and Carraton Hill -	$\frac{1}{15}$
Maker and Carraton Hill -	$\frac{1}{15}$
Karbonellis and the Deadman	$\frac{1}{15}$
Karbonellis and St. Agnes' Beacon	$\frac{1}{15}$
Karnminnis and St. Buryan -	$\frac{1}{15}$
Hensbarrow and Bodmin Down	$\frac{1}{15}$
Lansallos and Bodmin -	$\frac{1}{15}$
Butterton and the Bolt Head -	$\frac{1}{15}$
Haldon and Charton Common	$\frac{1}{17}$
Rippin Tor and Cawsand Beacon	$\frac{1}{17}$
Black Down and Bull Barrow	$\frac{1}{18}$
Black Down and Pilsden Hill	$\frac{1}{18}$
Black Down and Charton Common	$\frac{1}{18}$
Lansallos and Hensbarrow -	$\frac{1}{18}$
Rippin Tor and Haldon -	$\frac{1}{19}$
Butterton and Furland -	$\frac{1}{19}$
Butterton and Rippin Tor -	$\frac{1}{21}$
Kit Hill and Carraton -	$\frac{1}{26}$
Pilsden Hill and Charton Common	$\frac{1}{28}$
Wingreen and Bull Barrow -	$\frac{1}{31}$
Lansallos and Carraton Hill -	$\frac{1}{34}$
Haldon and the Horizon of the Sea	$\frac{1}{11}$
Pilsden Hill and the Horizon of the Sea	$\frac{1}{11}$

The mean refractions were found by the following rules.

1. Reduce the elevations, or depressions, to the place of the axis of the telescope at each station, by adding, or subtracting, as the case may require, the angle at the place of observation, subtended by the vertical height between the object, whose elevation or depression was observed, and the axis of the telescope when at that station.*

2. Then, if both are depressions, subtract their sum from the contained arc, and half the remainder is the mean refraction.

3. If one is a depression and the other an elevation, take their difference. Then, if the depression is greater than the elevation, subtract the difference from the contained arc, and half the remainder is the mean refraction. But if the elevation is greatest, add the difference to the contained arc, and half the sum is the mean refraction.

ART. III. *Table containing the Heights of the Stations.*

Stations.			Heights.
Black Down	-	-	817 feet.
Charton Common	-		582
Little Haldon	-	-	818
Rippin Tor	-	-	1549
Furland	-	-	589

* For example. At the station on Hensbarrow, the ground at Bodmin Down was depressed $31' 27''$: the distance of those stations is 47337 feet; and the axis of the telescope was $5\frac{1}{2}$ feet above the ground: therefore, as $47337 : radius :: 5\frac{1}{2} \text{ feet} : \text{tang. } 24''$ the angle subtended by $5\frac{1}{2}$ feet at that distance; which, taken from $31' 27''$, gives $31' 3''$ for the depression of the place of the axis, instead of the ground. Again, at Bodmin Down, the ground at Hensbarrow was elevated $23' 57''$, to which adding $24''$, we have $24' 21''$ for the elevation of the place of the axis.

Stations.			Heights.
Butterton	-	-	1203 feet.
Maker Heights	-	-	402
Bull Barrow	-	-	927
Mintern Hill	-	-	891
Pilsden Hill	-	-	934
Dumpton	-	-	879
Cawsand Beacon	-	-	1792
Bolt Head	-	-	430
Kit Hill	-	-	1067
Bindown	-	-	658
Carraton Hill	-	-	1208
Lansallos	-	-	514
Bodmin Down	-	-	649
Hensbarrow Beacon	-	-	1026
The Deadman	-	-	379
St. Agnes' Beacon	-	-	599
Karboneellis	-	-	822
Karminnis	-	-	805
St. Buryan	-	-	415

ART. IV. *Remarks, &c. on the foregoing Table.*

The height of the ground at the station on Maker Heights, 402 feet, was determined by levelling down to low-water mark, near the passage house, below Mount Edgcumbe, on April 15, 1796. This, however, had been done several years before, by some officers of the Royal Engineers, who found it to be 401 feet. The height of the station near Dunnose, in the Isle of Wight, was also found by levelling; of which an account is given in the Philosophical Transactions for 1795. It therefore

may be considered as the least exceptionable mode of procedure, to deduce the intermediate heights from both those stations; for which purpose, the following comparison was made, exhibiting the height of the station on Charton Common, both ways.

	Feet.
Height of Nine Barrow Down (Phil. Trans. 1795, p. 582)	642
of Black Down	825
of Charton Common, <i>deduced from the height of</i>	
Dunnose	597
Height of Butterton	1201
of Rippin Tor	1545
of Furland	585
of Haldon	811
of Charton Common, <i>deduced from the height of</i>	
Maker	568
<i>from that of Dunnose</i>	597
<i>difference</i>	29

Those are the heights resulting directly from the observations. Now, supposing the difference, or the errors, to arise from the mean refractions, and those errors to be nearly the same between every two stations, we shall obtain the corrected heights in the following manner:

Nine Barrow Down	642	-	4	=	638	} as in the table.
Black Down	825	-	8	=	817	
Charton Common	597	-	15	=	582	
Butterton	1201	+	2	=	1203	
Rippin Tor	1545	+	4	=	1549	
Haldon	811	+	7	=	818	
Charton Common	568	+	14	=	582	

From those corrected heights, the others to the northward have been deduced. The heights to the westward of Butterton were determined from that of Maker. A mean of two or three results, by using $\frac{1}{15}$ of the contained arcs for refraction, is taken for the height of the station on Mintern Hill.

We subjoin the following elevations and depressions, for the use of those who may wish to examine the tables of heights and refractions, in the Philosophical Transactions for 1795. And here it is to be noted, that the axis of the telescope was always about $5\frac{1}{2}$ feet from the ground, unless the contrary is specified.

At Hanger Hill.

The ground at St. Ann's Hill	<i>depr.</i>	4	36
at Banstead	<i>elev.</i>	10	39

At St. Ann's Hill.

The ground at Bagshot Heath	<i>elev.</i>	11	23	} Instrument on the half scaffold: the axis of the telescope $20\frac{1}{2}$ feet high.
at Banstead	<i>elev.</i>	10	2	
at Hanger Hill	<i>depr.</i>	6	13	

The top of the flagstaff near
Hampton Poor House *depr.* 12 54

N. B. The flagstaff was about 41 feet high.

Near Hampton Poor House.

The ground at St. Ann's Hill *elev.* 8 17 Instrument on the whole scaffold: the axis about $36\frac{1}{2}$ feet high.

At Banstead.

The ground at Leith Hill	<i>elev.</i> 17 29	} On the half scaffold: the axis 20½ feet high.
at Shooter's Hill	<i>depr.</i> 11 7	
at St. Ann's Hill	<i>depr.</i> 22 9	
at Hanger Hill	<i>depr.</i> 22 35	
The top of the flagstaff at Botley Hill	- - <i>elev.</i> 18 0	} The staff about 29 feet high.

At Leith Hill.

The top of the flagstaff at Banstead	- <i>depr.</i> 25 57	} The staff about 27½ feet high.
of the flagst. at Botley Hill	<i>depr.</i> 8 46	
The ground at Hind Head	<i>depr.</i> 8 28	
at Crowborough Beacon	<i>depr.</i> 13 48	
at Ditchling Beacon	<i>depr.</i> 12 34	
at Chanctonbury Ring	<i>depr.</i> 13 10	
The top of Severndroog Castle	<i>depr.</i> 22 9	

N. B. The axis of the telescope when at Shooter's Hill, was about 29½ feet lower than the top of the Castle.

At Shooter's Hill.

The ground at Leith Hill	<i>elev.</i> 2 35
at Banstead	<i>elev.</i> 0 15

On Bagsbot Heath.

The ground at Hind Head	<i>elev.</i> 10 37
at St. Ann's Hill	<i>depr.</i> 12 30

At Hind Head.

The ground at Leith Hill	<i>depr.</i> 2 59
at Chanctonbury Ring	<i>depr.</i> 11 11

The ground at Rook's Hill	<i>depr.</i>	14	51
at Butser Hill	<i>depr.</i>	5	54
at Bagshot Heath	<i>depr.</i>	23	12
at Highclere	<i>depr.</i>	10	42

On Rook's Hill.

The ground at Hind Head	<i>elev.</i>	3	9
at Chanctonbury Ring	<i>depr.</i>	1	35
at Bow Hill	-	<i>depr.</i>	1 5
at Portsdown	-	<i>depr.</i>	16 22

At Butser Hill.

The ground at Highclere	<i>depr.</i>	9	29
at Hind Head	-	<i>depr.</i>	4 44
at Motteston Down	<i>depr.</i>	15	27

At Chanctonbury Ring.

The ground at Rook's Hill	<i>depr.</i>	10	46
at Hind Head	<i>depr.</i>	4	20
at Leith Hill	<i>depr.</i>	1	13
at Beachy Head	<i>depr.</i>	16	27

On the half scaffold: the axis $20\frac{1}{2}$ feet high.

At Dunnose.

The ground at Nine Barrow			
Down	-	-	<i>depr.</i> 15 37
at Dean Hill	<i>depr.</i>	17	24

On Ditchling Beacon.

The ground at Leith Hill *depr.* 4 36

On Fairlight Down.

The ground at Beachy Head *depr.* 7 45

at Brightling Windmill *depr.* 0 49 The ground at the Wind-
mill is about 4 feet higher than the axis of the telescope when
at Brightling.

On Brightling Down.

The ground at Fairlight Down *depr.* 7 56

at Beachy Head - *depr.* 8 44

at Crowborough Beacon *elev.* 3 54

At Crowborough Beacon.

The ground at Leith Hill *depr.* 4 8

at Brightling Windmill *depr.* 12 21

at Botley Hill - *depr.* 3 5

At Beachy Head.

The ground at Fairlight Down *depr.* 5 17

at Brightling Windmill *depr.* 1 48

at Chanctonbury Ring *depr.* 5 6

At Dean Hill.

The ground at Highclere *elev.* 0 46

at Beacon Hill *elev.* 4 47

at Wingreen *elev.* 5 5

at Dunnose *depr.* 7 56

At Beacon Hill.

The ground at Highclere	<i>depr.</i>	0' 15"
at Wingreen	<i>depr.</i>	0 34
at Dean Hill	<i>depr.</i>	13 13

At Highclere.

The ground at Hind Head	<i>depr.</i>	10 42
at Butser Hill	<i>depr.</i>	9 26
at Dean Hill	<i>depr.</i>	18 12
at Beacon Hill	<i>depr.</i>	13 15

On Nine Barrow Down.

The ground at Wingreen	<i>depr.</i>	1 20
at Dunnose	<i>depr.</i>	10 8

At Wingreen.

The ground at Beacon Hill	<i>depr.</i>	15 30
at Nine Barrow Down	<i>depr.</i>	17 40
at Dean Hill	-	<i>depr.</i> 20 19

SECTION FOURTH.

Containing the secondary Triangles, in which two Angles only have been observed. The first three intersected Places are intended for the small Instrument, on Account of their commanding Situations.

ART. I. Triangles.

Distance from Pilsden Hill to Charton Common 49016,3 Feet.

No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.
157	Pilsden - Charton Common <i>Golden Cape</i>	$\begin{array}{ccc} 44 & 6 & 35 \\ 36 & 59 & 6 \end{array}$	} Golden Cape { Feet. 29848 34533

Distance from Rippin Tor to Cawsand Beacon 64020,5 feet.

158	Rippin Tor - Cawsand Beacon <i>Great Haldon</i>	$\begin{array}{ccc} 88 & 2 & 28 \\ 41 & 22 & 57 \end{array}$	} Great Haldon - { 54789 82829
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Distance from the Bolt Head to Maker Heights 100591 feet.

159	Bolt Head - Maker Heights - <i>Hemmerdon Ball</i>	$\begin{array}{ccc} 29 & 15 & 10 \\ 54 & 20 & 9 \end{array}$	} Hemmerdon Ball { 82239 49464
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Distance from Bull Barrow to Wingreen 69058 feet.

160	Bull Barrow - Wingreen - <i>Noil Windmill</i>	$\begin{array}{ccc} 109 & 12 & 12 \\ 33 & 45 & 11 \end{array}$	} Noil Windmill { 63692 108255
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No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.	
161	Bull Barrow - Wingreen - <i>Noil Steeple</i>	$\begin{matrix} 22 & 4 & 38 \\ 111 & 10 & 59 \end{matrix}$	} Noil Steeple - {	Feet. 88420 35641
162	Bull Barrow - Wingreen - <i>Holy Trinity Steeple, Shaftesbury</i>	$\begin{matrix} 18 & 16 & 15 \\ 65 & 39 & 45 \end{matrix}$		} H. Trinity Steeple, Shaftesbury {
163	Bull Barrow - Wingreen - <i>St. Rumbold's Steeple, Shaftesbury</i>	$\begin{matrix} 15 & 45 & 15 \\ 46 & 55 & 34 \end{matrix}$	} St. Rumbold's Steeple, Shaftesbury {	
164	Bull Barrow - Wingreen - <i>Maypowder Steeple</i>	$\begin{matrix} 129 & 15 & 18 \\ 12 & 31 & 19 \end{matrix}$		} Maypowder Steeple {
165	Bull Barrow - Wingreen - <i>Stourhead House</i>	$\begin{matrix} 44 & 25 & 52 \\ 88 & 31 & 14 \end{matrix}$	} Stourhead House {	

Distance from Bull Barrow to Nine Barrow Down 106213 feet.

166	Bull Barrow - Nine Barrow Down <i>Mr. Frampton's Obelisk</i>	$\begin{matrix} 32 & 25 & 49 \\ 27 & 44 & 1 \end{matrix}$	} Mr. Frampton's Obelisk - {	56980 65662
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Bull Barrow from Mintern, or Revel's Hill, 42653.4 feet.

167	Bull Barrow - Mintern - <i>Mere Steeple</i>	$\begin{matrix} 97 & 43 & 51 \\ 58 & 1 & 14 \end{matrix}$	} Mere Steeple - {	88095 102912
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No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.	
168	Bull Barrow - Mintern - <i>Mrs. Thornhill's Obelisk</i>	68 44 5 47 19 3	Mrs. Thornhill's Obelisk -	Feet. 34902 44245
169	Bull Barrow - Mintern - <i>Odcombe Steeple</i>	20 37 56 143 59 47	Odcombe Steeple	{ 94589 56700
170	Bull Barrow - D Mintern - <i>Milborne-port Steeple</i>	52 41 35 77 1 36	Milborne-port Steeple -	{ 54038 44107
171	Bull Barrow - D Mintern - <i>Lord Poulett's, Warren House</i>	7 39 0 132 19 30	Warren House	{ 8829 49035

Distance from Black Down to Pilsden 79110,7 feet.

172	Black Down - Pilsden - <i>Portland Light-house</i>	143 32 28 16 12 4	Light-House -	{ 63749 135775
173	Black Down - Pilsden - <i>Naval-Signal-staff on Puncknoll</i>	32 55 8 13 35 5	Signal-staff at Puncknoll -	{ 25615 59266
174	Black Down - Pilsden - <i>House in Lambert's Castle</i>	9 2 48 62 47 53	Lambert's Castle	{ 74048 13091

No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.	
175	Black Down -	26° 6' 41"	} Lyme Cobb -	Fect.
	Pilsden - -			90349
	Lyme Cobb			39815

Distance from Pilsden to Mintern 78177 feet.

176	Pilsden - -	64° 47' 55"	} Glastonbury Tor	{	127174
	Mintern - -				117551
	Glastonbury Tor				

Distance from Pilsden to Charton Common 49016,3 feet.

177	Pilsden - -	40° 30' 43"	} Bridport Beacon	{	44332
	Charton Common				32616
	Bridport Beacon, a Sea-mark				

178	Pilsden - -	15° 44' 0"	} Barn on Sidmouth	{	39824
	Charton Common				15191
	Barn on the bigg land near Sidmouth				

Distance from Dumpdon to Pilsden 78459 feet.

179	Dumpdon - -	50° 52' 11"	} Signal-staff on	{	50832
	Pilsden - -				60876
	Naval-Signal-staff on Whitlands				

180	Dumpdon - -	93° 52' 54"	} Catherstone Lodge	{	64521
	Pilsden - -				104901
	Catherstone Lodge, Quantock Hills				

Distance from Charton Common to Dumpdon 58012,4 feet.

No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.	Feet.
181	Charton Common Dumpdon - <i>Lord Lisburne's Obelisk on Haldon</i>	$\begin{matrix} 61 & 11 & 23 \\ 91 & 51 & 33 \end{matrix}$	} Lord Lisburne's Obelisk	{ 127936 112161

Distance from Dumpdon to Cawsand Beacon 181334 feet.

182	Dumpdon - Cawsand Beacon <i>Sir J. de la Pole's Flagstaff, near Sbute House</i>	$\begin{matrix} 128 & 45 & 59 \\ 13 & 59 & 24 \end{matrix}$	} Sir J. de la Pole's Flagstaff	{ 72435 233619
183	Dumpdon - Cawsand Beacon <i>Honiton Steeple</i>	$\begin{matrix} 64 & 18 & 8 \\ 4 & 0 & 39 \end{matrix}$	} Honiton Steeple	{ 13650 175852
184	Dumpdon - Cawsand Beacon <i>St. Mary Ottery Steeple</i>	$\begin{matrix} 34 & 20 & 21 \\ 12 & 27 & 16 \end{matrix}$	} St. Mary Ottery Steeple -	{ 53653 140335

Distance from Little Haldon to Dumpdon 126831 feet.

185	Dumpdon - Little Haldon - <i>Funnel on Sir R. Palk's Tower, Haldon</i>	$\begin{matrix} 17 & 20 & 53 \\ 63 & 7 & 37 \end{matrix}$	} Sir R. Palk's Tower	{ 114716 38347
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Distance from Cawsand Beacon to Little Haldon 106508 feet.

186	Cawsand Beacon D Little Haldon - <i>North Bovey Steeple</i>	$\begin{matrix} 7 & 9 & 50 \\ 10 & 38 & 19 \end{matrix}$	} North Bovey Stee- ple - -	{ 64313 43444
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Distance from Little Haldon to Rippin Tor 55988,7 feet.

No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.	
187	Little Haldon - Rippin Tor - <i>Eastern Karn, or heap of stones, on the high ground near Moreton Hampstead</i>	$\begin{matrix} 34 & 8 & 22 \\ 66 & 14 & 23 \end{matrix}$	Eastern Karn, near Moreton Hampstead -	Feet. 52099 31944
188	Little Haldon - Rippin Tor - <i>Western Karn, near Moreton Hampstead</i>	$\begin{matrix} 37 & 24 & 5 \\ 69 & 24 & 30 \end{matrix}$	Western Karn near Moreton Hampstead -	54751 35525
189	Little Haldon - Rippin Tor - <i>Naval-Signal-staff at West Down Beacon</i>	$\begin{matrix} 154 & 35 & 29 \\ 11 & 28 & 37 \end{matrix}$	Naval-Signal-staff, West Down Beacon -	46268 99715
190	Little Haldon - Rippin Tor - <i>Mr. Woodley's Summer House</i>	$\begin{matrix} 5 & 43 & 59 \\ 81 & 44 & 20 \end{matrix}$	Summer House	55462 5598
191	Little Haldon Rippin Tor - <i>Naval-Signal-staff, Berry Head, Torbay</i>	$\begin{matrix} 99 & 46 & 2 \\ 42 & 35 & 24 \end{matrix}$	Signal-staff on Berry Head	62040 90345
192	Little Haldon Rippin Tor - <i>Brixen Steeple</i>	$\begin{matrix} 91 & 52 & 49 \\ 48 & 37 & 47 \end{matrix}$	Brixen Steeple	66070 87993

No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.	
193	Little Haldon Rippin Tor - <i>Ipplepen Steeple</i>	$67^{\circ} 8' 45''$ $44 56 5$	} Ipplepen Steeple {	Feet. 42675 55677
194	Little Haldon Rippin Tor - <i>Three Barrow Tor,</i> <i>Dartmoor</i>	$20 40 42$ $125 6 32$		} Three Barrow Tor {
Distance from Furland to Little Haldon 72776 feet.				
195	Furland - - Little Haldon <i>Brent Tor</i>	$71 56 33$ $51 46 15$	} Brent Tor - {	68727 83180
Distance from Butterton to Rippin Tor 62951 feet.				
196	Butterton - Rippin Tor - <i>Chudleigh Steeple</i>	$17 4 21$ $136 27 46$	} Chudleigh Steeple {	97302 41471
Distance from Butterton to Furland 80547,8 feet.				
197	Butterton - - Furland - - <i>Naval-Signal-Staff at</i> <i>Coleton, near Froward</i> <i>Point</i>	$3 37 11$ $140 5 47$	} Naval-Signal-staff at Coleton {	87314 8593
198	Butterton - Furland - - <i>Naval-Signal-staff,</i> <i>Start Point</i>	$39 15 6$ $78 26 47$		} Naval-Signal-staff, Start Point {

No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.
199	Butterton - Furland - - <i>Marlborough Steeple</i>	$\begin{matrix} 61 & 55 & 7 \\ 48 & 18 & 25 \end{matrix}$	Marlborough Steeple - - { Feet. 64099 75736
200	Butterton - Furland - - <i>Naval-Signal-staff, near the Bolt Head</i>	$\begin{matrix} 63 & 40 & 32 \\ 53 & 24 & 17 \end{matrix}$	Naval-Signal-staff on the Bolt Head { 72632 81084

Distance from Butterton to Maker Heights 75760,8 feet.

201	Butterton - Maker - - <i>Highest Part of the Mewstone</i>	$\begin{matrix} 18 & 0 & 46 \\ 50 & 17 & 40 \end{matrix}$	Mewstone - { 62728 25213
202	Butterton - Maker Heights <i>Cupola of the Royal Hospital, Plymouth</i>	$\begin{matrix} 6 & 11 & 21 \\ 44 & 49 & 37 \end{matrix}$	Cupola of the Royal Hospital { 68709 10508
203	Butterton - Maker Heights <i>St. John's Steeple</i>	$\begin{matrix} 8 & 58 & 35 \\ 122 & 49 & 11 \end{matrix}$	St. John's Steeple { 85401 15856
204	Butterton - Maker Heights <i>Saltash Steeple</i>	$\begin{matrix} 19 & 46 & 39 \\ 75 & 36 & 25 \end{matrix}$	Saltash Steeple { 73708 25749
205	Butterton - Maker Heights <i>Penlee Beacon</i>	$\begin{matrix} 5 & 36 & 20 \\ 96 & 23 & 55 \end{matrix}$	Penlee Beacon { 76972 7566

Distance from Butterton to Kit Hill 100969 feet.

No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.	
206	Butterton - - Kit Hill - - - <i>Plymstock Steeple</i>	$\begin{array}{ccc} 0 & ' & '' \\ 39 & 1 & 33 \\ 27 & 49 & 38 \end{array}$	} Plymstock Steeple {	Feet. 51259 69143
207	Butterton - - Kit Hill - - - <i>Statten Barn</i>	$\begin{array}{ccc} 48 & 3 & 55 \\ 35 & 25 & 31 \end{array}$		} Statten Barn {
208	Butterton - - Kit Hill - - - <i>Mount Batton</i>	$\begin{array}{ccc} 41 & 56 & 57 \\ 37 & 8 & 33 \end{array}$	} Mount Batton {	
209	Butterton - - - Kit Hill - - - <i>Flagstaff in Plymouth Garrison</i>	$\begin{array}{ccc} 39 & 56 & 31 \\ 34 & 45 & 12 \end{array}$		} Flagstaff, Plymouth Garrison {
210	Butterton - - Kit Hill - - - <i>New Church Steeple at Plymouth</i>	$\begin{array}{ccc} 37 & 21 & 59 \\ 33 & 0 & 38 \end{array}$	} New Church Steeple - - {	
211	Butterton - - Kit Hill - - - <i>Old Church Steeple at Plymouth</i>	$\begin{array}{ccc} 37 & 45 & 52 \\ 34 & 3 & 52 \end{array}$		} Old Church Steeple - - {
212	Butterton - - Kit Hill - - - <i>West Chimney of the Governor's House, Plymouth Dock</i>	$\begin{array}{ccc} 37 & 5 & 33 \\ 39 & 58 & 36 \end{array}$	} Governor's House, Plymouth Dock {	

No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.
213	Butterton - Kit Hill - - <i>Flagstaff in the Fort on Mount Wise</i>	$\begin{matrix} 37 & 6 & 53 \\ 40 & 42 & 48 \end{matrix}$	} Flagstaff on Mount Wise - { $\begin{matrix} \text{Feet.} \\ 67374 \\ 62327 \end{matrix}$
214	Butterton - Kit Hill - - <i>Steeple of the Chapel, Plymouth Dock</i>	$\begin{matrix} 35 & 14 & 20 \\ 41 & 25 & 1 \end{matrix}$	} The Chapel, Ply- mouth Dock { $\begin{matrix} 68653 \\ 59874 \end{matrix}$
215	Butterton - Kit Hill - - <i>Flagstaff in St. Nicho- las' Island</i>	$\begin{matrix} 41 & 40 & 8 \\ 38 & 38 & 52 \end{matrix}$	} Flagstaff in St. Ni- cholas' Island { $\begin{matrix} 63970 \\ 68097 \end{matrix}$
216	Butterton - Kit Hill - - <i>Obelisk at Crimbill Passage</i>	$\begin{matrix} 38 & 40 & 39 \\ 42 & 48 & 20 \end{matrix}$	} Obelisk at Crim- hill Passage { $\begin{matrix} 69376 \\ 63803 \end{matrix}$
217	Butterton - Kit Hill - - <i>East Pinnacle on Mount Edgcumbe House</i>	$\begin{matrix} 40 & 29 & 28 \\ 42 & 49 & 3 \end{matrix}$	} Mount Edgcumbe House - { $\begin{matrix} 69096 \\ 66012 \end{matrix}$
218	Butterton - Kit Hill - - <i>Flagstaff on Maker Tower</i>	$\begin{matrix} 41 & 54 & 7 \\ 45 & 25 & 27 \end{matrix}$	} Maker Tower { $\begin{matrix} 72001 \\ 67507 \end{matrix}$
219	Butterton - Kit Hill - - <i>Naval-Signal-staff, near Maker Tower</i>	$\begin{matrix} 41 & 53 & 45 \\ 45 & 35 & 55 \end{matrix}$	} Naval-Signal-staff near Maker Tower { $\begin{matrix} 72207 \\ 67490 \end{matrix}$

No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.
220	Butterton - - Kit Hill - - - <i>Chestow Steeple</i>	$\begin{array}{ccc} 0 & ' & '' \\ 12 & 40 & 29 \\ 138 & 21 & 13 \end{array}$	Chestow Steeple $\left\{ \begin{array}{l} \text{Feet.} \\ 138522 \\ 45738 \end{array} \right.$

Distance from Butterton to Carraton Hill 131576 feet.

221	Butterton - Carraton Hill - <i>Stonehouse Steeple</i>	$\begin{array}{ccc} 40 & 34 & 1 \\ 23 & 29 & 2 \end{array}$	Stonehouse Steeple $\left\{ \begin{array}{l} 58310 \\ 95162 \end{array} \right.$
222	Butterton - Carraton Hill - <i>Obelisk at Puslinch</i>	$\begin{array}{ccc} 60 & 48 & 52 \\ 16 & 41 & 16 \end{array}$	Obelisk at Puslinch $\left\{ \begin{array}{l} 38700 \\ 117659 \end{array} \right.$
223	Butterton - Carraton Hill - <i>Rame Head</i>	$\begin{array}{ccc} 41 & 2 & 54 \\ 39 & 30 & 40 \end{array}$	Rame Head - $\left\{ \begin{array}{l} 84846 \\ 87594 \end{array} \right.$

Distance from Kit Hill to Maker Heights 67822.3 feet.

224	Kit Hill - - Maker Heights - <i>Brent Tor, near Lidford</i>	$\begin{array}{ccc} 116 & 24 & 26 \\ 24 & 3 & 10 \end{array}$	Brent Tor - $\left\{ \begin{array}{l} 43421 \\ 95419 \end{array} \right.$
225	Kit Hill - - Maker Heights - <i>Flag-staff of the Block House, near Dock</i>	$\begin{array}{ccc} 11 & 30 & 56 \\ 46 & 26 & 51 \end{array}$	Block House - $\left\{ \begin{array}{l} 57984 \\ 15972 \end{array} \right.$
226	Kit Hill - - Maker Heights - <i>Rame Steeple</i>	$\begin{array}{ccc} 4 & 3 & 42 \\ 141 & 4 & 23 \end{array}$	Rame Steeple - $\left\{ \begin{array}{l} 74547 \\ 8403 \end{array} \right.$

Distance from Carraton Hill to Maker Heights 82600,3 feet.

No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.	Feet.
227	Carraton Hill - Maker Heights - <i>Steeple of the Chapel in the Yard, Plymouth Dock</i>	$\begin{matrix} 0 & 7 & 28 & 15 \\ 64 & 48 & 50 \end{matrix}$	Dock-yard Chapel	$\begin{cases} 78468 \\ 11274 \end{cases}$
228	Carraton Hill - Maker Heights - <i>Windmill at Plymouth Dock</i>	$\begin{matrix} 7 & 34 & 6 \\ 71 & 29 & 35 \end{matrix}$	Windmill at Plymouth Dock	$\begin{cases} 79778 \\ 11080 \end{cases}$
229	Carraton Hill - Maker Heights - <i>Battery on Statten Heights</i>	$\begin{matrix} 7 & 31 & 7 \\ 133 & 32 & 55 \end{matrix}$	Statten Battery	$\begin{cases} 97488 \\ 17199 \end{cases}$

Distance from Kit Hill to Carraton Hill 33427 feet.

230	Kit Hill - - Carraton Hill - <i>St. Stephen's Steeple</i>	$\begin{matrix} 105 & 0 & 39 \\ 43 & 47 & 30 \end{matrix}$	St. Stephen's Steeple - -	$\begin{cases} 44659 \\ 62330 \end{cases}$
231	Kit Hill - - Carraton Hill - <i>St. Ive Steeple</i>	$\begin{matrix} 29 & 11 & 14 \\ 47 & 42 & 54 \end{matrix}$	St. Ive Steeple	$\begin{cases} 25390 \\ 16736 \end{cases}$
232	Kit Hill - - Carraton - <i>Callington Steeple</i>	$\begin{matrix} 42 & 31 & 4 \\ 10 & 20 & 54 \end{matrix}$	Callington Steeple	$\begin{cases} 7532 \\ 28336 \end{cases}$
233	Kit Hill - - Carraton Hill - <i>Linkinborn Steeple</i>	$\begin{matrix} 25 & 20 & 11 \\ 28 & 8 & 55 \end{matrix}$	Linkinhorn Steeple	$\begin{cases} 19621 \\ 17798 \end{cases}$

No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.	
234	Kit Hill - - D Carraton Hill - <i>St. Dominic Steeple</i>	$\begin{matrix} 121 & 48 & 29 \\ 9 & 59 & 38 \end{matrix}$	} St. Dominic Steeple {	Fect. 7776 38097
235	Kit Hill - - D Carraton Hill - <i>South Petherwin Steeple</i>	$\begin{matrix} 60 & 22 & 24 \\ 67 & 55 & 47 \end{matrix}$	} South Petherwin Steeple - {	39475 37027
236	Kit Hill - - Carraton Hill - <i>South Hill Steeple</i>	$\begin{matrix} 19 & 31 & 2 \\ 15 & 22 & 32 \end{matrix}$	} South Hill Steeple {	15493 19522
237	Kit Hill - - Carraton Hill - <i>Lord Mount Edgcombe's House, at Empercombe</i>	$\begin{matrix} 108 & 14 & 2 \\ 48 & 46 & 11 \end{matrix}$	} House at Empercombe - {	64348 81266
238	Kit Hill - - Carraton Hill - <i>Northern Sea-mark on the Hoe</i>	$\begin{matrix} 59 & 59 & 7 \\ 42 & 59 & 43 \end{matrix}$	} Sea-mark on the Hoe - - {	66387 87011

Distance from Kit Hill to Bindown 54902,7 feet.

239	Kit Hill - - Bindown - - <i>St. Cleer Steeple</i>	$\begin{matrix} 39 & 56 & 21 \\ 51 & 25 & 10 \end{matrix}$	} St. Cleer Steeple {	42931 35256

Distance from Carraton Hill to Bindown 42541.4 feet.

No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.
240	Carraton Hill - Bindown - - <i>The highest part of Brownwilly</i>	$\begin{matrix} 130 & 14 & 2 \\ 26 & 32 & 44 \end{matrix}$	Brownwilly - { Feet. 48221 82371
241	Carraton Hill - Bindown - - <i>Cheese Rings</i>	$\begin{matrix} 138 & 42 & 49 \\ 7 & 21 & 53 \end{matrix}$	Cheese Rings - { 9773 50300
242	Carraton Hill - Bindown - - <i>Liskeard Steeple</i>	$\begin{matrix} 18 & 2 & 57 \\ 17 & 6 & 59 \end{matrix}$	Liskeard Steeple { 21739 22885
243	Carraton Hill - Bindown - - <i>Duloe Steeple</i>	$\begin{matrix} 18 & 6 & 21 \\ 84 & 32 & 47 \end{matrix}$	Duloe Steeple - { 43403 13550
244	Carraton Hill - Bindown - - <i>Menbeniot Steeple</i>	$\begin{matrix} 9 & 16 & 26 \\ 14 & 32 & 34 \end{matrix}$	Menheniot Steeple { 21502 13806
245	Carraton Hill - Bindown - - <i>Landrake Steeple</i>	$\begin{matrix} 43 & 17 & 44 \\ 75 & 46 & 11 \end{matrix}$	Landrake Steeple { 47177 33376
246	Carraton Hill - Bindown - - <i>Naval-Signal-staff at Nealand, near Pol- parrow</i>	$\begin{matrix} 22 & 51 & 23 \\ 129 & 59 & 13 \end{matrix}$	Signal-staff at Nealand - { 36203 71413

Distance from Lansallos to Carraton Hill 68929,7 feet.

No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.	
247	Carraton Hill -	$\overset{\circ}{2}5 \overset{\prime}{4}1 \overset{\prime\prime}{5}7$	} Boconnock Steeple {	Feet.
	Lansallos - -			46079
	<i>Boconnock Steeple</i>			33495
248	Carraton Hill -	$24 \ 4 \ 10$ $41 \ 27 \ 47$	} Obelisk at Boconnock - {	50139
	Lansallos - -			30886
	<i>Obelisk at Boconnock, (Lord Camelford's)</i>			
249	Carraton Hill -	$41 \ 29 \ 10$ $94 \ 48 \ 32$	} Roach Rock - {	99410
	Lansallos - -			66086
	<i>Roach Rock</i>			
250	Carraton Hill -	$42 \ 1 \ 28$ $94 \ 41 \ 58$	} Roach Steeple {	100214
	Lansallos - -			67314
	<i>Roach Steeple</i>			

Distance from Lansallos to Hensbarrow Beacon 62044,8 feet.

251	Lansallos -	$21 \ 34 \ 34$ $46 \ 16 \ 45$	} Helmen Tor - {	48412
	Hensbarrow Beacon			24633
	<i>Helmen Tor</i>			
252	Lansallos -	$37 \ 8 \ 29$ $70 \ 7 \ 42$	} Summer House {	61105
	Hensbarrow Beacon			39231
	<i>Mr. Tremaine's Summer House</i>			
253	Lansallos -	$45 \ 34 \ 10$ $72 \ 3 \ 29$	} Gorran Steeple {	66624
	Hensbarrow Beacon			50008
	<i>Gorran Steeple</i>			

No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.
254	Lansallos - Hensbarrow Beacon <i>Naval-Signal-staff on the Deadman</i>	$\begin{matrix} 52 & 43 & 25 \\ 71 & 28 & 51 \end{matrix}$	Naval-Signal-staff at the Deadman { Fet. 71136 59696
255	Lansallos - - Hensbarrow Beacon <i>Gwineas Rocks</i>	$\begin{matrix} 51 & 21 & 9 \\ 60 & 17 & 27 \end{matrix}$	Gwineas Rocks, off Mevagissy { 57977 52133

Distance from Bodmin Down to Hensbarrow Beacon 47337,2 feet.

256	Bodmin Down Hensbarrow Beacon <i>Hendellion Steeple</i>	$\begin{matrix} 97 & 21 & 30 \\ 89 & 57 & 45 \end{matrix}$	Hendellion Steeple { 44851 69255
257	Bodmin Down Hensbarrow Beacon <i>The high Stone on St. Braeg Down</i>	$\begin{matrix} 48 & 38 & 46 \\ 55 & 1 & 58 \end{matrix}$	The high Stone on St. Braeg Down { 39924 36571
258	Bodmin Down Hensbarrow Beacon <i>St. Dennis Steeple</i>	$\begin{matrix} 13 & 28 & 31 \\ 120 & 37 & 11 \end{matrix}$	St. Dennis Steeple { 56722 15359
259 D	Bodmin Down Hensbarrow Beacon <i>Lansallos Steeple</i>	$\begin{matrix} 64 & 55 & 8 \\ 68 & 45 & 47 \end{matrix}$	Lansallos Steeple { 61011 59285

Deadman Head from Lansallos 70686,8 feet.

260 D	Deadman - Lansallos - - <i>St. Veep Steeple</i>	$\begin{matrix} 12 & 51 & 38 \\ 73 & 45 & 53 \end{matrix}$	St. Veep Steeple { 67986 15761
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Lansallos from Bodmin Down 61597,1 feet.

No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.	
261	Lansallos - -	26° 19' 35"	} Lanlivery Steeple {	Feet. 39552
D	Bodmin Down <i>Lanlivery Steeple</i>	33 51 19		31486
Hensbarrow Beacon from Deadman Head 59284,2 feet.				
262	Hensbarrow Beacon	30 50 7	} Gerrans Steeple {	83901
D	Deadman - <i>Gerrans Steeple</i>	106 31 21		44858
263	Hensbarrow Beacon	13 56 6	} St. Michael Car- hayes Steeple {	48309
D	Deadman - <i>St. Michael Carbayes Steeple</i>	43 10 53		17001
264	Hensbarrow Beacon	31 22 22	} St. Kivern Steeple {	136676
D	Deadman - <i>St. Kivern Steeple</i>	128 53 52		91426
265	Hensbarrow Beacon	29 6 51	} Signal-staff at Black Head {	146770
D	Deadman - <i>Naval-Signal-staff at Black Head</i>	133 59 31		99260
266	Hensbarrow Beacon	62 46 29	} Fowey Windmill {	45036
D	Deadman - <i>Windmill near Fowey</i>	45 59 37		55677
267	Hensbarrow Beacon	56 10 33	} Menabilly House {	35221
D	Deadman - <i>Menabilly House</i>	36 24 22		49300

No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.
268	Hensbarrow Beacon	6° 28' 23"	} Old Tower at Pol- ruan - {
	Deadman - <i>Old Tower at Polruan</i>	49 6 10	
269	Hensbarrow Beacon	30 52 0	} Signal-staff, St. Anthony's Head {
	Deadman - <i>Naval-Signal-staff at St. Anthony's Head</i>	116 42 13	
Distance from Hensbarrow Beacon to St. Agnes' Beacon 97084.8 feet.			
270	Hensbarrow Beacon	31 37 12	} St. Columb Minor { Steeple - {
	D St. Agnes' Beacon <i>St. Columb Minor Steeple</i>	28 56 16	
271	Hensbarrow Beacon	11 43 0	} Peranzabulo Stee- ple - - {
	D St. Agnes' Beacon <i>Peranzabulo Steeple</i>	31 9 39	
272	Hensbarrow Beacon	57 24 41	} St. Eval Steeple {
	St. Agnes' Beacon <i>St. Eval Steeple</i>	35 11 34	
273	Hensbarrow Beacon	15 2 26	} Cubert Steeple {
	St. Agnes' Beacon <i>Cubert Steeple</i>	30 37 20	
274	Hensbarrow Beacon	41 44 14	} Pendennis Castle {
	St. Agnes' Beacon <i>Flagstaff in Pendennis Castle</i>	72 36 24	

No.	Triangles.	Observed angles.	Distances of the stations from the interse.cted objects.	
275	Hensbarrow Beacon St. Agnes' Beacon <i>Windmill near St. Mawes</i>	$\begin{array}{ccc} 0 & 11 & 25 \\ 42 & 3 & 38 \\ 61 & & \end{array}$	} Windmill near St. Mawes -	$\left\{ \begin{array}{l} \text{Feet.} \\ 87286 \\ 66985 \end{array} \right.$
Distance from St. Agnes' Beacon to Karnminnis 84610,6 feet.				
276	St. Agnes' Beacon Karnminnis <i>Karnbre Castle</i>	$\begin{array}{ccc} 49 & 20 & 11 \\ 20 & 23 & 49 \end{array}$	} Karnbre Castle	$\left\{ \begin{array}{l} 31435 \\ 68417 \end{array} \right.$
277	St. Agnes' Beacon Karnminnis <i>Cupola of the Market House in Redruth</i>	$\begin{array}{ccc} 55 & 59 & 58 \\ 17 & 46 & 35 \end{array}$	} Cupola in Redruth	$\left\{ \begin{array}{l} 26903 \\ 79054 \end{array} \right.$
278	St. Agnes' Beacon Karnminnis <i>Camborn Steeple</i>	$\begin{array}{ccc} 30 & 57 & 7 \\ 21 & 45 & 40 \end{array}$	} Camborn Steeple	$\left\{ \begin{array}{l} 39427 \\ 54696 \end{array} \right.$
279	St. Agnes' Beacon Karnminnis <i>Illugan Steeple</i>	$\begin{array}{ccc} 31 & 12 & 56 \\ 10 & 49 & 6 \end{array}$	} Illugan Steeple	$\left\{ \begin{array}{l} 23718 \\ 65490 \end{array} \right.$
280	St. Agnes' Beacon Karnminnis <i>St. Paul Steeple</i>	$\begin{array}{ccc} 40 & 52 & 42 \\ 117 & 47 & 27 \end{array}$	} St. Paul Steeple	$\left\{ \begin{array}{l} 110564 \\ 81794 \end{array} \right.$
281	St. Agnes' Beacon Karnminnis <i>Lord de Dunstanville's House</i>	$\begin{array}{ccc} 20 & 40 & 33 \\ 10 & 47 & 12 \end{array}$	} Lord de Dunstanville's House	$\left\{ \begin{array}{l} 30339 \\ 57237 \end{array} \right.$

No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.	
282	St. Agnes' Beacon D Karnminnis <i>Gwinear Steeple</i>	$\begin{matrix} 21 & 40 & 24 \\ 40 & 30 & 41 \end{matrix}$	} Gwinear Steeple {	Feet. 62144 35330
283	St. Agnes' Beacon Karnminnis <i>Mr. Kneil's Obelisk,</i> <i>near St. Ives</i>	$\begin{matrix} 53 & 24 & 45 \\ 88 & 37 & 42 \end{matrix}$		} Mr. Kneil's Obelisk {
284	St. Agnes' Beacon Karnminnis <i>Highest of the Rocks</i> <i>called the Cow and Calf</i>	$\begin{matrix} 141 & 53 & 34 \\ 20 & 9 & 34 \end{matrix}$	} Cow and Calf Rocks - {	

Distance from St. Agnes' Beacon to Karnbonellis 45461.9 feet.

285	St. Agnes' Beacon Karnbonellis <i>St. Erme Steeple</i>	$\begin{matrix} 94 & 43 & 5 \\ 42 & 10 & 34 \end{matrix}$	} St. Erme Steeple {	44668 66303
286	St. Agnes' Beacon Karnbonellis <i>St. Allen Steeple</i>	$\begin{matrix} 98 & 13 & 52 \\ 35 & 41 & 11 \end{matrix}$		} St. Allen Steeple {
287	St. Agnes' Beacon Karnbonellis <i>Ludgvan Steeple</i>	$\begin{matrix} 44 & 12 & 31 \\ 105 & 49 & 41 \end{matrix}$	} Ludgvan Steeple {	

Distance from Karnminnis to Karnbonellis 71578.3 feet.

288	Karnminnis Karnbonellis <i>Windmill near the Li-</i> <i>zard</i>	$\begin{matrix} 41 & 26 & 59 \\ 95 & 31 & 22 \end{matrix}$	} Lizard Windmill {	104413 69440
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No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.	
289	Karnminnis Karnbonellis <i>Grade Steeple</i>	$\begin{array}{ccc} 40 & 7 & 0 \\ 100 & 25 & 15 \end{array}$	} Grade Steeple	} Feet. 110762 72566
290	Karnminnis Karnbonellis <i>Ruan Major Steeple</i>	$\begin{array}{ccc} 38 & 32 & 27 \\ 97 & 30 & 19 \end{array}$		
291	Karnminnis Karnbonellis <i>St. Hilary Steeple</i>	$\begin{array}{ccc} 39 & 52 & 32 \\ 25 & 24 & 25 \end{array}$	} St. Hilary Steeple	} 33808 50519
292	Karnminnis Karnbonellis <i>Castle Dennis (Mr. Rogers's Tower)</i>	$\begin{array}{ccc} 10 & 0 & 52 \\ 74 & 13 & 53 \end{array}$		

Distance from Karnbonellis to St. Buryan 99786 feet.

293	Karnbonellis St. Buryan <i>Madern Steeple</i>	$\begin{array}{ccc} 9 & 32 & 41 \\ 33 & 51 & 25 \end{array}$	} Madern Steeple	} 80908 24081
294 D	Karnbonellis St. Buryan <i>Perranuthno Steeple</i>	$\begin{array}{ccc} 60 & 38 & 57 \\ 49 & 18 & 46 \end{array}$		
295 D	Karnbonellis St. Buryan <i>Girnhove Steeple</i>	$\begin{array}{ccc} 76 & 57 & 1 \\ 50 & 25 & 43 \end{array}$	} Girnhove Steeple	} 46355 58583
296	Karnbonellis St. Buryan <i>Naval-Signal-staff, Park Loughs</i>	$\begin{array}{ccc} 60 & 25 & 48 \\ 40 & 43 & 1 \end{array}$		

Distance from Pertinney to Karnminnis 41407,7 feet.

No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.
297	Pertinney Karnminnis <i>St. Buryan Steeple</i>	$\begin{matrix} 116 & 12 & 46 \\ 13 & 40 & 7 \end{matrix}$	St. Buryan Steeple { Feet. 12751 48411

Distance from St. Buryan to Pertinney 12450,2 feet.

298	St. Buryan Pertinney <i>Chapel Karnbury</i>	$\begin{matrix} 23 & 28 & 57 \\ 58 & 34 & 54 \end{matrix}$	Chapel Karnbury { 10728 5009
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299	St. Buryan Pertinney <i>Naval-Signal-staff, St. Leven's Point</i>	$\begin{matrix} 75 & 36 & 7 \\ 67 & 31 & 4 \end{matrix}$	Signal-staff, St. Leven's Point { 20094 19169
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300	St. Buryan Pertinney <i>Sennen Steeple</i>	$\begin{matrix} 69 & 21 & 10 \\ 68 & 58 & 0 \end{matrix}$	Sennen Steeple { 17475 17520
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Distance from Sennen to Pertinney 20199,9 feet.

301	Sennen Pertinney <i>Stone near the Land's End</i>	$\begin{matrix} 106 & 43 & 44 \\ 7 & 15 & 12 \end{matrix}$	Stone near the Land's End { 2791 21173
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302	Sennen Pertinney <i>Longship's Light-house</i>	$\begin{matrix} 126 & 1 & 11 \\ 18 & 6 & 39 \end{matrix}$	Longship's Light- house { 10717 27883
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The above triangles, and those which follow in this section, are numbered in order from the secondary series, given in the Philosophical Transactions for 1795.

ART. II. *Triangles for ascertaining the Distances of the Eddystone Light-house, from the Flagstaff of Plymouth Garrison, and the Rame-head.*

The ball on the lantern of the Light-house was observed from the stations on Butterton, Kit Hill, and Carraton Hill; and as much uncertainty has heretofore existed, with respect to a knowledge of its true distance from any point in the neighbourhood of Plymouth, observations were made on various arcs of the circle of the instrument, at the two first stations.

The triangles are the following.

Distance from Butterton to Kit Hill 100969 feet.

No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.			
303	Butterton - - Kit Hill - - Eddystone Light-house	$66^{\circ} 46' 21''$ $64^{\circ} 27' 46''$	Eddystone Light-house - - <table style="display: inline-table; vertical-align: middle;"> <tr> <td style="text-align: right;">Fees.</td> </tr> <tr> <td style="text-align: right;">121159</td> </tr> <tr> <td style="text-align: right;">123399</td> </tr> </table>	Fees.	121159	123399
Fees.						
121159						
123399						

Distance from Butterton to Carraton Hill 131576 feet.

304	Butterton - - Carraton Hill - Eddystone Light-house	$60^{\circ} 5' 31''$ $55^{\circ} 52' 41''$	Eddystone Light-house - - <table style="display: inline-table; vertical-align: middle;"> <tr> <td style="text-align: right;">121158</td> </tr> <tr> <td style="text-align: right;">126863</td> </tr> </table>	121158	126863
121158					
126863					

With the distance of the Eddystone Light-house from Kit Hill, and also that of the Flagstaff in Plymouth garrison from the same station, we find the distance from the Light-house to the Flagstaff = 73061 feet;* the observed angle being $29^{\circ} 42' 34''$: and, computing with the *data* obtained from the last triangle, and the 223d,

* On referring to the late Mr. SMEATON'S Narrative of the Building of the Eddystone Light-house, it will be found, that, from a trigonometrical process, founded on two bases measured on the Hoe, among other deductions, he concluded the distance between the above objects was 73464 feet; being 403 greater than the distance found by the above computation.

with the observed angle at Carraton Hill = $16^{\circ} 22' 1''$, we get 49435 feet for the distance of the Eddystone Light-house from the building on Rame-head. It may be proper to observe, that the Eddystone Light-house is nearer to the Rame-head than to any other point on the coast.

ART. III. *Triangles for ascertaining the Situations of the Lizard Light-houses; and the Lizard Point.*

Distance from Karnbonellis to Pertinney 101474 feet.

No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.
305	Karnbonellis - Pertinney - - <i>Eastern Light-house</i>	$78^{\circ} 49' 28''$ $42^{\circ} 56' 51''$	Eastern Light-house - - { Feet. 81323 117097
306	Karnbonellis - Pertinney - - <i>Western Light-house</i>	$78^{\circ} 40' 5''$ $43^{\circ} 0' 53''$	Western Light-house - - { 81348 116921
307	Karnbonellis - Pertinney - - <i>Naval-Signal-staff</i>	$78^{\circ} 8' 57''$ $42^{\circ} 28' 45''$	Signal-staff - { 79635 115408

Distance from Karnbonellis to St. Buryan 99786 feet.

308	Karnbonellis - St. Buryan - - <i>Naval-Signal-staff</i>	$71^{\circ} 7' 19''$ $45^{\circ} 30' 56''$	Signal-staff - { 79645 105873
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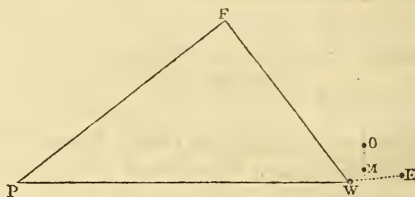
From the two last triangles we obtain 79640 feet for the mean distance between the Lizard Signal-staff and the station on Karnbonellis. Computing with this distance, and also that from the Western Light-house to the same station, with the observed angle $0^{\circ} 31' 8''$, we get 1857 feet for the distance between those objects.

For the purpose of ascertaining the situation of the Lizard Point, two angles in the following triangle were observed with a sextant, *viz.*

Naval-Signal-staff	-	77 ^o 4 [']
Western Light-house	-	60 50

Lizard Point

These, with the computed distance from the Signal-staff to the Light-house, give the distance of the Lizard Point from the {Signal-staff 2419 } feet. Hence, the distance of the point from the station on Karnbonellis is 81085 feet, the angle at that station, between the Lizard Point and Western Light-house, being $1^{\circ} 53' 47''$. With respect to the means by which the situation of the spot, on which Mr. BRADLEY erected his observatory in 1769, may hereafter be determined, it will be readily understood from the following diagram; where E is the Ea tern Light-house, W the Western Light-house, F the Signal-staff, P the Lizard Point, and O the place of the Observatory. The distance between the spot O, and M,* the place where his meridian mark was fixed, we measured and found = 800 feet; M being 24 feet north of the line joining the centres of the Light-houses.



* The person spoken of in Sect. 1. Art. 3. as having the care of the Light-houses, pointed out this spot.

ART. IV. *Triangles for finding the Distances of the Day-Mark, St. Agnes' Light-house, and other Objects in the Scilly Isles, from particular Stations in the West of Cornwall.*

Observations made at Karnminnis.

Between				Mean.
The station at St. Buryan and the Day-Mark	39	3	$22\frac{3}{4}$	"
			$22\frac{3}{4}$	} 23
			$23\frac{3}{4}$	

At St. Buryan.

Karnminnis and the Day-Mark	-	129	52	22	} 22
				$22\frac{1}{4}$	
Pertinney and St. Agnes' Light-house	-	83	59	$51\frac{3}{4}$	} 51
				50	
Flagstaff of the fort in St. Mary's and Karnminnis	-	134	39	$45\frac{3}{4}$	} $45\frac{1}{2}$
				45	
Windmill in St. Mary's and Pertinney	-	84	23	$53\frac{1}{2}$	} $53\frac{1}{4}$
				53	

At Pertinney.

St. Agnes' Light-house and Karnminnis		92	6	20	} $21\frac{3}{4}$
				$21\frac{1}{4}$	
				$21\frac{1}{2}$	
				$23\frac{1}{2}$	
Day-Mark and Karnminnis	-	148	11	$8\frac{1}{2}$	} $9\frac{1}{4}$
				$10\frac{1}{4}$	
Flagstaff in St. Mary's and St. Buryan		93	47	18	
Windmill in St. Mary's and St. Buryan		92	26	33	

At Sennen.

Day-Mark and Pertinney	-	145	20	$8\frac{1}{2}$	} $9\frac{1}{4}$
				10	
St. Agnes' Light-house and Pertinney	-	152	43	24	} $24\frac{1}{4}$
				$24\frac{1}{2}$	

From those observations, result the following triangles, when the necessary corrections are applied for reducing the observed angles to those formed by the chords, *viz.*

Distance from Karnminnis to St. Buryan 47786,7 feet.

No.	Triangles.	Observed angles cor.	Distances of the stations from the intersected objects.
309	Karnminnis - - St. Buryan - - Day-Mark	$\begin{array}{ccc} 39 & 3 & 24 \\ 129 & 52 & 19 \end{array}$	Day-Mark - $\left\{ \begin{array}{l} \text{Feet.} \\ 190985 \\ 156796 \end{array} \right.$

Distance from Karnminnis to Pertinney 41407,7 feet.

310	Karnminnis - - Pertinney - - Day-Mark	$\begin{array}{ccc} 25 & 15 & 8 \\ 148 & 11 & 5 \end{array}$	Day-Mark - $\left\{ \begin{array}{l} 190989 \\ 154551 \end{array} \right.$
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Distance from Sennen to Pertinney 20199,9 feet.

311	Sennen - - - Pertinney - - - Day-Mark	$\begin{array}{ccc} 145 & 20 & 7 \\ 30 & 24 & 7 \end{array}$	Day-Mark - $\left\{ \begin{array}{l} 137526 \\ 154568 \end{array} \right.$
312	Sennen - - - Pertinney - - - St. Agnes' or the Scilly Light-house	$\begin{array}{ccc} 152 & 43 & 20 \\ 24 & 21 & 55 \end{array}$	St. Agnes' Light-house - $\left\{ \begin{array}{l} 164010 \\ 182199 \end{array} \right.$

Distance from St. Buryan to Pertinney 12450,2 feet.

313	St. Buryan - - - Pertinney - - - St. Agnes' Light-house	$\begin{array}{ccc} 83 & 59 & 51 \\ 92 & 6 & 22 \end{array}$	St. Agnes' Light-house - $\left\{ \begin{array}{l} 183096 \\ 182215 \end{array} \right.$
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No.	Triangles.	Observed angles cor.	Distances of the stations from the intersected objects.
314	St. Buryan - -	83° 24' 53"	} Windmill in St. Mary's
	Pertinney - -	92 26 33	
	<i>Windmill in St. Mary's</i>		
			Feet. 172183 171203
315	St. Buryan - -	82 8 18	} Flagstaff in St. Mary's
	Pertinney - -	93 47 18	
	<i>Flagstaff of the fort in St. Mary's</i>		
			174890 173626

The distance from the Day-Mark to Karnminnis, as obtained from the 309th triangle, is 190985 feet, and by the 310th, 190989 feet, which differs only 4 feet from the former; and by the 310th and 311th triangles, the difference of the distances from the same object, to the station on Pertinney, is 17 feet; which, allowing for the shortness of the bases, must be considered as trifling. We may presume, therefore, that had not the Day-Mark been seen from Karnminnis, but from Sennen and Pertinney alone, the observations from which the angles of the 311th triangle are derived, would have afforded the means of computing the distance with sufficient precision. In like manner the 312th and 313th triangles seem to prove, that the observations made to St. Agnes' Light-house were sufficiently accurate, as there is a difference only of 16 feet between the distances of the Light-house from Pertinney. The ball on the top of the Light-house was the object always observed; and the Day-Mark being pyramidal, we had the means of making the observations at the different stations to the same point of this building.

ART. v. *Of the Distances of the Objects in the Scilly Isles, (intersected from the Stations in the West of Cornwall) from Sennen Steeple; the Stone near the Land's End; and the Longship's Light-house.*

As the observations made to the Day Mark, and St. Agnes' Light-house, may be supposed sufficiently accurate; and the ball on the top of the Longship's Light-house was also observed under favourable circumstances, it will be proper to apply the corrections to the horizontal angles, in order to obtain those formed by the chords. Taking, therefore, Pertinney as the angular point, and computing with the following *data, viz.*

Station on Pertinney from	-	<table style="border-collapse: collapse; margin: 0;"> <tr> <td style="padding: 2px 5px;">Day-Mark</td> <td style="padding: 2px 5px;">-</td> <td style="padding: 2px 5px;">-</td> <td style="padding: 2px 5px;">=</td> <td style="padding: 2px 5px;">154551</td> <td rowspan="3" style="border-left: 1px solid black; padding-left: 5px; vertical-align: middle;">} Feet. And</td> </tr> <tr> <td style="padding: 2px 5px;">St. Agnes' Light-house</td> <td style="padding: 2px 5px;">-</td> <td style="padding: 2px 5px;">-</td> <td style="padding: 2px 5px;">=</td> <td style="padding: 2px 5px;">182207</td> </tr> <tr> <td style="padding: 2px 5px;">Longship's Light-house</td> <td style="padding: 2px 5px;">-</td> <td style="padding: 2px 5px;">-</td> <td style="padding: 2px 5px;">=</td> <td style="padding: 2px 5px;">27883</td> </tr> </table>	Day-Mark	-	-	=	154551	} Feet. And	St. Agnes' Light-house	-	-	=	182207	Longship's Light-house	-	-	=	27883
Day-Mark	-	-	=	154551	} Feet. And													
St. Agnes' Light-house	-	-	=	182207														
Longship's Light-house	-	-	=	27883														
the angle at Pertinney, augmented for calculation, between the Longship's Light-house and																		
the Longship's Light-house from	-																	

Calculating also, with the distances of the two other objects in the Scilly Isles, and likewise those of Sennen Steeple, and the Stone near the Land's End from Pertinney, with the included angles at the same station, we get

		Feet.		Miles.				
Sennen Steeple from -	{	Day Mark	-	-	=	139521	=	26,43
		St. Agnes' Light-house	-	-	=	166255	=	31,49
		Flagstaff in St. Mary's	-	-	=	157912	=	29,95
		Windmill in St. Mary's	-	-	=	155299	=	29,41
Stone near the Land's End from -	{	Day Mark	-	-	=	135343	=	25,63
		St. Agnes' Light-house	-	-	=	162100	=	30,7
		Flagstaff in St. Mary's	-	-	=	153744	=	29,11
		Windmill in St. Mary's	-	-	=	151138	=	28,63

Of the Scilly Isles, Menawthen is the nearest to the Land's End, being about $1\frac{2}{16}$ miles eastward of the Day-Mark; and the cluster of rocks, called the Bishop and his Clerks, the most remote, being $3\frac{1}{3}$ miles west of St. Agnes' Light-house. Combining, therefore, the above particulars with those distances, we may conclude, that the nearest part of the Scilly Isles is about 24.7 miles from the Land's End, and the farthest nearly 34.

PART SECOND.

SECTION I.

Account of a Trigonometrical Survey carried on in Kent, in the Years 1795, and 1796, with the small circular Instrument.

ARTICLE I. *Particulars respecting the Instrument.*

The instrument used in this survey was announced in the Philosophical Transactions for 1795, p. 590. It was made by Mr. RAMSDEN; and is about half the size of his large theodolite, or circular instrument, with which we take the horizontal angles, but nearly similar to it in all its parts; consequently a very brief description will be sufficient.

The most material variations in the construction are,

1. The levelling or feet screws. These are below that horizontal movement which serves to direct the lower telescope to any particular object. By this position of the screws, the horizontal circle being once made level, the whole instrument may be moved round without disturbing its horizontality; the levelling screws remaining stationary during that operation,

which cannot be done in the large instrument, because the screws are carried round with it.

2. The diameter of the horizontal circle being only half that of the larger one, it follows, that the space between any two dots on the limb, gives double the number of minutes that are contained in the same space on the greater circle: on this account, each revolution in the micrometer screw in the microscope answers to $2'$; and the circle on the microscopic micrometer being divided into 60 parts, each division becomes equal to $2''$, but for the conveniency of notation, they are numbered at every 5th, with 10, 20, &c. to 50, the 60th being marked 1, to denote $1'$: the number of seconds then commencing as before, the whole revolution becomes $2'$. The revolutions are counted by means of notches on one side of the field in the microscope, in the same manner as in those of the large instrument.

3. This instrument not being intended for determining the direction of the meridian, a vertical semicircle for directing the telescope to the pole star became unnecessary; yet some apparatus was required, whereby small elevations or depressions from the horizon might be ascertained with a tolerable degree of precision. For this purpose, a moveable index, of about four inches long, is made to turn on the horizontal axis of the upper telescope, and so constructed, that by means of a finger screw, it can be fixed firmly in any position. The lower end of this index is furnished with a steel micrometer screw, having a circle on its head, divided into 100 parts, for shewing the fractional parts of a revolution, while other divisions, on a chamfered edge of the index which marks the fractional parts, give the number of revolutions made by the micrometer screw.

The method of finding the value of a revolution of the micrometer head in parts of a degree, &c. was as follows :

A rod, 14 or 16 feet long, was placed horizontally about three quarters of a mile off, and the angle subtended by its ends measured with the instrument in the usual way : the rod was then set up perpendicular at the same place, and the cross wires in the telescope directed to one of its extremities : the telescope was then moved in the vertical plane, by means of the micrometer screw, till the cross wires coincided with the other extremity. In this manner, by counting the number of revolutions, &c. necessary to move the telescope from one position to the other, an angle was measured vertically with the micrometer screw, equal to the former horizontal angle. From repeated trials, the value of a revolution was found equal to $10' 27''$.

This instrument, on account of its portable size, may very readily be taken to the tops of steeples, towers, &c. and is, therefore, extremely well adapted to the uses for which it was intended.

ART. II. *Situations of the Stations on which Observations were made with the small circular Instrument, in the Summer of the Year 1795.*

Folkstone Turnpike, the station made use of by General Roy in 1787.

Hawkinge, about three quarters of a mile from Folkstone Turnpike. This station was chosen for the purpose of having a view of the Belvidere in Waldershare Park, which cannot be seen from the station of 1787.

Dover Castle.

Paddlesworth; about 400 feet from the station of 1787. This new spot was selected, because *Hardres Steeple* is not visible from the old station.

Waldersbare; on the *Belvidere* in the Earl of Guilford's Park.

On *Ringswold Steeple.*

On a sand hill near the sea shore, between Deal and Ramsgate: this station is denominated *Shore.*

Near *Mount Pleasant House*, Isle of Thanet.

On a rising ground near *Wingham.*

On *Chislet Steeple.*

In *Beverley Park*, near Canterbury.

On Upper *Hardres Steeple.*

ART. III. *Triangles for determining the Distances of the Stations.*

As the station on the Keep of Dover Castle, in 1787, was directly over the steps of the Turret, a new point was chosen about $6\frac{1}{2}$ feet from the former, where the instrument could stand conveniently: this new point is about 2,8 feet farther from Folkstone Turnpike, and 1 foot farther from Paddlesworth, than the point marking the old station.

From General Roy's Account of the Trigonometrical Survey in 1787, we have

Dover Castle from Folkstone Turnpike	31554,6	}	feet.
from Paddlesworth	42561,2		

Now, augmenting those distances in the proportion of 141747 to 141753 (see *Phil. Trans.* Vol. LXXX, p. 595, and the Vol.

for 1795, p. 508), we get 31556, and 42563 feet; to which adding 2,8, and 1, respectively, we have

The new point on Dover Castle from Folkstone

Turnpike	-	-	-	-	-	-	}	feet.
						from Paddlesworth		42564

In order to obtain the distance between Waldershare and Dover Castle from those new sides, or distances, the three angles of the following triangle were very carefully taken.

1	{	Dover Castle	-	$3^{\circ} 49' 16''$	$3^{\circ} 49' 15''$	}	for computation.
		Folkstone Turnpike	$36^{\circ} 6' 31''$	$36^{\circ} 6' 30''$			
		Hawkinge	-	$140^{\circ} 4' 16''$	$140^{\circ} 4' 15''$		

The third angles of the two next triangles were not observed :

2	{	Hawkinge	-	-	-	$44^{\circ} 23' 30''$
		Dover Castle	-	-	-	$73^{\circ} 53' 44''$
		<i>Waldersbare</i>	-	-	-	$61^{\circ} 42' 46''$
3	{	Dover Castle	-	-	-	$62^{\circ} 24' 7''$
		Paddlesworth (the station of 1787)	-	-	-	$32^{\circ} 36' 9''$
		<i>Waldersbare</i>	-	-	-	$84^{\circ} 59' 44''$

By the two first triangles, Dover Feet.
 Castle from Waldershare 23019,4 } 23020,5 mean dis-
 From the latter - - 23021,5 } tance.

And *Hawkinge* from { Dover Castle 28976
 { Waldershare 31616

N. B. The angles at the stations, or objects, denoted in *italics*, are supplemental, or were not observed. And it is also to be remarked, that whenever Paddlesworth is mentioned hereafter, the *new station* is to be understood.

No.	Names of stations.	Observed angles.	Distances.	
4	Waldershare Paddlesworth Dover -	$\begin{array}{r} 85^{\circ} \quad 2' \quad 25'' \\ 32 \quad 53 \quad 10 \\ 62 \quad 4 \quad 25 \end{array}$	Paddlesw. { from { Dover Waldershare	Feet. 42239 37460
5	Waldershare Paddlesworth Hardres	$\begin{array}{r} 57 \quad 1 \quad 15 \\ 69 \quad 21 \quad 59 \\ 53 \quad 36 \quad 46 \end{array}$	Hardres { { Waldershare Paddlesworth	43548 39035
6	Dover Waldershare Ringswold	$\begin{array}{r} 66 \quad 46 \quad 45 \\ 57 \quad 57 \quad 24 \\ 55 \quad 15 \quad 51 \\ \hline 180 \quad 0 \quad 0 \end{array}$	Ringswold { { Dover Waldershare	23745 25743
7	Waldershare Ringswold Shore -	$\begin{array}{r} 45 \quad 43 \quad 8 \\ 97 \quad 38 \quad 32 \\ 36 \quad 38 \quad 20 \end{array}$	Shore { { Waldershare Ringswold	42755 30883
8	Mount Pleasant Shore - Waldersbare	$\begin{array}{r} 40 \quad 53 \quad 17 \\ 111 \quad 8 \quad 27 \\ 27 \quad 58 \quad 16 \end{array}$	Mt. Pleasant { { Shore Waldershare	30635 60920
9	Mount Pleasant Chislet - Wingham	$\begin{array}{r} 38 \quad 32 \quad 17 \\ 79 \quad 25 \quad 36 - 35 \\ 62 \quad 2 \quad 8 \\ \hline 180 \quad 0 \quad 1 \end{array}$	Chislet { { Mount Pleasant Wingham	30062 21206
10	Hardres Wingham Waldersbare	$\begin{array}{r} 52 \quad 46 \quad 14 \\ 69 \quad 29 \quad 1 \\ 57 \quad 44 \quad 55 \end{array}$	Hardres from Wingham	39322
11	Wingham Beverley Park Hardres	$\begin{array}{r} 50 \quad 4 \quad 0 \\ 75 \quad 0 \quad 0 \\ 54 \quad 56 \quad 4 - 0 \\ \hline 180 \quad 0 \quad 4 \end{array}$	Beverley Park { { Wingham Hardres	33320 31215

ART. IV. Secondary Triangles.

No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.	
12	Paddlesworth - Waldershare - <i>Barbam Windmill</i>	$\begin{matrix} 38 & 28 & 36 \\ 70 & 22 & 24 \end{matrix}$	} Windmill -	{ $\begin{matrix} \text{Feet.} \\ 37283 \\ 24628 \end{matrix}$
13	Dover - - Waldershare <i>St. Radigund's Abbey</i>	$\begin{matrix} 51 & 40 & 11 \\ 44 & 23 & 40 \end{matrix}$	} St. Radigund's Ab- bey - -	{ $\begin{matrix} 16196 \\ 18160 \end{matrix}$
14	Dover - - Waldershare - <i>Hougham Steeple</i>	$\begin{matrix} 75 & 15 & 45 \\ 40 & 31 & 40 \end{matrix}$	} Hougham Steeple	{ $\begin{matrix} 16614 \\ 24726 \end{matrix}$
15	Dover - - Waldershare - <i>Gunston Steeple</i>	$\begin{matrix} 32 & 41 & 51 \\ 17 & 46 & 31 \end{matrix}$	} Gunston Steeple	{ $\begin{matrix} 9111 \\ 16123 \end{matrix}$
16	Dover - - Waldershare - <i>St. Margaret's Steeple</i>	$\begin{matrix} 88 & 19 & 36 \\ 32 & 34 & 23 \end{matrix}$	} St. Margaret's Steeple -	{ $\begin{matrix} 14444 \\ 26817 \end{matrix}$
17	Hawkinge - Waldershare - <i>Elham Windmill</i>	$\begin{matrix} 84 & 50 & 30 \\ 15 & 3 & 14 \end{matrix}$	} Elham Windmill	{ $\begin{matrix} 8335 \\ 31963 \end{matrix}$
18	Dover - - Ringswold - <i>South Foreland Light- house</i>	$\begin{matrix} 39 & 48 & 39 \\ 28 & 8 & 7 \end{matrix}$	} South Foreland Light-house	{ $\begin{matrix} 12081 \\ 16403 \end{matrix}$
19	Waldershare - Ringswold - <i>Upper Deal Windmill</i>	$\begin{matrix} 17 & 10 & 7 \\ 102 & 11 & 7 \end{matrix}$	} Upper Deal Wind- mill - -	{ $\begin{matrix} 28870 \\ 8718 \end{matrix}$

No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.	
20	Waldershare -	22 20 10	} Upper Deal Chapel {	Fet. 30160 11663
	Ringswold -	100 38 27		
	<i>Upper Deal Chapel</i>			
21	Waldershare -	19 1 31	} Lower Deal Wind- mill - {	31226 10857
	Ringswold -	110 21 19		
	<i>Lower Deal Windmill</i>			
22	Waldershare -	19 28 27	} Deal Castle {	34689 13498
	Ringswold -	121 2 45		
	<i>Deal Castle</i>			
23	Waldershare -	42 26 26	} Norbourn Wind- mill - {	22102 17648
	Ringswold -	57 41 19		
	<i>Norbourn Windmill</i>			
24	Waldershare -	9 19 40	} Watch-house {	31317 7238
	Ringswold -	135 28 3		
	<i>Watch-house near the Sea shore</i>			
25	Waldershare -	29 45 47	} Sandown Castle {	38185 20351
	Ringswold -	111 20 13		
	<i>Sandown Castle</i>			
26	Waldershare -	12 29 13	} Walmer Steeple {	29491 7069
	Ringswold -	115 33 51		
	<i>Walmer Steeple</i>			
27	Waldershare -	15 35 53	} Ripple Steeple {	24209 6947
	Ringswold -	69 33 23		
	<i>Ripple Steeple</i>			

No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.	
28	Waldershare Ringswold - <i>Waldersbare Steeple</i>	$\begin{matrix} 20^{\circ} & 45' & 23'' \\ 5 & 35 & 50 \end{matrix}$	Waldershare Stee- ple -	$\left\{ \begin{array}{l} \text{Feet.} \\ 5656 \\ 20552 \end{array} \right.$
29	Waldershare Shore - - <i>Eastry Steeple</i>	$\begin{matrix} 16 & 23 & 49 \\ 21 & 57 & 46 \end{matrix}$	Eastry Steeple	$\left\{ \begin{array}{l} 25766 \\ 19448 \end{array} \right.$
30	Waldershare Shore - - <i>Ash Steeple</i>	$\begin{matrix} 35 & 10 & 6 \\ 56 & 41 & 26 \end{matrix}$	Ash Steeple -	$\left\{ \begin{array}{l} 35750 \\ 24639 \end{array} \right.$
31	Waldershare Shore - - <i>Minster Steeple</i>	$\begin{matrix} 28 & 29 & 39 \\ 103 & 15 & 30 \end{matrix}$	Minster Steeple	$\left\{ \begin{array}{l} 55782 \\ 27341 \end{array} \right.$
32	Waldershare - Shore - - <i>Woard Steeple</i>	$\begin{matrix} 5 & 43 & 2 \\ 19 & 37 & 24 \end{matrix}$	Woard Steeple	$\left\{ \begin{array}{l} 33548 \\ 9951 \end{array} \right.$
33	Waldershare - Shore - - <i>Sandwich, highest Stee- ple</i>	$\begin{matrix} 13 & 35 & 31 \\ 59 & 30 & 36 \end{matrix}$	Sandwich Steeple	$\left\{ \begin{array}{l} 38505 \\ 10501 \end{array} \right.$
34	Ringswold - Shore - - <i>Mongeham Steeple</i>	$\begin{matrix} 24 & 46 & 49 \\ 13 & 3 & 56 \end{matrix}$	Mongeham Steeple	$\left\{ \begin{array}{l} 11379 \\ 21098 \end{array} \right.$
35	Ringswold - Shore - - <i>Norbourn Steeple</i>	$\begin{matrix} 35 & 9 & 0 \\ 25 & 59 & 2 \end{matrix}$	Norbourn Steeple	$\left\{ \begin{array}{l} 15450 \\ 20303 \end{array} \right.$

No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.	
36	Ringswold - Shore - - <i>Woodnessborough Steeple</i>	$\begin{matrix} 33 & 7 & 44 \\ 77 & 48 & 16 \end{matrix}$	Woodnessborough Steeple -	Feet. 32320 18071
37	Shore - - Mount Pleasant <i>Ramsgate Windmill</i>	$\begin{matrix} 41 & 10 & 35 \\ 47 & 47 & 27 \end{matrix}$	Ramsgate Windmill -	22695 20173
38	Shore - - Mount Pleasant <i>St. Lawrence Steeple</i>	$\begin{matrix} 36 & 26 & 58 \\ 54 & 52 & 36 \end{matrix}$	St. Lawrence Steeple -	25064 18205
39	Waldershare - Mount Pleasant <i>Wingham Steeple</i>	$\begin{matrix} 32 & 2 & 55 \\ 31 & 1 & 14 \end{matrix}$	Wingham Steeple	35214 36259
40	Waldershare - Mount Pleasant <i>Goodneston Steeple</i>	$\begin{matrix} 31 & 12 & 40 \\ 17 & 58 & 32 \end{matrix}$	Goodneston Steeple -	24841 41711
41	Mount Pleasant Chislet - - <i>Birchington Steeple</i>	$\begin{matrix} 77 & 19 & 0 \\ 22 & 10 & 4 \end{matrix}$	Birchington Steeple -	11500 29735
42	Mount Pleasant Chislet - - <i>St. Nicholas Steeple</i>	$\begin{matrix} 19 & 36 & 3 \\ 21 & 19 & 41 \end{matrix}$	St. Nicholas Steeple - -	16690 15394
43	Mount Pleasant Chislet - - <i>Stormouth Steeple</i>	$\begin{matrix} 16 & 56 & 56 \\ 33 & 29 & 54 \end{matrix}$	Stormouth Steeple -	21519 11366

No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.
44	Mount Pleasant Chislet - - <i>Reculver Windmill</i>	$\begin{matrix} 22 & 14 & 40 \\ 81 & 14 & 59 \end{matrix}$	Reculver Windmill { $\begin{matrix} \text{Feet.} \\ 30556 \\ 11703 \end{matrix}$
45	Mount Pleasant Wingham - <i>South Reculver</i>	$\begin{matrix} 69 & 57 & 57 \\ 51 & 54 & 46 \end{matrix}$	South Reculver { $\begin{matrix} 31012 \\ 37017 \end{matrix}$
46	Mount Pleasant Wingham - <i>Hearne Windmill</i>	$\begin{matrix} 50 & 51 & 41 \\ 78 & 50 & 42 \end{matrix}$	Hearne Windmill { $\begin{matrix} 42669 \\ 33732 \end{matrix}$
47	Wingham - Waldershare - <i>Littlebourn Steeple</i>	$\begin{matrix} 102 & 34 & 17 \\ 11 & 3 & 35 \end{matrix}$	Littlebourn Steeple - { $\begin{matrix} 7752 \\ 39442 \end{matrix}$
48	Wingham - Chislet - - <i>Blean Steeple</i>	$\begin{matrix} 58 & 30 & 34 \\ 88 & 52 & 9 \end{matrix}$	Blean Steeple { $\begin{matrix} 39329 \\ 33544 \end{matrix}$
49	Wingham - Chislet - - <i>Wickham Steeple</i>	$\begin{matrix} 59 & 11 & 7 \\ 24 & 25 & 37 \end{matrix}$	Wickham Steeple { $\begin{matrix} 8824 \\ 18326 \end{matrix}$
50	Wingham - Chislet - - <i>Ickham Steeple</i>	$\begin{matrix} 72 & 3 & 26 \\ 22 & 6 & 13 \end{matrix}$	Ickham Steeple { $\begin{matrix} 8001 \\ 20228 \end{matrix}$
51	Wingham - Beverley Park - <i>Bridge Windmill</i>	$\begin{matrix} 47 & 35 & 34 \\ 44 & 59 & 50 \end{matrix}$	Bridge Windmill { $\begin{matrix} 23584 \\ 24628 \end{matrix}$

No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.	
52	Wingham - Beverley Park - <i>Nackington Steeple</i>	$\overset{\circ}{33} \overset{'}{27} \overset{''}{20}$ $68 \ 29 \ 54$	Nackington Stee- ple - -	Feet. 31688 18776
53	Wingham - - Hardres - - <i>Chillendon Windmill</i>	$80 \ 53 \ 7$ $21 \ 53 \ 16$	Chillendon Wind- mill -	15031 39811
54	Wingham - - Hardres - - <i>Preston Steeple</i>	$122 \ 1 \ 10$ $8 \ 3 \ 28$	Preston Steeple	7220 43572
55	Wingham - - Hardres - - <i>Sbottenden Windmill</i>	$30 \ 49 \ 24$ $118 \ 30 \ 8$	Shottenden Wind- -mill - -	67736 39494
56	Hardres - - Beverley Park - <i>St. Martin's Windmill</i>	$11 \ 35 \ 23$ $27 \ 48 \ 16$	St. Martin's Wind- mill -	22943 9881
57	Hardres - - Beverley Park - <i>Harbledown Steeple</i>	$12 \ 11 \ 37$ $39 \ 25 \ 30$	Harbledown Stee- ple - -	25289 8411
58	Hardres - - Beverley Park - <i>Sturry Steeple</i>	$17 \ 29 \ 59$ $84 \ 3 \ 53$	Sturry Steeple	31691 9581
59	Waldershare - Hardres - - <i>Canterbury Cathedral</i>	$24 \ 29 \ 21$ $105 \ 36 \ 14$	Canterbury Cathe- dral -	54827 23597

No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.
60	Hardres - -	4 ^o 45' 34"	} West-Stone-Street { } Windmill {
	Paddlesworth - - <i>West - Stone - Street</i> <i>Windmill</i>	27 23 18	
61	Hardres - -	31 0 20	} Stelling Windmill {
	Paddlesworth - - <i>Stelling Windmill</i>	15 3 20	

ART. V. *Triangles carried over another Part of Kent in 1795; with Remarks.*

On account of the high woody lands to the westward of Hardres and Paddlesworth, the triangles could not be extended in that direction, and therefore the following may be considered as a detached part of the Survey this year.

The Stations were,

Westwell Down,

Wye Down,

Brabourn Down,

Allington, or Aldington Knoll, the station of 1787.

Allington Knoll from Tenterden, according to General Roy's account, is 61775.3 feet, which increased in the proportion of 141747 to 141753 becomes 61778 feet. The centre of the top of Tenterden Steeple is about 4 or 4½ feet farther from Allington Knoll than the point marking the station in 1787; therefore the distance of the centre from Allington Knoll will be 61782 feet, which is used in the following computations; because, as a flagstaff of moderate height

cannot be easily distinguished among the pinnacles at any considerable distance, it was thought it might be sufficiently accurate for the present purpose, to intersect the steeple itself.

Triangles for determining the Distances of the Stations.

No.	Stations.	Observed angles.	Distances.		
62	Allington Knoll Westwell Down <i>Tenterden</i>	61 37 46 68 0 16 50 21 58	Westwell D. from	{ Tenterden Allington K.	Fect. 58629 51316
63	Allington Knoll Westwell Down Wye Down	34 37 37 45 54 19 99 28 5-4 180 0 1	Wye Down	{ Allington K. Westwell D.	37363 29562
64	Allington Knoll Wye Down <i>Tenterden</i>	96 15 23 54 19 24 29 25 13	Wye Down	{ Allington Tenterden	37360 75603
65	Wye Down Westwell Down <i>Tenterden</i>	45 8 41 113 54 35 20 56 44	Westwell D. from Wye D.		29566
66	Allington Knoll Brabourn Down <i>Tenterden</i>	116 49 40 45 25 31 17 44 49	Brabourn D.	{ Allington K. Tenterden	26437 77397
67	Allington Knoll Brabourn Down <i>Westwell Down</i>	55 11 54 93 52 23 30 55 43	Brabourn D.	{ Westwell D. Allington K.	42233 26435

ART. VI. Secondary Triangles.

No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.	Feet.
68	Wye Down - Westwell Down <i>Ashford Steeple</i>	$\begin{matrix} 42 & 20 & 58 \\ 53 & 35 & 53 \end{matrix}$	Ashford Steeple	$\begin{matrix} 23922 \\ 20023 \end{matrix}$
69	Wye Down - Westwell Down <i>Brook Steeple</i>	$\begin{matrix} 86 & 44 & 28 \\ 15 & 18 & 43 \end{matrix}$	Brook Steeple	$\begin{matrix} 7983 \\ 30181 \end{matrix}$
70	Wye Down - Westwell Down <i>Willsborough Steeple</i>	$\begin{matrix} 60 & 6 & 18 \\ 45 & 28 & 29 \end{matrix}$	Willsborough Steeple	$\begin{matrix} 21881 \\ 26607 \end{matrix}$
71	Wye Down - Westwell Down <i>Willsborough Windmill</i>	$\begin{matrix} 58 & 2 & 28 \\ 41 & 37 & 0 \end{matrix}$	Willsborough Windmill	$\begin{matrix} 19916 \\ 25443 \end{matrix}$
72	Wye Down - Westwell Down <i>Kingsnorth Steeple</i>	$\begin{matrix} 58 & 20 & 46 \\ 65 & 40 & 7 \end{matrix}$	Kingsnorth Steeple	$\begin{matrix} 32498 \\ 30360 \end{matrix}$
73	Wye Down - Westwell Down <i>Shadoxhurst Steeple</i>	$\begin{matrix} 52 & 13 & 44 \\ 85 & 50 & 2 \end{matrix}$	Shadoxhurst Steeple	$\begin{matrix} 44118 \\ 34966 \end{matrix}$
74	Wye Down - Westwell Down <i>Kennington Steeple</i>	$\begin{matrix} 26 & 38 & 18 \\ 27 & 54 & 54 \end{matrix}$	Kennington Steeple	$\begin{matrix} 16989 \\ 16271 \end{matrix}$
75	Wye Down - Allington Knoll <i>Great Chart Steeple</i>	$\begin{matrix} 62 & 23 & 7 \\ 54 & 24 & 4 \end{matrix}$	Great Chart Steeple	$\begin{matrix} 34029 \\ 37083 \end{matrix}$

No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.
76	Wye Down - Allington Knoll <i>Westwell Steeple</i>	$\begin{array}{r} 96^{\circ} 45' 26'' \\ 33 \quad 49 \quad 30 \end{array}$	Westwell Steeple { $\begin{array}{r} \text{Feet.} \\ 27384 \\ 48851 \end{array}$
77	Westwell Down Allington Knoll <i>Pluckley Steeple</i>	$\begin{array}{r} 97 \quad 22 \quad 43 \\ 20 \quad 53 \quad 1 \end{array}$	Pluckley Steeple { $\begin{array}{r} 20768 \\ 57778 \end{array}$
78	Westwell Down Allington Knoll <i>Eastwell Steeple</i>	$\begin{array}{r} 37 \quad 55 \quad 0 \\ 7 \quad 17 \quad 0 \end{array}$	Eastwell Steeple { $\begin{array}{r} 9168 \\ 44441 \end{array}$
79	Westwell Down Allington Knoll <i>Charing Steeple</i>	$\begin{array}{r} 146 \quad 22 \quad 23 \\ 5 \quad 24 \quad 0 \end{array}$	Charing Steeple { $\begin{array}{r} 10211 \\ 60085 \end{array}$
80	Westwell Down Allington Knoll <i>Allington Steeple</i>	$\begin{array}{r} 3 \quad 15 \quad 4 \\ 57 \quad 34 \quad 51 \end{array}$	Allington Steeple { $\begin{array}{r} 49609 \\ 3333 \end{array}$
81	Brabourn Steeple Allington Knoll <i>Lymne Steeple</i>	$\begin{array}{r} 34 \quad 50 \quad 49 \\ 75 \quad 59 \quad 12 \end{array}$	Lymne Steeple { $\begin{array}{r} 27443 \\ 16161 \end{array}$
82	Brabourn Down Allington Knoll <i>Mersham Steeple</i>	$\begin{array}{r} 33 \quad 12 \quad 51 \\ 45 \quad 9 \quad 19 \end{array}$	Mersham Steeple { $\begin{array}{r} 19136 \\ 14784 \end{array}$
83	Brabourn Down Allington Knoll <i>Monks Horton Steeple</i>	$\begin{array}{r} 67 \quad 22 \quad 25 \\ 23 \quad 46 \quad 14 \end{array}$	Monks Horton Steeple - { $\begin{array}{r} 10657 \\ 24405 \end{array}$

The bearings, and distances of the stations and intersected objects, together with their latitudes and longitudes, are given in the following Section.

SECTION II.

Operations in 1796, with the small circular Instrument.

ART. I. *Situations of the Stations.*

Lydd	}	Stations in the Survey of 1787.
Allington Knoll		
High Nook		
Fairlight Down		
Goudhurst		
Tenterden		

Westwell Down Station, used in 1795. See Art. v. Section I.

Part Second.

Silver Hill, near Robertsbridge. The station is 22 yards S. W. of the Windmill.

Boughton Malherb Steeple.

ART. II. *Triangles for finding the Distances of the Stations.*

From the 5th Article in the last Section, we get the distance from *Westwell Down* to the *new* station on *Tenterden Steeple* = 58629,4 feet. This used in the following triangle,

84	Boughton Malherb	81	55	9
	Westwell Down	63	44	8
	Tenterden -	34	20	43

gives the distance from *Boughton Malherb* to *Westwell Down* 33409 feet. Also in the following triangle, using 54376,9 feet for the distance from *Tenterden* to *Goudhurst*,

85	Goudhurst -	52	5	44
	Boughton Malherb	53	54	20
	Tenterden -	75	59	56
	MDCXCVII.		3	Y

we get 33404,5 feet for the distance between the same stations: hence the mean, 33406,8 feet, may be taken for the true distance between Boughton Malherb and Westwell Down. From this latter triangle also, we obtain the distance from Boughton Malherb to Tenterden 53097,6 feet.

No.	Triangles.	Observed angles.	Distances.	
86	Goudhurst Silver Hill Tenterden	65° 29' 7" 70 32 26 43 58 27	Silver Hill from Goudhurst	40043,1
				Feet.

Fairlight Down from Tenterden 71637,7 feet.

87	Fairlight Down Silver Hill Tenterden	46 34 5 82 25 8 51 0 47	Silver Hill from Fairlight D.	56174,2
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By the two last triangles, we get 52472,4 and 52481,4 feet for the distances of Tenterden from Silver Hill; the mean of which, 52476,9, we shall hereafter use in determining the distances of the objects, intersected from those stations.

The distance of Goudhurst from Tenterden, and that of Tenterden from Fairlight Down, are derived from those given by General Roy, in the Philosophical Transactions, Vol. LXXX. augmented in the proportion of 14,1747 to 14,1753. The distances also, hereafter made use of, between Lydd, and the stations on Fairlight Down, Tenterden Steeple, Allin on Knoll, and High Nook, together with that of High Nook from Allington Knoll, are obtained by increasing the distances, found in the same work, in the above proportion. It is proper to

remark, that it has not been thought necessary to reduce the distance between the station on Westwell Down, and the new station on Tenterden Steeple, to that between the former, and the old point at Tenterden, from the trifling difference of those distances.

During the operation of this year, the instrument was also taken to the following stations, *viz.*

- Bidenden Steeple,
- Hartridge,
- Warehorn Steeple,
- Stone Crouch,
- Iden Steeple.

To determine the distances between these objects, and the stations from whence they were observed, we have the following triangles.

No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.	
88	Goudhurst - Tenterden - <i>Bidenden Steeple</i>	$\begin{matrix} 18 & 16 & 4 \\ 40 & 0 & 12 \end{matrix}$	} Bidenden Steeple {	Feet. 41100 20040
89	Goudhurst - Tenterden - <i>Hartridge</i>	$\begin{matrix} 27 & 21 & 34 \\ 13 & 14 & 13 \end{matrix}$		} Hartridge {
90	Allington Knoll Lydd - - <i>Stone Crouch</i>	$\begin{matrix} 44 & 16 & 25 \\ 73 & 7 & 50 \end{matrix}$	} Stone Crouch {	
91	Allington Knoll Stone Crouch <i>Warehorn</i>	$\begin{matrix} 15 & 46 & 51 \\ 17 & 18 & 22 \end{matrix}$		} Warehorn - {

No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.
92	Tenterden - Fairlight Down - <i>Iden Steeple</i>	$\begin{matrix} 28 & 55 & 46 \\ 20 & 42 & 7 \end{matrix}$	<i>Iden Steeple</i> - $\left\{ \begin{array}{l} \text{Feet.} \\ 33239 \\ 45483 \end{array} \right.$

ART. III. *Secondary Triangles.*

93	Goudhurst - Tenterden - <i>Ulcomb Steeple</i>	$\begin{matrix} 59 & 47 & 4 \\ 61 & 44 & 12 \end{matrix}$	<i>Ulcomb Steeple</i> $\left\{ \begin{array}{l} 56184 \\ 55123 \end{array} \right.$
94	Goudhurst - Tenterden - <i>Sutton Windmill</i>	$\begin{matrix} 65 & 36 & 50 \\ 52 & 13 & 42 \end{matrix}$	<i>Sutton Windmill</i> $\left\{ \begin{array}{l} 48610 \\ 56009 \end{array} \right.$
95	Goudhurst - Tenterden - <i>Chart Sutton Steeple</i>	$\begin{matrix} 70 & 48 & 44 \\ 48 & 11 & 12 \end{matrix}$	<i>Chart Sutton Steeple</i> - - $\left\{ \begin{array}{l} 46338 \\ 58717 \end{array} \right.$
96	Goudhurst - Tenterden - <i>Linton Steeple</i>	$\begin{matrix} 91 & 32 & 50 \\ 36 & 54 & 6 \end{matrix}$	<i>Linton Steeple</i> $\left\{ \begin{array}{l} 41690 \\ 69407 \end{array} \right.$
97	Goudhurst - Tenterden - <i>Headcorn Windmill</i>	$\begin{matrix} 49 & 11 & 14 \\ 47 & 51 & 2 \end{matrix}$	<i>Headcorn Windmill</i> - $\left\{ \begin{array}{l} 40621 \\ 41468 \end{array} \right.$
98	Goudhurst - Hartridge <i>Cranbrook Steeple</i>	$\begin{matrix} 29 & 8 & 0 \\ 70 & 10 & 0 \end{matrix}$	<i>Cranbrook Steeple</i> $\left\{ \begin{array}{l} 18239 \\ 9439 \end{array} \right.$
99	Tenterden - Boughton Malherb <i>Benenden Steeple</i>	$\begin{matrix} 94 & 50 & 33 \\ 24 & 7 & 11 \end{matrix}$	<i>Benenden Steeple</i> $\left\{ \begin{array}{l} 24799 \\ 60471 \end{array} \right.$

No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.	Feet.
100	Bidenden - Goudhurst - <i>Staplehurst Steeple</i>	$\begin{matrix} 37 & 0 & 0 \\ 38 & 47 & 0 \end{matrix}$	} Staplehurst Steeple {	$\begin{matrix} 25514 \\ 26555 \end{matrix}$
101	Bidenden - Goudhurst - <i>Marden Steeple</i>	$\begin{matrix} 33 & 30 & 0 \\ 70 & 42 & 33 \end{matrix}$	} Marden Steeple {	$\begin{matrix} 40015 \\ 23399 \end{matrix}$
102	Boughton Malherb Goudhurst - <i>Frittenden Steeple</i>	$\begin{matrix} 14 & 39 & 40 \\ 17 & 10 & 0 \end{matrix}$	} Frittenden Steeple {	$\begin{matrix} 36203 \\ 31405 \end{matrix}$
103	Tenterden - Silver Hill - <i>Brasses Windmill</i>	$\begin{matrix} 20 & 46 & 0 \\ 76 & 45 & 52 \end{matrix}$	} Brasses Windmill {	$\begin{matrix} 51527 \\ 18768 \end{matrix}$
104	Tenterden - Silver Hill - <i>Hawkhurst Steeple</i>	$\begin{matrix} 11 & 2 & 0 \\ 42 & 17 & 30 \end{matrix}$	} Hawkhurst Steeple {	$\begin{matrix} 44028 \\ 12522 \end{matrix}$
105	Silver Hill - Fairlight Down <i>Sandhurst Steeple</i>	$\begin{matrix} 72 & 5 & 37 \\ 17 & 1 & 25 \end{matrix}$	} Sandhurst Steeple {	$\begin{matrix} 16448 \\ 53460 \end{matrix}$
106	Silver Hill - Fairlight Down <i>Whittersham Steeple</i>	$\begin{matrix} 58 & 27 & 19 \\ 55 & 42 & 10 \end{matrix}$	} Whittersham Stee- ple - - {	$\begin{matrix} 50861 \\ 52469 \end{matrix}$
107	Silver Hill - Fairlight Down <i>Peasemarsb Steeple</i>	$\begin{matrix} 38 & 49 & 4 \\ 59 & 39 & 33 \end{matrix}$	} Peasemarsb Stee- ple - - {	$\begin{matrix} 49016 \\ 35602 \end{matrix}$

No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.	
108	Silver Hill - Fairlight Down <i>Rolvenden Steeple</i>	$\begin{array}{ccc} 82 & 8 & '' \\ 36 & 28 & 0 \end{array}$	Rolvenden Steeple {	$\begin{array}{r} \text{Feet.} \\ 38028 \\ 63380 \end{array}$
109	Silver Hill - Fairlight Down <i>Beckley Steeple</i>	$\begin{array}{ccc} 42 & 30 & 35 \\ 35 & 36 & 7 \end{array}$	Beck'ey Steeple {	$\begin{array}{r} 33419 \\ 38790 \end{array}$
110	Allington Knoll High Nook - <i>New Church Steeple</i>	$\begin{array}{ccc} 46 & 3 & 7 \\ 36 & 41 & 43 \end{array}$	New Church Stee- ple - - {	$\begin{array}{r} 13967 \\ 16828 \end{array}$
111	Allington Knoll High Nook - <i>Ivy Church Steeple</i>	$\begin{array}{ccc} 52 & 3 & 53 \\ 76 & 5 & 26 \end{array}$	Ivy Church Steeple {	$\begin{array}{r} 28621 \\ 23256 \end{array}$
112	Allington Knoll High Nook - <i>St. Mary's Steeple</i>	$\begin{array}{ccc} 27 & 21 & 0 \\ 80 & 5 & 0 \end{array}$	St. Mary's Steeple {	$\begin{array}{r} 23939 \\ 11165 \end{array}$
113	Tenterden - Lydd - - <i>Playden Steeple</i>	$\begin{array}{ccc} 34 & 33 & 5 \\ 34 & 35 & 48 \end{array}$	Playden Steeple {	$\begin{array}{r} 40204 \\ 40158 \end{array}$
114	Iden - - - Fairlight Down <i>Winchelsea Steeple</i>	$\begin{array}{ccc} 21 & 57 & 0 \\ 17 & 5 & 40 \end{array}$	Winchelsea Stee- ple - - {	$\begin{array}{r} 21224 \\ 26990 \end{array}$
115	Winchelsea - Fairlight Down <i>Brede Steeple</i>	$\begin{array}{ccc} 48 & 6 & 0 \\ 67 & 26 & 0 \end{array}$	Brede Steeple {	$\begin{array}{r} 26373 \\ 21755 \end{array}$

No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.	
116	Brede Steeple - Fairlight Down <i>Icklesham Steeple</i>	$\begin{matrix} 56 & 0 & 0 \\ 55 & 1 & 0 \end{matrix}$	} Icklesham Steeple {	Feet. 19091 19313
117	Stone Crouch - Allington Knoll <i>Woodchurch Steeple</i>	$\begin{matrix} 55 & 9 & 34 \\ 32 & 59 & 15 \end{matrix}$		} Woodchurch Steeple - {
118	Stone Crouch - Allington Knoll <i>Old Romney Steeple</i>	$\begin{matrix} 41 & 36 & 38 \\ 35 & 59 & 39 \end{matrix}$	} Old Romney Steeple - {	
119	Stone Crouch - Allington Knoll <i>New Romney Steeple</i>	$\begin{matrix} 41 & 54 & 7 \\ 52 & 11 & 33 \end{matrix}$		} New Romney Steeple - {
120	Stone Crouch - Allington Knoll <i>Brookland Steeple</i>	$\begin{matrix} 40 & 47 & 1 \\ 14 & 44 & 21 \end{matrix}$	} Brookland Steeple {	
121	Stone Crouch - Allington Knoll <i>Orleston Steeple</i>	$\begin{matrix} 20 & 16 & 5 \\ 29 & 46 & 58 \end{matrix}$		} Orleston Steeple {
122	Stone Crouch - Lydd - - <i>East Guilford Steeple</i>	$\begin{matrix} 67 & 14 & 56 \\ 24 & 46 & 59 \end{matrix}$	} East Guilford Steeple - {	
123	Stone Crouch - Lydd - - <i>Snargate Steeple</i>	$\begin{matrix} 53 & 4 & 1 \\ 28 & 2 & 7 \end{matrix}$		} Snargate Steeple {

No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.	
124	Stone Crouch - Warehorn Steeple <i>Snave Steeple</i>	$\begin{array}{ccc} 0 & ' & '' \\ 25 & 37 & 0 \\ 81 & 34 & 0 \end{array}$	} Snave Steeple - {	Fect. 26667 11629
125	Stone Crouch - Warehorn - <i>Appledore Steeple</i>	$\begin{array}{ccc} 9 & 11 & 12 \\ 6 & 46 & 0 \end{array}$		} Appledore Steeple {
126	Warehorn - D Allington Knoll <i>Brenzset Steeple</i>	$\begin{array}{ccc} 91 & 6 & 0 \\ 30 & 5 & 41 \end{array}$	} Brenzset Steeple {	
127	Allington Knoll - Westwell Down <i>Bethersden Steeple</i>	$\begin{array}{ccc} 36 & 36 & 26 \\ 68 & 55 & 44 \end{array}$		} Bethersden Steeple {
128	Allington Knoll Westwell Down <i>High Halden Steeple</i>	$\begin{array}{ccc} 49 & 12 & 12 \\ 70 & 39 & 8 \end{array}$	} High Halden Stee- ple - {	
129	Westwell Down Boughton Malherb <i>Lenham Steeple</i>	$\begin{array}{ccc} 17 & 24 & 40 \\ 64 & 19 & 30 \end{array}$		} Lenham Steeple {
130	Westwell Down Boughton Malherb <i>Egerton Steeple</i>	$\begin{array}{ccc} 12 & 31 & 21 \\ 30 & 1 & 45 \end{array}$	} Egerton Steeple {	
131	Westwell Down Boughton Malherb <i>Turret on Romden Stables</i>	$\begin{array}{ccc} 42 & 50 & 41 \\ 71 & 6 & 34 \end{array}$		} Turret on Romden Stables - {

No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.			
132	Westwell Down - Boughton Malherb Smarden Steeple	$\begin{array}{ccc} \overset{\circ}{49} & \overset{\prime}{12} & \overset{\prime\prime}{12} \\ 70 & 39 & 8 \end{array}$	Smarden Steeple { <table style="display: inline-table; vertical-align: middle;"> <tr> <td style="text-align: right;">Fees.</td> </tr> <tr> <td style="text-align: right;">39106</td> </tr> <tr> <td style="text-align: right;">23850</td> </tr> </table>	Fees.	39106	23850
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SECTION III.

Containing the Distances of the Objects intersected in the Survey with the small circular Instrument, from the Meridian of Greenwich, and from the Perpendicular to that Meridian. Also their Latitudes and Longitudes.

ART. I. *Bearings and Distances, 1795.*

At Folkstone turnpike, the bearing of the station on Dover Castle in 1787, from the parallel to the meridian of Greenwich is $65^{\circ} 52' 46''$ NE (See Phil. Trans. Vol. LXXX, page 603). The new point on the Keep is $6\frac{1}{2}$ feet north-eastward from the old one, which will subtend an angle at Folkstone turnpike of about $38''$; therefore the new station bears $65^{\circ} 52' 8''$ NE. The bearing of the centre of Tenterden Steeple from Allington Knoll, is nearly the same as that of the station in 1787, or $85^{\circ} 47' 25''$ S W. : but the distances of those stations (Folkstone turnpike and Allington Knoll, see page 232 of the same Volume), from the meridian of Greenwich, and its perpendicular, are augmented in the proportion of 141747 to 141753, for obtaining the distances in the 3d and 4th columns of the following table: Folkstone turnpike being 274979 and 137220; and Allington Knoll 219935 and 144038 feet, respectively, from the meridian, and its perpendicular.

Bearings and Distances of the Stations.

Bearings from the Parallels to the Meridian of Greenwich.		Distances from merid.	Distances from perp.
<i>At Folkstone Turnpike.</i>			
Dover	65 52 8 NE	303780	124318
Hawkinge	29 45 38 NE	276605	134376
<i>At Dover.</i>			
Paddlesworth	81 30 42 SW	262004	130553
Waldershare	56 24 53 NW	290114	105792
Ringswold	30 21 52 NE	315783	103830
<i>At Waldersbare.</i>			
Shore	39 54 35 NE	317545	72997
Mount Pleasant	11 56 19 NE	302716	46190
Wingham	16 36 24 NW	279533	70315
Hardres	74 21 9 NW	248180	94046
Hawkinge	25 17 53 SW		
Ringswold	85 37 43 NE		
<i>Near the Shore.</i>			
Ringswold	3 16 15 SW		
Mount Pleasant	28 56 58 NW		
<i>At Mount Pleasant.</i>			
Wingham	43 51 31 SW		
Chislet	82 23 48 SW	272918	50168
<i>At Wingham.</i>			
Chislet	18 10 37 NW		
Hardres	52 52 37 SW		
Beverley Park	77 3 23 NW	247060	62852
<i>At Beverley Park.</i>			
Hardres	2 3 23 SE		
<i>At Allington Knoll.</i>			
Tenterden	85 47 25 SW		
Westwell Down	32 34 49 NW	192302	100797
Wye Down	2 2 48 NE	221269	106701
Brabourn Down	22 37 5 NE	230102	119636

Interior Objects.

<i>At Dover.</i>			
St. Radigund's Abbey	88 5 4 NW	287597	123777
Hougham Steeple	68 19 22 SW	288341	130455
Gunston Steeple	3 43 2 NW	303189	115226

Bearings from the Parallels to the Meridian of Greenwich.			Distances from merid.	Distances from perp.	
	°	'	"	Feet.	Feet.
St. Margaret's Steeple - - -	51	54	43	315148	115408
South Foreland Light-House - -	70	10	31	315145	120721
<i>At Waldersbare.</i>					
Barham Windmill - - -	61	0	4	278573	93852
Elham Windmill - - -	10	14	39	284430	137246
Upper Deal Chapel - - -	63	17	33	317056	92237
Deal Castle - - -	66	9	16	321842	91768
Watch-house near the Shore - - -	85	2	37	321314	108498
Sandown Castle - - -	55	51	56	321721	84365
Walmer Steeple - - -	73	8	30	318338	97239
Ripple Steeple - - -	70	1	50	302867	97534
Waldershare Steeple - - -	64	52	20	295235	103390
Eastry Steeple - - -	23	30	46	300393	82166
Ash Steeple - - -	4	44	29	293069	70165
Minster Steeple - - -	11	24	56	301155	51113
Woard Steeple - - -	34	11	33	308967	78042
Sandwich highest Steeple - - -	26	19	14	307187	71279
Wingham Steeple - - -	20	6	36	278007	72725
Goodneston Steeple - - -	19	16	21	281915	82343
Littlebourn Steeple - - -	27	39	59	278100	70860
Canterbury Cathedral - - -	49	51	48	248198	60458
<i>At Ringswold.</i>					
Mongeham Steeple - - -	21	30	34	311611	93243
Norbourn Steeple - - -	31	52	45	307623	90710
Woodnesborough Steeple - - -	29	51	29	299693	75800
<i>Near the Shore.</i>					
Ramsgate Windmill - - -	12	13	43	321363	50817
St. Lawrence Steeple - - -	7	30	6	320817	48148
<i>At Mount Pleasant.</i>					
Birchington Steeple - - -	20	17	12	298729	35403
St. Nicholas Steeple - - -	78	0	9	286391	42721
Stormouth Steeple - - -	65	26	52	283143	55132
<i>At Wingham.</i>					
The South Reculver - - -	8	3	15	274346	33663
Hearne Windmill - - -	34	59	11	260191	42679
Blean Steeple - - -	76	41	11	241261	61259
Wickham Steeple - - -	77	21	44	270923	68384
Bridge Windmill - - -	55	21	3	260132	83723
Nackington Steeple - - -	69	29	17	249854	81418
Chillingdon Windmill - - -	28	0	30	286591	83586
Preston Steeple - - -	5	6	13	278891	63124
Shottenden Windmill - - -	83	42	1	212206	77748
Ickham Steeple - - -	89	45	57	271533	70348

Bearings from the Parallels to the Meridian of Greenwich.		Distances from merid.	Distances from perp.
<i>At Hardres.</i>		Feet.	Feet.
Harbledown Steeple - - -	0 15 0 NW	241955	69535
Sturry Steeple - - -	15 26 36 NE	256019	63499
West Stone-street Windmill - - -	35 46 24 SW	236870	109743
Stelling Windmill - - -	26 1 10 SW	242003	106700
<i>On Westwell Down.</i>			
Ashford Steeple - - -	24 53 15 SE	200728	118961
Brook Steeple - - -	63 10 25 SE	219234	114417
Willsborough Steeple - - -	33 0 39 SE	200797	123109
Kingsnorth Steeple - - -	12 49 1 SE	199037	130400
Shadoxhurst Steeple - - -	7 20 54 SW	187830	135470
Kennington Steeple - - -	50 34 14 SE	204869	111131
<i>At Allington Knoll.</i>			
Great Chart Steeple - - -	52 21 16 NW	190572	121389
Westwell Steeple - - -	31 46 42 NW	194208	102510
Pluckley Steeple - - -	53 27 50 NW	173511	109641
Eastwell Steeple - - -	25 17 49 ⁰ NW	200945	103951
Charing Steeple - - -	37 58 49 NW	182959	96677
Allington Steeple - - -	25 0 2 NE	221344	141017
Lymne Steeple - - -	81 23 44 SE	235914	146456
Mersham Steeple - - -	22 32 14 NW	214269	130383
Monks-Horton Steeple - - -	46 23 19 NE	237605	127204

ART. II. *Bearings and Distances of the Stations, and Interior Objects, intersected in 1796.*

<i>At Goudburst.</i>			
Boughton Malherb - - -	54 59 23 NE	159324	95480
Bitenden - - -	88 49 3 NE	147431	131744
Hartridge - - -	79 43 33 NE		
<i>At Fairlight Down.</i>			
Silver Hill - - -	34 28 24 NW		
Iden Steeple - - -	33 33 48 NE	168454	180711
Brede Steeple - - -	13 48 32 NW	138116	197485
<i>At Allington Knoll.</i>			
Stone Crouch - - -	57 3 23 SW	176642	172082
Warehorn Steeple - - -	72 50 14 SW	193071	152324

Interior Objects.

Bearings from the Parallels to the Meridian of Greenwich.		Distances from merid.	Distances from perp.
<i>At Goudburst.</i>		Feet.	Feet.
Frittenden Steeple - - -	72 9 23 NE	135894	123079
Lintley Steeple - - -	15 32 17 NE	117510	92425
Chart Sutton Steeple - - -	36 16 23 NE	133757	95234
Sutton Windmill - - -	41 28 17 NE	138534	96169
Ulcomb Steeple - - -	47 18 3 NE	147633	94491
Headcorn Windmill - - -	57 54 53 NE	140758	111015
Staplehurst - - -	51 49 3 NE	127216	116176
Cranbrook Steeple - - -	71 8 27 SE	123602	138488
<i>At Fairlight Down.</i>			
Rolvenden Steeple - - -	1 59 36 NE	145513	155271
Beckley Steeple - - -	1 7 43 NE	144072	179830
Peasemash Steeple - - -	25 11 9 NE	158458	186395
Whittersham Steeple - - -	21 13 46 NE	162307	169704
Sandhurst Steeple - - -	17 26 59 NW	127277	167613
Winchelsea Steeple - - -	50 39 28 NE	164181	201501
Icklesham Steeple - - -	41 12 28 NW	156031	204073
<i>At Allington Knoll.</i>			
Bethersden - - -	69 11 15 NW	173469	126373
High Halden - - -	81 47 1 NW	164672	136054
Orleston Steeple - - -	86 50 21 SW	196655	145317
Woodchurch Steeple - - -	89 57 22 NW	177569	144000
Warehorn Steeple - - -	72 50 14 SW	193071	152324
Brookland Steeple - - -	42 19 2 SW	192410	174253
Old Romney Steeple - - -	21 3 44 SW	207322	176759
New Romney Steeple - - -	4 41 50 SW	217098	178460
<i>At Boughton Malberb.</i>			
Benenden Steeple - - -	25 12 54 SW	129542	150187
<i>At Silver Hill.</i>			
Brasses Windmill - - -	40 7 4 SE	123521	187554
<i>At High Nook.</i>			
New Church Steeple - - -	57 43 31 NW	214018	156687
Ivy Church Steeple - - -	82 52 46 SW	205170	168562
St. Mary's Steeple - - -	78 53 12 SW	204756	170287
<i>At Lydd.</i>			
Playden Steeple - - -	85 1 0 NW	169333	187207

Bearings from the Parallels to the Meridian of Greenwich.				Distances from merid	Distances from perp.
<i>At Westwell.</i>				Feet.	Feet.
Lenham Steeple	-	-	63 25 45 NW	165089	87178
Egerton Steeple	-	-	86 38 14 SW	167021	102243
Smarden Steeple	-	-	61 47 14 SW	157842	119273
Turret on Romden Stables	-	-	56 18 54 SW	193521	115970
<i>At Stone Crouch.</i>					
Appledore Steeple	-	-	30 33 49 NE	182243	162595
Snave Steeple	-	-	65 22 1 NE	200828	160993
Snargate Steeple	-	-	66 35 7 NE	193068	164909
East Guilford Steeple	-	-	6 54 4 SW	174746	187750

ART. III. *Latitudes and Longitudes of Objects intersected in*
1795.

Names of Objects.	Latitude.	Longitude east from Greenwich.	
		In degrees.	In time.
The Belvidere in Waldershare Park	51 11 13	0 1 39	m. 5.
Ringswold, or Kingswold Steeple	51 11 8	1 15 39	5 2.6
Upper Hardres Steeple	51 13 1	1 22 20	5 29.3
Chislet Steeple	51 13 1	1 4 45	4 19
St. Radigund's Abbey	51 20 4	1 11 24	4 45.6
Hougham Steeple	51 7 56	1 14 44	4 58.9
Gunston Steeple	51 6 50	1 15 4	5 0.3
St. Margaret's Steeple	51 9 18	1 19 0	5 16
South Foreland Light-House	51 9 14	1 22 7	5 28.5
Burham Windmill	51 8 21	1 22 6	5 28.4
Elham Windmill	51 12 52	1 12 41	4 50.7
Upper Deal Chapel	51 5 44	1 14 1	4 56.1
Deal Castle	51 13 2	1 22 44	5 30.9
Watch-house near the sea shore	51 13 5	1 23 59	5 35.9
Sandown Castle	51 10 21	1 23 46	5 35.1
Walmer Steeple	51 14 18	1 23 59	5 35.9
Ripple Steeple	51 15 29	1 23 8	5 32.5
Waldershare Steeple	51 12 12	1 19 0	5 16
Eastry Steeple	51 11 15	1 16 59	5 7.9
Ash Steeple	51 14 44	1 18 26	5 13.7
Minster Steeple	51 16 44	1 16 34	5 6.3
Woad Steeple	51 19 50	1 18 46	5 15.1
Sandwich highest Steeple	51 15 23	1 20 41	5 22.7
Wingham Steeple	51 16 30	1 20 15	5 21
Goodneston Steeple	51 16 21	1 12 38	4 50.5
Littlebourn Steeple	51 14 45	1 13 26	4 53.7
Canterbury Cathedral	51 16 40	1 11 1	4 44.1
	51 18 26	1 4 53	4 19.5

Names of objects.	Latitude.	Longitude east from Greenwich.	
		In degrees.	In time.
Mongeham Steeple	° 51 12 53	° 1 21 18	m. 5 25.2
Norbourn, or Northbourn Steeple	51 13 18	1 20 17	5 21.1
Woodnessborough, or Woodnesbor. Steeple	51 14 47	1 18 16	5 13.1
Ramsgate Windmill	51 19 49	1 24 4	5 36.3
St. Lawrence Steeple	51 20 16	1 23 56	5 43.7
Birchington Steeple	51 22 25	1 16 13	5 4.8
St. Nicholas Steeple	51 21 15	1 14 57	4 59.8
Stourmouth, or Stormouth Steeple	51 19 8	1 14 3	4 56.2
The South Reculver	51 22 47	1 11 50	4 47.3
Hearne Windmill	51 21 20	1 8 6	4 32.4
Blean Steeple	51 18 19	1 3 4	4 12.3
Wickham Steeple	51 17 5	1 10 48	4 43.2
Ickham Steeple	51 17 47	1 10 7	4 40.5
Bridge Windmill	51 14 35	1 7 55	4 31.7
Nackington Steeple	51 14 59	1 5 14	4 20.9
Chillingdon Windmill	51 14 30	1 14 49	4 59.3
Preston Steeple	51 17 55	1 12 54	4 51.6
Shottenden Windmill	51 15 41	0 55 25	3 41.7
Harbledown Steeple	51 16 58	1 3 13	4 12.9
Sturry Steeple	51 17 55	1 7 5	4 28.3
West-Stone-street Windmill	51 10 22	1 1 45	4 7
Stelling Windmill	51 10 51	1 3 6	4 12.4
Ashford Steeple	51 8 56	0 52 18	3 29.2
Brook Steeple	51 9 38	0 57 8	3 48.5
Willsborough Steeple	51 8 14	0 53 52	3 35.5
Kingsnorth Steeple	51 7 3	0 51 49	3 27.3
Shadoxhurst Steeple	51 6 14	0 48 53	3 15.5
Kennington Steeple	51 10 12	0 53 17	3 33.2
Great Chart Steeple	51 8 33	0 49 39	3 18.6
Westwell Steeple	51 11 39	0 50 39	3 22.6
Pluckley Steeple	51 10 30	0 45 14	3 0.9
Eastwell Steeple	51 11 23	0 52 24	3 29.6
Charing Steeple	51 12 37	0 47 44	3 10.9
Allington, or Aldington Steeple	51 5 16	0 57 36	3 50.4
Lymne Steeple	51 4 20	1 1 22	4 5.5
Mersham Steeple	51 7 1	0 55 47	3 43.1
Monks Horton Steeple	51 7 30	1 1 53	4 7.5

Latitudes and Longitudes of Objects intersected in 1796.

Names of objects.	Latitude.	Longitude east from Greenwich.	
		In degrees.	In time.
Linton Steeple	0 51 13 24	0 30 40	m. s. 2 2,7
Sutton Windmill	51 12 46	0 36 9	2 24,6
Chart Sutton Steeple	51 12 56	0 34 54	2 19,6
Lenham Steeple	51 14 13	0 43 6	2 52,4
Romden Stables	51 8 49	0 42 36	2 50,4
Smarden Steeple	51 8 57	0 41 8	2 44,5
Bethersden Steeple	51 7 45	0 45 10	3 0,7
Rolvenden Steeple	51 3 3	0 37 50	2 31,3
Beckley Steeple	50 59 1	0 37 24	2 29,6
Bidenden Steeple	51 7 3	0 38 23	2 33,5
Headcorn Windmill	51 10 21	0 36 41	2 26,7
Ulcomb Steeple	51 13 1	0 38 31	2 33
Staplehurst Steeple	51 9 30	0 33 9	2 12,6
Cranbrook Steeple	51 5 50	0 32 10	2 8,7
Egerton Steeple	51 11 44	0 43 43	2 54,9
Frittenden Steeple	51 8 20	0 35 24	2 21,6
Snargate Steeple	51 1 23	0 50 10	3 20,7
Snave Steeple	51 2 1	0 52 12	3 28,8
Warehorn Steeple	51 3 27	0 50 13	3 20,9
Orleston Steeple	51 4 36	0 51 10	3 24,7
Winchelsea Steeple	50 55 26	0 43 34	2 54,3
Sandhurst Steeple	51 1 3	0 33 4	2 12,3
Whitershams Steeple	51 0 39	0 42 10	2 48,7
New Church Steeple	51 2 42	0 55 38	3 42,5
Ivy Church Steeple	51 0 45	0 53 18	3 33,2
St. Mary's Steeple	51 0 29	0 53 11	3 32,7
East Guilford Steeple	50 57 50	0 45 21	3 1,4
Appledore Steeple	51 1 47	0 47 22	3 9,5
Old Romney Steeple	50 59 25	0 53 50	3 35,3
New Romney Steeple	50 59 7	0 56 22	3 45,5
Playden Steeple	50 57 46	0 43 56	2 55,7
Brookland Steeple	50 59 51	0 49 58	3 19,9
Iden Steeple	50 58 50	0 43 43	2 54,9
Brede Steeple	50 56 7	0 35 49	2 23,3
Benenden Steeple	51 3 54	0 33 41	2 14,8
Brasses Windmill	50 57 46	0 32 3	2 8,2
Icklesham Steeple	50 55 1	0 40 29	2 42
Boughton Malherb Steeple	51 12 51	0 41 34	2 46,3
Peasemarshe Steeple	50 57 54	0 41 7	2 44,5
Woodchurch Steeple	51 4 51	0 46 12	3 4,8
High Halden Steeple	51 6 11	0 42 52	2 51,5

CONCLUSION.

THE account contained in the foregoing pages is presented in its present form, agreeable to the resolution expressed in our last communication. It is there stated, or rather implied, that, as materials are collected, details will meet the public eye through the medium of the Philosophical Transactions. The publishing of these particulars at periods not very remote from each other, will prove convenient, as we shall be enabled to communicate many *data*, which would be necessarily withheld, were these disclosures less frequent. It is on this account, that the particulars in *Part the First* do not contain the latitudes and longitudes of the stations, and objects intersected, as sufficient *data* have not yet been obtained for making the computations in an unexceptionable manner: but the contents of the *Second Part* are more complete, that Survey having been carried on in a country sufficiently near the meridian of Greenwich to give the necessary arguments with precision.

It is perhaps scarcely necessary to observe, that the design intended to be answered by an admission of the plans of the triangles annexed to this account, is to enable the reader to comprehend with ease the state of the operation, and to apply, without difficulty, the materials found in the body of the work to future Surveys. We have therefore, not attempted to delineate any varieties of ground in the plan of the western triangles (Tab. XI.): and it may, in this place, be proper to mention,

that the ranges of hills expressed in the plan found in our last account, were copied from authorities of the late Major General ROY. The map now given, of the operations performed in Kent (Tab. XII.), has the ground depicted in as accurate a manner as the scale will admit of, Mr. GARDNER, from the minuteness of this Survey, being enabled to do it with accuracy.

On adverting to a principal object of this undertaking, that of preparing materials for correcting the geography of the country, it may be expected something should be said, respecting the accuracy of the maps of those counties in which our operations have been carried on. It is almost unnecessary to observe, that great correctness cannot result from the methods commonly taken in large surveys, which are usually made with an apparatus altogether unfit for measuring angles or bases with a sufficient degree of accuracy: and it will evidently appear, on applying the distances given in this, and our former paper, to those maps, that they are, generally, very defective. We must, however, observe, that LINLEY'S and CROSSLEY'S Map of Surry, and GARDNER'S Map of Sussex, are the best which have yet fallen under our notice: the first is, in some measure, indebted for its excellence to the Trigonometrical Operation in 1787; and the latter to our own; as the distances between many stations, and the situations of many churches, in the southern, and western parts of Sussex, were given to Mr. GARDNER prior to the publication of our last account. The geography of Devonshire and Dorsetshire is found particularly erroneous, as may be easily discovered by an application of our distances to the best maps of those counties.

N. B. In Tab. XI. the triangles connecting the three principal objects in the Scilly Isles, and the stations from whence they were intersected, are laid down in that detached position to shorten the plan.

Errata in the Account of the Survey, Philos. Trans. 1795.

Page 469, line 4, for 124 read 125.

507, line 18, for 258 read 285.

527, in the table, for 51° and 60° read 50° and 66° .

ib. *ib.* col. 4, for 30 read 33.

554, against Southwick Church, for 57710 read 5771.

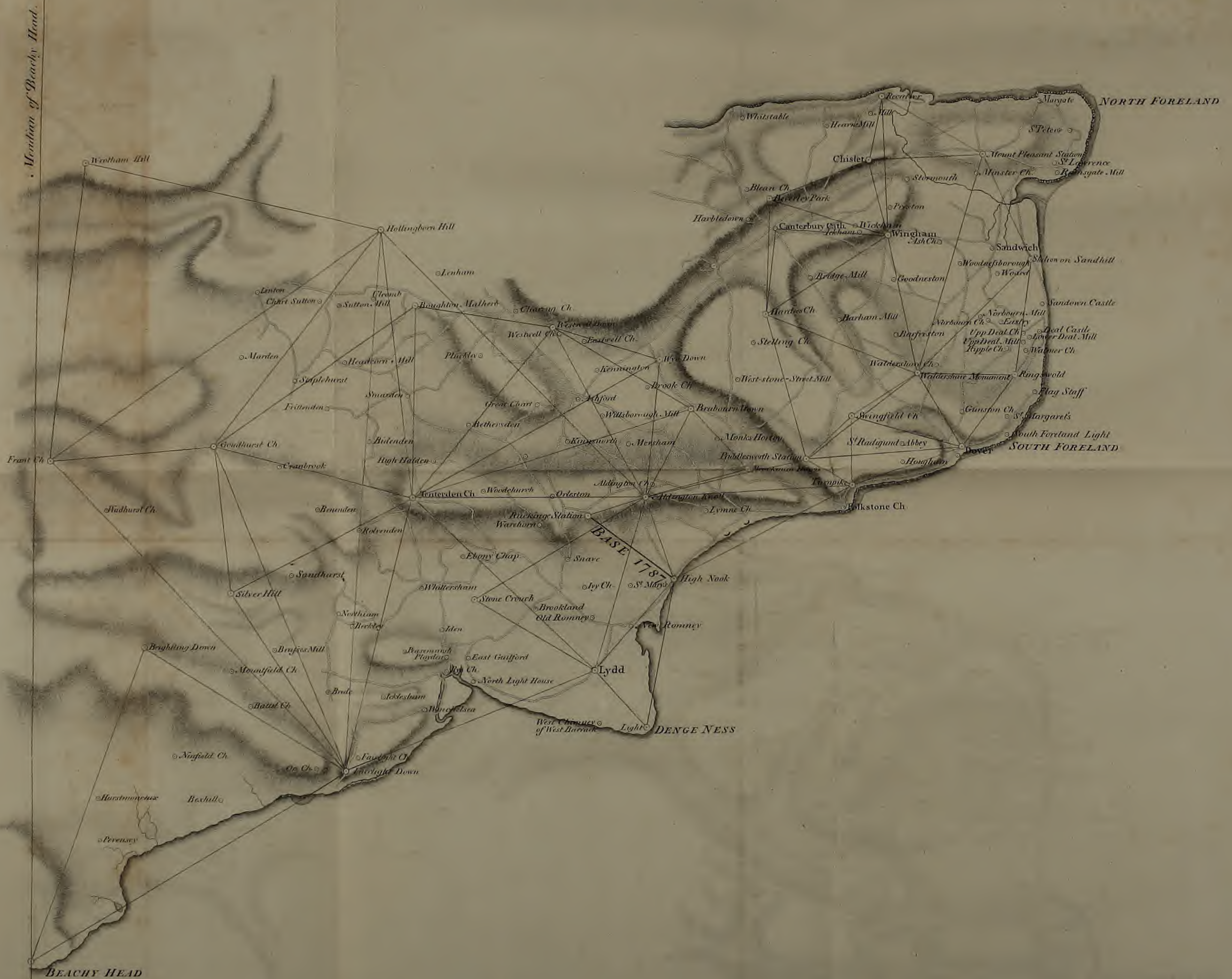
558 *et alibi*, for Mitford read Milford.

559 *et alibi*, for Funtingdon read Fordington.

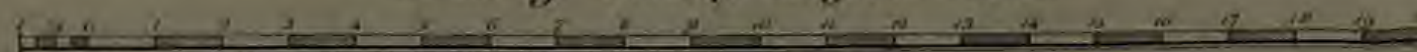
580, line 10 from bottom, for $39''$ read $47''$.

584, lines 2 and 3, for $\frac{1}{15}$ read $\frac{1}{13}$.

The triangles numbered 84, 100, 105, are doubtful, and consequently the results depending on them are uncertain.

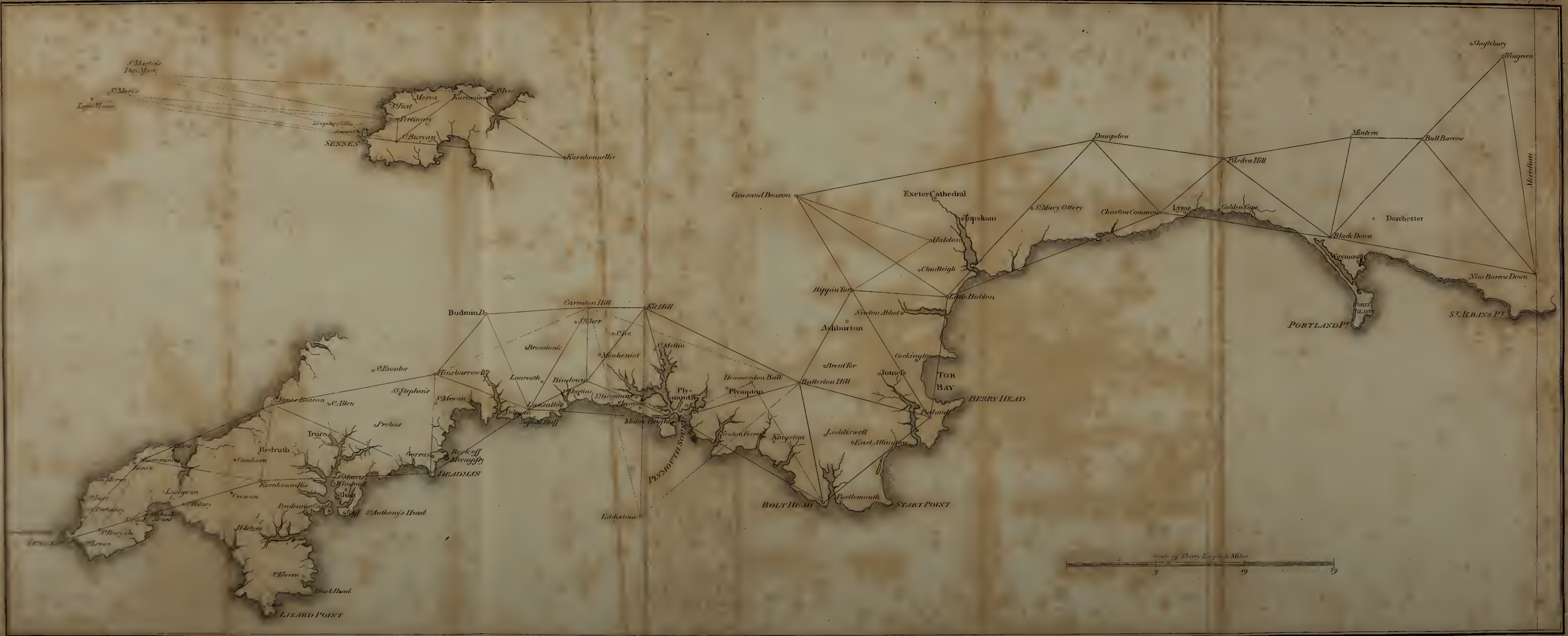


Scale of Twenty English Miles





PLAN of the PRINCIPAL TRIANGLES in the TRIGONOMETRICAL SURVEY, 1795-1796.





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	Anatomisches Museum, gesammelt von J. G. Walter, beschrieben von F. A. Walter. 1 und 2 Theil. Berlin, 1796. 4°	Professor J. G. Walter, F. R. S.
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ERRATA.

Page 259, line 5 below the table, for 436800 read 412529.
 386, at end of paragraph, after lithic add acid.
 393, line 2, for earth read earths.
 397, line 17, for By acid of read By aid of.



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