PHILOSOPHICAL TRANSACTIONS,

OF THE

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PART I.

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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.

CONTENTS.

APPENDIX.

THE PRESIDENT and COUNCIL of the ROYAL SOCIETY adjudged, for the year 1794, the Medal on Sir GODFREY COPLEY's Donation, to Sig. ALESSANDRO VOLTA, Professor of Experimental Philosophy in the University of Pavia, for his several communications explanatory of certain Experiments published by Professor Galvani.

PHILOSOPHICAL

TRANSACTIONS.

I. *The* Croonian *Lecture on Muscular Motion. By* Everard Home, *Esq. F. R. S.*

Read November 13, 1794.

WHEN I had the honour last year of presenting an apology for the unfinished state in which Mr. HUNTER left the CROONIAN lecture, I laid before this learned Society the plan upon which he meant to proceed; but my mind was at that time unfitted to prosecute so arduous an inquiry.

The progress Mr. HUNTER had made in this investigation enabled him to prove the crystalline humour of the eye to be laminated, and the laminæ to be composed of fibres; but the use to which these fibres are applied in the œconomy of the eye he had not ascertained, although several experiments were instituted with that view: his opinion was certainly in favour of their being muscular, for the purpose of adjusting the eye to different distances by their contraction and relaxation.

Being unwilling that a subject on which Mr. HUNTER had so publicly given his opinion should remain in an unfinished state, I requested the President's permission to be allowed to give the CROONIAN lecture for the present year, as it would afford me an opportunity of weighing with impartiality the facts already ascertained, and of endeavouring by my own labours to add to their number.

In prosecuting this inquiry, I consider myself to have been particularly fortunate in having had the assistance of my friend Mr. RAMSDEN. It was a subject connected with his own pursuits, and one which had always engaged his attention; he was therefore peculiarly fitted, both by his own ingenuity and knowledge in optics, for such an investigation.

In conversing upon the different uses of the crystalline humour, he made the following observations.

He said, that as the crystalline humour consists of a substance of different densities, the central parts being the most compact, and from thence diminishing in density gradually in every direction, approaching the vitreous humour on one side, and the aqueous humour on the other, its refractive power becomes nearly the same with that of the two contiguous substances. That some philosophers have stated the use of the crystalline humour to be, for accommodating the eye to see objects at different distances; but the firmness of the central part, and the very small difference between its refractive power near the circumference and that of the vitreous, or the aqueous humour, seemed to render it unfit for that purpose; its principal use rather appearing to be for correcting the aberration arising from the spherical figure of the cornea, where the principal part of the refraction takes place, producing the same effect that in an achromatic object glass we obtain in a less perfect manner, by proportioning the radii of curvature of the different lenses. In the eye, the correction seems perfect, which in the object glass can only be an approximation, the contrary aberrations of the lenses not having the same ratio; so that if this aberration be perfectly corrected at any given distance from the centre, in every other it must be in some degree imperfect.

Pursuing the same comparison: In the achromatic object glass, we may conceive how much an object must appear fainter from the great quantity of light lost by reflection at the surfaces of the different lenses, there being as many primary reflections as there are surfaces; and it would be fortunate if this reflected light was totally lost. Part of it is again reflected towards the eye by the interior surfaces of the lenses, which by diluting the image formed in the focus of the object glass, makes that image appear far less bright than it would otherwise have done, producing that milky appearance so often complained of in viewing lucid objects through this sort of telescope.

In the eye the same properties that obviate this defect, serve also to correct the errors from the spherical figure, by a regular diminution of density from the centre of the crystalline outward. Every appearance shews the crystalline to consist of laminæ of different densities; and if we examine the junction of different media, having a very small difference of refraction, we shall find that we may have a sensible refraction without reflection: now if the difference between the contiguous media in the eye, or the laminæ in the crystalline, be very small, we shall have refraction without having reflection, and this appears to be the state of the eye; for although we have two surfaces of the aqueous, two of the crystalline, and two of the vitreous humour, yet we have only one reflected image, and that being from the anterior surface of the cornea, there can be no surface to reflect it back, and dilute an image on the retina.

This hypothesis may be put to the test, whenever accident shall furnish us with a subject having the crystalline extracted from one eye, the other remaining perfect in its natural state; at the same time we may ascertain whether or no the crystalline is that part of the organ, which serves for viewing objects at different distances distinctly. Seeing no reflection at the surface of the crystalline, might lead some persons to infer that its refractive power is very inconsiderable, but many circumstances shew the contrary; yet what it really is may be readily ascertained, by having the focal length and distance of a lens from the operated eye, that enables it to see objects the most distinctly; also the focal length of a lens, and its distance from the perfect eye that enables it to see objects at the same distance as the imperfect eye; these data will be sufficient, whereby to calculate the refractive power of the crystalline with considerable precision.

Again, having the spherical aberration of the different humours of the eye, and having ascertained the refractive power of the crystalline, we have data from whence to determine the proportional increase of its density as it approaches the central part, on a supposition that this property corrects the aberration.

These observations of Mr. RAMSDEN respecting the use of the crystalline lens, I was very desirous of bringing to the proof;

and while my mind was strongly impressed by them, a favourable opportunity occurred. A young man came into St. George's hospital with a cataract in the right eye: this proved to be a fair case for an operation, to which the man very cheerfully submitted, and was put under my care for that purpose.

In performing the operation, the crystalline lens was very readily extracted, and the union of the wound in the cornea took place unattended by inflammation, so that the eye suffered the smallest degree of injury that can attend so severe an operation; these circumstances it is proper to mention, as they contributed to render the patient a more favourable subject for experiment.

The man's name was BENJAMIN CLERK; he was a seafaring man, 21 years of age, and in perfect health. Both his eyes were free from complaint till about the 11th of April, 1793, at which time he was on a voyage home from the East Indies, a sudden mist or dimness appeared before his right eye; this increased very rapidly, and on the 18th of the same month the sight was entirely obscured. The crystalline humour was extracted on the 25th of November; and 27 days after the operation the eye was so far recovered as to admit of the following observations and experiments being made upon it.

In this man we had all the circumstances combined, which seemed to be required to determine how far the crystalline lens was the principal agent in adjusting the eye. The man himself was in health, young, intelligent, and his left eye perfect; the other had been an uncommonly short time in a diseased state, and appeared to be free from every other defect but the loss of the crystalline lens. He very willingly allowed me to make the following experiments on him; and remained in town, although inconvenient to himself, till they were completed; the greater part of them were instituted by Mr. RAMSDEN, and all of them carried through under his direction.

The experiments were begun on the 22d of December, 1793, at which time the following observations were made upon the imperfect eye. The eye bore the light of the day very well; but was fatigued by strong sunshine, or the glare of candle-light. In weak lights objects were not seen at all by the imperfect eye, but in strong lights they presented a faint image, which appeared at the same distance with that seen by the perfect eye, and close to it, or nearly so, but always to the left.

The imperfect eye, unassisted by glasses, could see objects, but it was with a degree of indistinctness; and this indistinct vision only took place at a distance between six and nine inches. With a double convex glass, the radius of one surface an inch and an half, of the other six inches, the flat side towards the eye, having a focus of $2\frac{1}{4}$ inches, objects appeared most distinct at $4\frac{1}{2}$ inches, and the extremes were $2\frac{1}{2}$ inches, and $5\frac{1}{2}$ inches. The different distances were ascertained by placing one end of a foot rule against the man's forehead, and giving him the book in his own hand, desiring him to carry it to the distance at which he saw best, and afterwards to the two extremes of distinct vision, the upper end of the book being always in contact with the rule; so that the moment he adjusted the book, the distance was read off from the scale. The accuracy with which he brought it to the same point in repeating the experiments, proved his eye to be uncommonly correct; for as he did not himself see the scale, there could be no source of fallacy.

Making these experiments fatigued the eye considerably, and repeating them after very short intervals made the eye water, and gave a slight degree of pain; this, however, soon went off.

In looking at objects through this glass, the image was free from any tinge of colour, unless he directed his eye towards the circumference of the glass, and then it had a considerable tinge, which evidently arose from the prismatic figure of that part of the glass.

A comparative experiment was made upon the perfect eye, with a glass of 15 inches focus. Objects were found in one experiment to appear most distinct at $8\frac{1}{2}$ inches, the extremes 3 inches and 11 inches; in another, most distinct at 7 inches, the extremes as before, 3 and 11 inches.

On the 29th of December, 34 days after the operation, the following experiments were made by candle-light, about six o'clock in the evening.

The experiment with the double convex glass was repeated, the aperture being diminished to $\frac{3}{20}$ of an inch; objects appeared most distinct at 5 inches, the extremes 3 inches and $7\frac{3}{4}$ inches. The aperture was diminished to $\frac{3}{40}$ of an inch, and vision appeared most distinct at 5 inches, the extremes $3\frac{1}{2}$ inches and 7 inches. When the aperture was reduced to $\frac{1}{20}$ of an inch, the inflexion of the rays produced the appearance of a speck, which obscured his vision.

By diminishing the aperture, spherical aberration was in a great measure corrected, and vision rendered more distinct.

A plano-convex glass of $2\frac{7}{8}$ inches focus, with the plane towards the eye, was now applied, and the objects were most distinct at 6 inches, but by no means well defined: the aperture was now reduced to $\frac{4}{30}$ of an inch, and objects appeared much more distinct at $5\frac{1}{2}$ inches; when the glass was brought within half an inch of the eye, objects were still more distinct, and were seen at 5 inches.

The eye was less affected by these than the former experiments, nor was it fatigued by the light of the candle. In strong lights a faint image was seen by the imperfect eye, and always to the left of the other.

The perfect eye, with a glass of 15 inches focus, saw objects most distinctly at $8\frac{1}{2}$ inches, the extremes $3\frac{1}{2}$ inches and $11\frac{1}{4}$ inches.

As these experiments were made with a view to determine whether the eye, when deprived of its crystalline humour, had a power of adjusting itself to different distances; that being ascertained, they were not prosecuted further, on account of the tender state of the man's eye, who went into the country as soon as they were completed.

On the 4th of November, 1794, the man returned to London, and submitted himself to be the subject of further experiments. This afforded us an opportunity of ascertaining the comparative adjustment of the two eyes, when by means of different glasses they were brought to see distinctly at nearly the same focal distance: an experiment we had been unable to make before for want of proper glasses.

Sir HENRY ENGLEFIELD, who will be found to have given us his assistance in the subsequent part of this investigation, was present at this experiment, and was much astonished, as we had been in the former ones, at the accuracy with which the man's eye was adjusted to the same distance in the repeated trials that were made with it.

The perfect eye, with a glass of $6\frac{1}{2}$ inches focus, had distinct vision at 3 inches; the near limit was $1\frac{7}{8}$ inch, the distant one less than 7 inches.

The imperfect eye, with a glass $2\frac{2}{10}$ inches focus, with an aperture $\frac{3}{40}$ of an inch, had distinct vision at $2\frac{7}{8}$ inches, the near limit $1\frac{7}{8}$ inch, the distant one 7 inches.

From the result of this experiment we find that the range of adjustment of the imperfect eye, when the two eyes were made to see at nearly the same focal distance, exceeded that of the perfect eye.

These experiments were made by Mr. RAMSDEN, who took particular care to avoid every thing that might be productive of error or deception; and repeated them several times before any conclusions were drawn from them. Several others were made on the same subject, but as they only tended to confirm those already mentioned, it would be taking up the time of this learned Society unnecessarily to detail them.

It may be proper to mention a reason which suggested itself to Mr. RAMSDEN, why the point of distinct vision of the imperfect eye appeared to the man himself nearer than it was in reality; it arose from his judging of distinctness by the legibility of the letters, which were easier read when they subtended a greater angle (from the imperfection of his eye) than at his real point of distinct vision.

The result of these experiments convinced us that the internal power of the eye, by which it is adjusted to see at different distances, does not reside in the crystalline lens; we were also satisfied by the facts and arguments adduced in Mr. HUNTER's letter on this subject, published in the first part of the last volume of the Philosophical Transactions, that it does not arise from a change in the general form of the globe of the eye; we therefore abandoned both of these theories.

It suggested itself that any change in the curve of the cornea (could it be produced), would vary the refraction of the rays, so as considerably to alter the focus of the eye; and upon considering this subject, Mr. RAMSDEN made a rough calculation, from which it appeared, that a very small alteration in that part would vary the adjustment of the eye from parallel rays to its shortest distance of distinct vision.

This opened to us a new field of inquiry, and I endeavoured to ascertain how far the cornea admitted of such a change, and if it did, how far that change operated in producing this particular effect.

For the first of these purposes I made the following experiments in the presence of Mr. RAMSDEN.

A portion of the cornea $\frac{1}{8}$ of an inch broad, and $\frac{11}{20}$ of an inch long, was removed from the eye of a person 40 years of age, two days after death, with a part of the sclerotic coat on each side attached to it. This was laid upon a piece of glass immersed in water, under which was a scale divided into very minute parts, these divisions being very readily seen through the glass. One end of the cornea was made fast by fixing the sclerotic coat, and a force was applied to the other; this power was found capable of elongating the cornea $\frac{1}{20}$ part of an inch; and on removing it, the cornea recovered itself to its original length. In different trials it varied in the quantity of elongation, but in all of them it was fully $\frac{1}{11}$ part of the whole length, or diameter of the cornea.

The elasticity of the cornea being thus ascertained, encouraged me to proceed in the anatomical investigation; and

I was desirous of determining more exactly than had hitherto been done, the precise insertion of the tendons of the four straight muscles of the eye, so as to know whether their action could be extended to the cornea or not.

In dissecting these muscles to their termination, I found that they approached within $\frac{1}{8}$ of an inch of the cornea, before their tendons became attached to the sclerotic coat upon which they lay; it was evident that they did not terminate at this part, but were so united as to be difficultly separated by dissection; I therefore endeavoured by gentle force to pull them asunder, as in that way the parts would separate in the direction of their fibres. In doing this, they not only admitted of separation to the edge of the cornea, but brought away a lamina of the cornea with them. I thought this would be better seen in an eye after putrefaction had begun to take place, but found that in that state it could scarcely be demonstrated; while in the recent eye the whole of the external lamina of the cornea could be brought away along with the four straight muscles, leaving the surface underneath uniform, but without polish, and upon the same plane with the sclerotic coat, of which it was a continuation.

As this was a new fact, and a very important one, shewing a connection between these muscles and the cornea, I have dried the parts, and preserved them in that state, to shew the mode in which the tendons of the straight muscles are lost in the cornea, giving it the appearance of a central tendon.

The cornea from this investigation is proved to be composed of two laminæ, the external a continuation of the tendons of the four straight muscles, the other a continuation of the sclerotic coat, and the uniting medium between them is not unlike very fine cellular membrane.

If the cornea is examined at its attachment to the sclerotic coat and tendons of the straight muscles, it appears to be of exactly the same thickness with those parts, but grows thicker towards the centre; this increase of thickness is principally in the external lamina, for when that is removed, the other appears equally so through its whole extent.

To ascertain that the cornea is really thickest in the middle, I made a transverse section of it, and Mr. RAMSDEN, with several other gentlemen, examined the cut edge through a magnifying glass, and all of them were satisfied with the fact of the central part being evidently thicker than that which was nearer to the circumference.

It is necessary to mention, that in stretching the cornea the central part yields most readily to the power applied; this is so much the case, that if the cut edge of the cornea is examined while it is several times drawn out and allowed to contract again, the change in the centre will be found the most distinct; the principal elasticity appearing to reside in that part.

Before these experiments were made upon the cornea, Mr. RAMSDEN had promised me that he would contrive an instrument by which the cornea might be examined, while the eye was adapting itself to different distances; so as to enable us to decide whether any change took place at these times in its external figure.

When I state to this learned Society, that seven months elapsed before the apparatus for this experiment was completed, they will not attribute it to a want of solicitude on my part, or a want of attention in Mr. RAMSDEN; but to delays which must necessarily occur to an artist so extensively employed in business, and at the same time so ready to engage both from inclination, and the urgent requests of his friends, in promoting philosophical inquiries.

On the 31st of July, 1794, we were enabled to begin our experiments, for which the following apparatus was constructed.

A thick board was fixed to a strong upright support, directly opposite to the window of Mr. RAMSDEN's front room on the first floor, which looks up Sackville-street, at the distance of one foot from the window. In this board was a square hole, large enough to admit a person's face, the forehead and chin resting against the upper and lower bars, and the cheek against either of the sides, so that when the face was protruded, the head was steadily fixed by resting on three sides, and in this position the left eye projected beyond the outer surface of the board.

On the outside of the board, or that next the window, upon the left of the square hole, was fixed a microscope, so placed as to take into its field the lateral part of the front of the cornea, which projects beyond the eyelids. The microscope had not only a movement directly forwards, but by means of endless screws, had also a vertical and horizontal motion, without which the experiments could not have been made with any degree of precision.

From the upper part of the square hole an horizontal brass beam projected towards the window, with joints, by which it could be lengthened or shortened, and at the end of this a brass plate was suspended, which admitted of being raised or depressed, so as to bring a small hole that had been drilled through it directly opposite to the eye.

With this apparatus we began our experiments; and I consider it as a fortunate circumstance that Sir HENRY ENGLEFIELD arrived in town the night before they were made; he very cheerfully gave us his assistance the moment I made the request.

Sir HENRY, from his practical knowledge of mathematical instruments, and the habit of making observations with them, rendered us very material assistance in the course of our experiments, and I feel myself obliged to him for remaining in town till they were completed. To Mr. RAMSDEN and myself it was a particular satisfaction to have an evidence who had no presupposed opinion, therefore impartial; whose knowledge of the subject enabled him to form a judgment of the results, and to correct any error we might fall into in conducting the experiments. This circumstance will also give to the experiments an additional claim upon the notice of this learned Society.

The first experiment was made at three o'clock, at which were present Sir HENRY ENGLEFIELD, Mr. RAMSDEN, and myself. It required some time, and considerable ability, in which I can claim no part, to adjust the microscope, and bring the cornea into its field; when this was done, the appearances were so different from what were expected, that we had a difficulty in recognizing the object; all that could be seen was 4 curved lines, but even these were rendered confused by reflections from the cross bars of the sash of the window. Upon throwing up the sash, the curved lines became very distinct, and that which appeared the inner one in the microscope, was ascertained to be the convex projecting surface of the cornea.

This being determined, the person whose eye was the object of the experiment was desired to look at the corner of a chimney at the upper end of Sackville-street, a distance of 235 yards, through the hole in the brass plate, and afterwards to look at the edge of the small hole itself, which was only 6 inches from the eye. In doing this several times, the curved lines were seen to separate from each other; and the microscope required being withdrawn from the object whenever the person's eye was adjusted to the near distance; but the very reverse took place when it was fixed on the distant one.

In making these experiments, the least motion of the head carried the cornea out of the field of the microscope; it was therefore necessary that the two objects should be exactly in the same line respecting the eye, and that the person should remain silent. When he complied with any request which had been made, he signified by touching the knee of the observer with his hand, that he had done so. This experiment was made upon the eyes of all present, and the same appearances were uniformly observed; and after several trials we became so familiar with the appearances, that the observer only required information of the adjustment having been changed, to enable him to tell which of the objects the eye was fixed upon.

August the 1st, about four o'clock, these experiments were repeated, and after several attempts were made, without success, to explain the cause of the curved lines, we found it necessary to shade a part of the window, to take off the glare of light which fatigued the eye, and rendered it unsteady; this made the curved lines less distinct; and when the whole window was shaded they disappeared altogether, leaving a very distinct view of the whole thickness of the cornea, with a well defined line formed by its anterior projecting surface. This discovery proved the curved lines to be reflections from the sides of the window upon the cornea; but as it was not made till six o'clock, we were obliged to postpone any further observations upon it.

August the 3d, at seven o'clock in the morning, Mr. RAMSDEN and myself resumed our experiments, Sir HENRY ENGLEFIELD being unable to attend at that hour. The eye of the person under observation was shaded from the light by shutting the half of the window-shutter directly before it, and to direct the sight to pass through it, a hole was bored in the shutter; the other half of the shutter was turned back, so as to take off the side light, only letting in enough to illuminate the cornea; in this state the cornea was very distinctly seen, and the former experiments were repeated upon it, with a micrometer wire in the focus of the eye-glass, so placed as accurately to oppose the anterior edge of the cornea.

The motion of the cornea became now perfectly distinct; its surface remained in a line with the wire when the eye was adjusted to the distant object, but projected considerably beyond it when adapted to the near one; and the space through which it moved was so great as readily to be measured by magnifying the divisions upon a scale, and comparing them; in this way we estimated it at the 800 part of an inch, a space distinctly seen in a microscope magnifying 30 times.

It may not be improper, for the sake of accuracy, to mention that the hole made in the window-shutter did not admit of seeing up Sackville-street, so that the distant object was now only at 90 feet, which is rather less than is necessary for parallel rays; a circumstance, so far as it can be considered, in favour of the experiment, as a more distant object must have increased the effect upon the cornea. Having satisfied ourselves fully respecting the result of this experiment, we desisted from further trials.

At twelve o'clock of the same day, we prevailed on Sir HENRY ENGLEFIELD to make the experiment on my eye, without giving him any information of the observations that had been made in the morning. He was very much struck with the distinctness of the cornea; and told me without difficulty the different objects to which my eye was adjusted, and was as fully satisfied as either Mr. RAMSDEN or myself with the result of the experiment.

Mr. RAMSDEN now made the same experiment on Sir HENRY's eye, but was unable to retain it in the field of the microscope; the motion of the cornea was always in one direction, and very irregular; after repeated trials, equally unsatisfactory, the eye became so fatigued that he was obliged to desist.

August the 4th, Mr. RAMSDEN repeated the experiment on Sir HENRY's eye, to ascertain if possible the cause of his former want of success, and found the same circumstances again take place; the curve of the cornea moved always in the same direction, never returning to the wire. This could not be accounted for, till it was accidentally discovered to arise from the motion of his hand in touching the knee of the observer, for when that was omitted, the experiment was followed by the same results as those made on the rest of the company. I have been more particular in mentioning this circumstance, as it shows that the most trifling things may interfere with the result of the experiment, and that it required a considerable degree of nicety and management in adjusting the instrument, without which the experiment could not have been made.

August the 28th, the former experiments were repeated by Sir HENRY ENGLEFIELD, Mr. RAMSDEN, and myself, upon the eye of a young lad, and the result was similar to the others, the motion of the cornea was uncommonly distinct. Sir HENRY now became the subject of the experiment, and changed the adjustment of his eye from one distance to another in a very irregular manner, without giving the smallest information, with a view to embarrass Mr. RAMSDEN who was the observer, but without effect, for Mr. RAMSDEN was able to tell every change in distance he had made, without a single mistake; this exceeded our expectation, and appeared to us so satisfactory that we required no further proofs of the truth of our former observations.

Before we concluded our experiments, every mode that could be devised was put in practice to see how far there might be any deception; the eye was moved upon its axis, and in different directions, but these motions did not give at all similar appearances to those seen in the adjusting of the eye to different distances.

From the different experiments which I have had the honour to lay before this learned Society, I shall consider the following facts to have been ascertained.

1st, That the eye has a power of adjusting itself to different distances when deprived of the crystalline lens; and therefore the fibrous and laminated structure of that lens is not intended to alter its form, but to prevent reflections in the passage of the rays through the surfaces of media of different densities, and to correct spherical aberration.

2d, That the cornea is made up of laminæ; that it is elastic, and when stretched, is capable of being elongated $\frac{1}{11}$ part of its diameter, contracting to its former length immediately upon being left to itself.

3d, That the tendons of the four straight muscles of the eye are continued on to the edge of the cornea, and terminate, or are inserted, in its external lamina; their action will therefore extend to the edge of the cornea.

4th, That in changing the focus of the eye from seeing with parallel rays to a near distance, there is a visible alteration produced in the figure of the cornea, rendering it more convex; and when the eye is again adapted to parallel rays, the alteration by which the cornea is brought back to its former state is equally visible.

Having supported these facts by the evidence of anatomical structure, and absolute demonstration, I shall consider them to be established; and make some observations upon the muscular and elastic power by which so very curious an effect as the adjustment of the eye is produced.

The four straight muscles of the eye are attached to the bottom of the bony orbit near the foramen opticum; they become broader as they pass forward, and when arrived at the anterior part of the eye-ball, are insensibly changed for tendons; these adhere to the sclerotic coat, and terminate in the external lamina of the cornea, which appears to be a continuation of them.

When we consider the situation of these muscles, it is evident that their action will produce three very different effects upon the eye, according to circumstances. When they act separately, they will move the eye in different directions; when together, with only a small quantity of contraction, they will steady the eye-ball; and when this is increased they will compress the lateral and posterior parts of the eye. This compression of the eye will force the aqueous humour forwards against the centre of the cornea, while the circumference is steadied by the muscles, so that the radius of curvature of the cornea will be rendered shorter, and its distance from the retina increased.

That the eye-ball cannot be made to recede in the orbit by any of these actions, is sufficiently proved by its not having done so in any of the experiments.

These muscles are uncommonly large, and come much further forward than appears necessary for the purposes generally assigned to them; but when applied to so important an office as that we have just stated, their size, and anterior insertion, are easily explained.

It may be imagined that I have allotted to these muscles a greater variety of uses than is compatible with the simplicity of the general laws of the animal œconomy; but to prove this not to be the case, I shall only bring the biceps flexor cubiti as an instance of a similar kind. That muscle is attached to the scapula by both its heads, one of which passes through the joint of the shoulder, they afterwards unite, and their common tendon is inserted into the radius; when the muscle contracts, the first effect will be to steady the joint of the shoulder; if the contraction is increased, it will rotate the radius, and if still more increased, bend the fore-arm.

There are many instances in animal bodies of elasticity being substituted for muscular action, but this in the eye is by much the most beautiful of those applications.

In the vascular system the arteries are composed of muscular fibres, and an elastic substance; in the natural easy state of the circulation, the re-action in the larger vessels is principally the effect of elasticity, but when increased, it is the effect of muscular contraction.

The claws of the lion are drawn up, and supported from the ground, by means of elastic ligaments; but they are brought down for use, which is an action not so often required, by muscles.

In the adjustment of the eye it is the same; the state fitted for parallel rays is the effect of elasticity, but that for nearer distances, which is less frequently wanted, is the effect of muscular action.

In these different instances, the intention is uniformly to avoid the expence of muscular action whenever the effect can be produced in any other way, as muscular actions consume a considerable quantity of blood, which is the nourishment of the body.

That the adjusting the eye to near distances is the effect of an action, or exertion, was very evident to every gentleman concerned in these experiments. In changing the focus of our eyes, we were much astonished, particularly Sir HENRY ENGLEFIELD, at the exertion required to adjust the eye to the near distances, and the facility with which it was adapted to distant ones; the first was a strain upon the eye, the second appeared a relief to it.

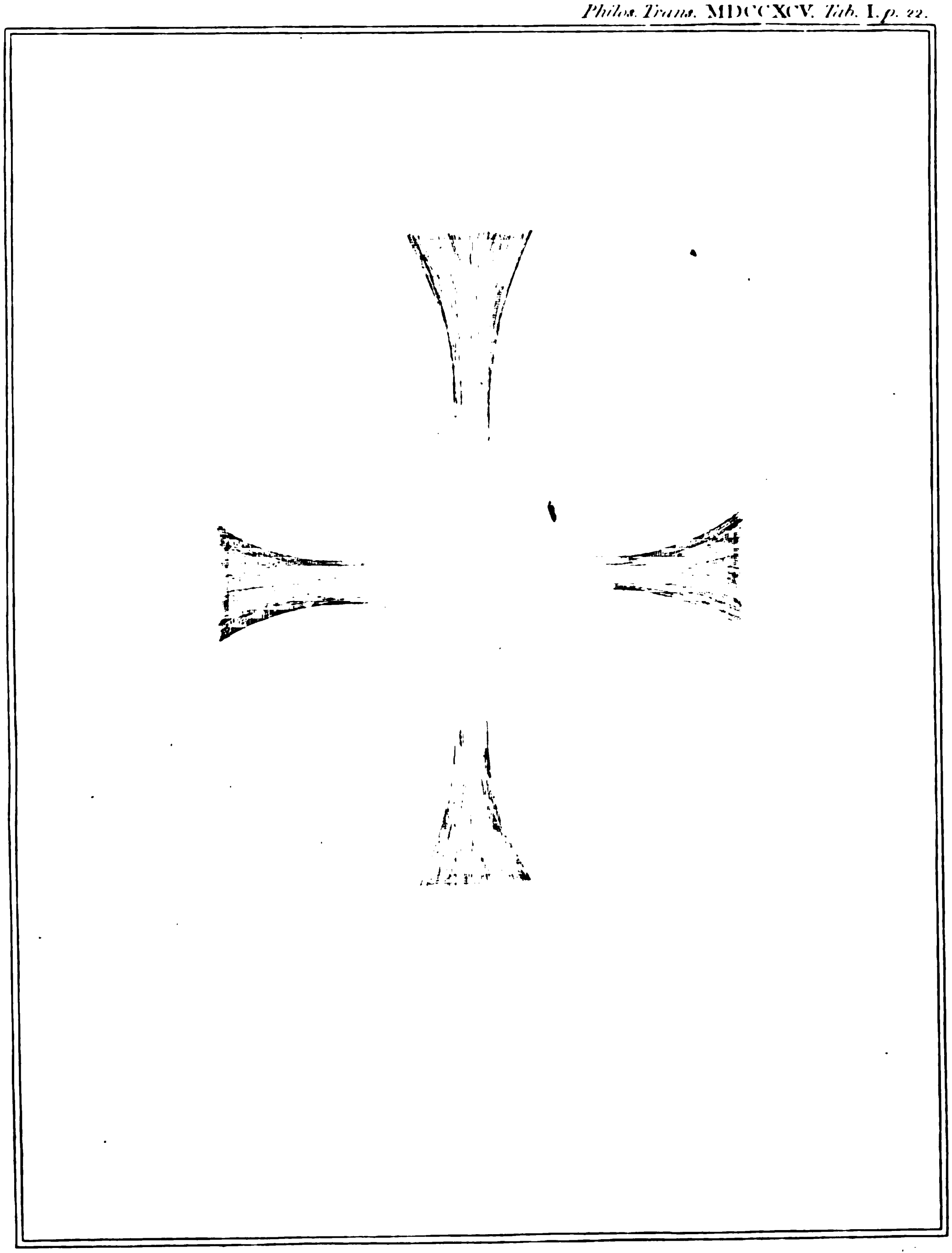
When the eye was intent upon the near object, it required the attention to be constantly kept up, or the object became indistinct; and if we looked at it beyond a certain time, the eye was so much fatigued as to lose it at intervals. This corresponds with other muscular actions, for whenever muscles are kept long in one state they begin to vibrate involuntarily.

These circumstances explain what may be called a *coup d'œil*, or the distinctness with which an object is seen when the eye is first fixed upon it. This arises from the nice adjustment produced by the muscles when first thrown into action, which they cannot keep up, being unable to remain long in the same state; nor can they, after having been used for any time, return to this adjustment with the same exactness.

The change that takes place in the eye at an advanced period of life, by which it loses its adjustment to very near, and very distant objects, does not arise from any defect in the muscles, as might at first be imagined, since that would not account for the eye being unable to see with parallel rays; nor is there any obvious reason why these muscles should lose their powers, while others, which are not apparently so strong, if we may judge by their effects, retain their full action long after the eye has undergone this change.

This defect in the eye I am led to believe is brought on by the cornea losing its elasticity as we advance in life, neither contracting nor being elongated to its usual extent, but remaining in a middle state. That elastic substances in the body do undergo such a change may be well illustrated in the vascular system. The aorta is composed almost entirely of elastic substance, and there is probably no part of the body, at an advanced age, which is so often found to have lost its natural action; it appears to undergo a change from age alone, becoming inelastic, and then taking on diseases of different kinds, as being ossified, or becoming aneurismal; but in neither of these diseases is it found to be contracted, although often the reverse, and when disease has not supervened, the artery more commonly remains in the middle state.

The cornea having similar properties must be liable to a similar change, but its action being less constant, and the power which it is to resist being weaker, the change will be probably more gradual and less in degree, but sufficient to



account for the alteration we find in the focus of the eyes of old people.

There are many other circumstances respecting vision, and many which occur in disease, that may be explained by a knowledge of these facts; but as this lecture is only intended to establish the facts themselves, in doing which I have already taken up too much of the time of this learned Society, I shall at some future period consider their application to the phænomena of vision in health, and disease.

EXPLANATION OF THE PLATE. (Tab. I.)

Portions of the four straight muscles of the eye, with their tendons insensibly lost in the external lamina of the cornea, stretched out and dried. The tendons become broader as they approach the cornea, and form a circle of which the cornea appears to be a continuation.

II. *The* Bakerian *Lecture. Observations on the Theory of the Motion and Resistance of Fluids; with a Description of the Construction of Experiments, in order to obtain some fundamental Principles. By the Rev.* Samuel Vince, *A. M. F. R. S.*

Read November 27, 1794.

HOWEVER satisfactory the general principles of motion may be, when applied to the action of bodies upon each other, in all those circumstances which are usually included in that branch of natural philosophy called MECHANICS, yet the application of the same principles in the investigation of the motions of FLUIDS, and their actions upon other bodies, is subject to great uncertainty. That the different kinds of airs are constituted of particles endued with repulsive powers, is manifest from their expansion when the force with which they are compressed is removed. The particles being kept at a distance by their mutual repulsion, it is easy to conceive that they may move very freely amongst each other, and that this motion may take place in all directions, each particle exerting its repulsive power equally on all sides. Thus far we are acquainted with the constitution of these fluids; but with what absolute degree of facility the particles move, and how this may be affected under different degrees of compression, are circumstances of which we are totally ignorant. In respect to those fluids which are denominated liquids, we are still less acquainted with their nature. If we suppose their particles to be in contact, it is extremely difficult to conceive how they can move amongst each other with such extreme facility, and produce effects in directions opposite to the impressed force without any sensible loss of motion. To account for this, the particles are supposed to be perfectly smooth and spherical. If we were to admit this supposition, it would yet remain to be proved how this would solve all the phænomena, for it is by no means self-evident that it would. If the particles be not in contact, they must be kept at a distance by some repulsive power. But it is manifest that these particles attract each other, from the drops of all perfect liquids affecting to form themselves into spheres. We must therefore admit in this case both powers, and that where one power ends the other begins, agreeable to Sir ISAAC NEWTON's * idea of what takes place not only in respect to the constituent particles of bodies, but to the bodies themselves. The incompressibility of liquids (for I know no decisive experiments which have proved them to be compressible) seems most to favour the former supposition, unless we admit, in the latter hypothesis, that the repulsive force is greater than any human power which can be applied. The expansion of water by heat, and the possibility of actually converting it into two permanently elastic fluids, according to some late experiments, seem to prove that a repulsive power exists between the particles; for it is hard to conceive that heat can actually create any such new powers, or that it can of itself produce any such effects. All these uncertainties respecting the constitution of fluids must render the conclusions deduced from any *theory* subject to considerable

* See his Optics, Que. 31.

errors, except *that* which is founded upon such experiments as include in them the consequences of all those principles which are liable to any degree of uncertainty.

A fluid being composed of an indefinite number of corpuscles, we must consider its action, either as the joint action of all the corpuscles, estimated as so many distinct bodies, or we must consider the action of the whole as a mass, or as one body. In the former case, the motion of the particles being subject to no regularity, or at least to none that can be discovered by any experiments, it is impossible from this consideration to compute the effects; for no calculation of effects can be applied when produced by causes which are subject to no law. And in the latter case, the effects of the action of one body upon another differ so much, in many respects, from what would be its action as a solid body, that a computation of its effects can by no means be deduced from the same principles. In mechanics no equilibrium can take place between two bodies of different weights, unless the lighter acts at some mechanical advantage; but in hydrostatics, a very small weight of fluid may, without its acting at any mechanical advantage whatever, be made to balance a weight of any magnitude. In mechanics, bodies act only in the direction of gravity; but the property which fluids have of acting equally in all directions, produces effects of such an extraordinary nature as to surpass the power of investigation. The indefinitely small corpuscles of which a fluid is composed, probably possess the same powers, and would be subject to the same laws of motion, as bodies of finite magnitude, could any two of them act upon each other by contact; but this is a circumstance which certainly never takes place in any of the aerial fluids, and probably not in any liquids. Under the circumstances, therefore, of an indefinite number of bodies acting upon each other by repulsive powers, or by absolute contact under the uncertainty of the friction which may take place, and of what variation of effects may be produced under different degrees of compression, it is no wonder that our theory and experiments should be so often found to disagree.

Sir ISAAC NEWTON seems to have been well aware of all these difficulties, and therefore in his PRINCIPIA he has deduced his laws of resistance, and the principles upon which the times of emptying vessels are founded, entirely from experiment. He was too cautious to trust to theory alone, under all the uncertainties to which he appears to have been sensible it must be subject. He had, in a preceding part of that great work, deduced the general principles of motion, and applied them to the solution of problems which had never before been attempted; but when he came to treat of fluids, he saw it was necessary to establish his principles upon experiments; principles, not indeed mathematically true, like his general principles of motion before delivered, but, under certain limitations, sufficiently accurate for all practical purposes.

The principle to be established in order to determine the time of emptying a vessel through an orifice at the bottom, is the relation between the velocity of the fluid at the orifice and the altitude of the fluid above it. Most writers upon this subject have considered the column of fluid over the orifice as the expelling force, and from thence some have deduced the velocity at the orifice to be that which a body would acquire in falling down the *whole* depth of the fluid; and others that acquired in falling through *half* the depth, without any regard to the magnitude of the orifice; whereas it is manifest from experiment, that the velocity at the orifice, the depth of the fluid being the same, depends upon the proportion which the magnitude of the orifice bears to the magnitude of the bottom of the vessel, supposing, for instance, the vessel to be a cylinder standing on its base; and in all cases the velocity, *cæteris paribus*, will depend upon the ratio between the magnitude of the orifice and that of the surface of the fluid. Conclusions thus contrary to matter of fact show, either that the principle assumed is not true, or that the deductions from it are not applicable to the present case. The most celebrated theories upon this subject are those of D. BERNOUILLI and M. D'ALEMBERT; the *former* deduced his conclusions from the principle of the *conservatio virium vivarum*, or as he calls it, the *equalitas inter descensum actualem ascensumque potentialem*, where by the *descensus actualis* he means the actual descent of the centre of gravity, and by the *ascensus potentialis* he means the ascent of the centre of gravity, if the fluid which flows out could have its motion directed upwards; and the *latter* from the principle of the *equilibrium* of the fluid. This principle of M. D'ALEMBERT leads immediately to that assumed by D. BERNOUILLI, and consequently they both deduce the same fluxional equation, the fluent of which expresses the relation between the velocity of the fluid at the orifice, and the perpendicular altitude of the fluid above it. How far the principles here assumed can be applied in our reasoning upon fluids, can only be determined by comparing the conclusions deduced from them with experiments.

The fluxional equation above mentioned cannot in general be integrated, and therefore the relation between the velocity of the fluid at the orifice and its depth cannot from thence be determined in all cases. If the magnitude of the orifice be indefinitely less than that of the surface of the fluid, the equation gives the velocity of the effluent fluid to be equal to that which a body would acquire by falling *in vacuo* through a space equal to the depth of the fluid. But the velocity here determined is not that at the orifice, but at a small distance from the orifice; for the fluid flowing to the orifice contracts the stream, and the velocity being inversely as the area of the section, the velocity continues to increase as long as the stream, by the expelling force of the fluid, keeps diminishing, and when the stream ceases to be contracted by that force, at that section of the stream called the *vena contracta,* the velocity is that which a body would acquire in falling through a space equal to the depth of the fluid. If, therefore AB *cd* EF (Tab. II. fig. 1.) be the vessel, *cd* the orifice, *c m n d* the form of the stream till it comes to the *vena contracta,* then this investigation supposes AB *c m n d* EF to be the form of the vessel, and *m n* the orifice, the fluid flowing through *c m n d* just as if the vessel were so continued. But as the proposition is to find the velocity of the fluid going out of the vessel, it may perhaps appear an arbitrary assumption to substitute the orifice *m n* instead of *c d,* when no such a quantity as *m n* appears in the investigation. If, however, we grant that the expelling force must act without any diminution until the fluid comes to *m n,* it seems that from the principles here assumed we ought to substitute *m n* instead of *c d,* as otherwise we get the velocity generated by the action of only a part of the force. The conclusion here deduced agrees very well with experiment; but an application of the same principles to another case differs so widely from matter of fact, as to render it very doubtful how far the principles here applied can be admitted. And if we were to grant the application of the principles here assumed, so far as regards the determination of the velocity, yet the time of emptying a vessel can by no means be deduced from it.

In order to determine the time of emptying a vessel, we must know both the area of the orifice *c d*, and the velocity at that orifice. Now the theory gives only the velocity at *m n*; and as it gives not the ratio of *m n* to *c d*, the velocity at the orifice cannot be deduced from thence, and therefore we cannot find the time of emptying. No theory whatever has attempted to investigate the ratio of *mn* to *cd*; it is well known that that is only to be determined by an actual mensuration. When the orifice is very small, Sir ISAAC NEWTON found the ratio to be that of 1 to $\sqrt{2}$; when the orifice is larger, the ratio approaches nearer to that of equality. We cannot therefore, even in the most simple case, determine, by theory alone, the time in which a vessel will empty itself.

If A B C D (fig. 2.) be a vessel filled with a fluid, and a pipe *m n r s* be inserted at the bottom, *m n* being very small in respect to B C, then, according to the theory of D. BERNOUILLI, the fluid ought to flow out of the pipe at *r s* with the same velocity it would out of a vessel A L M D through the orifice *r s*. Now in this latter case, the velocity, according to his own principles, varies as the square root of L A, and therefore it varies in the same ratio in the former case; hence if the length *m r* of the pipe bears but a very small proportion to A B, the velocity with which the fluid flows out of the pipe will be very nearly equal to the velocity with which it would flow through an orifice at the bottom equal to $r\,s$ or $m\,n$, the pipe being supposed to be cylindrical. To find how far this conclusion agrees with experiment, I made a cylinder 12 inches deep, and at the bottom I made a small circular orifice, whose area was about the 130th part of the area of the bottom of the cylinder: I also put a cylindrical pipe into the bottom, whose internal diameter was exactly equal to that of the hole, and length 1 inch. Hence, according to the theory, the velocity of the fluid out of the pipe ought to be to the velocity out of the orifice as $\sqrt{13} : \sqrt{12}$, or as 26 : 25 nearly. But by experiment, the quantity of fluid which run through the pipe in 12″ (the vessel being kept full) was to the quantity which run through the orifice in the same time, very nearly in the ratio of 4 to 3, and consequently that ratio expresses the ratio of the velocities; a consequence totally different from that which the theory gives. I then took a vessel of a different base, but the same altitude, and altered the diameter of the orifice and pipe, still keeping them equal, and made the pipe only half an inch long; in this case the velocities, by the theory, ought to have been in the ratio of $\sqrt{12,5}$ to $\sqrt{12}$, or as 49 to 48 nearly; whereas by experiment the ratio of the velocities came out the same as before, that is, as 4 to 3 nearly. I then reduced the pipe to the length of a quarter of an inch, and in that case the velocity did not sensibly differ from that through the orifice. Upon examining the stream, in consequence of this great difference in the two cases, when the lengths of the pipes differed by so small a quantity, I found that in the latter case the stream did not fill the pipe, as it did in the former case, but that the fluid was contracted as when it run through the simple orifice. At what length of pipe the stream will cease to fill it, is a circumstance to which no theory has ever been applied, but the determination thereof must be a matter of experiment entirely.

I next inserted pipes of different lengths, and found that when the length of the pipe was equal to the depth of the vessel, the velocity of the effluent fluid by theory was to that by experiment as about 7 to 6; and by increasing the length of the pipe, the ratio approached nearer to that of equality. In long pipes, therefore, the difference between theory and experiment is not greater than what might be expected from the friction of the pipes, and other circumstances which may be supposed to retard the velocity.

If the pipe be conical, increasing downwards, the velocity, by theory, is still the same, and consequently the quantity run out will be in proportion to the magnitude of *r s*. As long as the expelling force can keep the tube full, this appears to be the case; but by increasing the orifice *r s*, the pipe will, at a certain magnitude, cease to be kept full; at what time this happens must depend entirely upon experiment. But if the pipe decrease, having its orifice *r s* equal to that of a cylindrical pipe of the same length, the velocity through the former appears, from the experiment I made, to be greater than through the latter in the ratio of 14 to 11.

If the pipe *m r* (fig. 3.) be inserted horizontally into the side of a vessel, the velocity at the orifice *r s*, by theory, is always in proportion to the square root of the altitude C D, the orifice being still supposed to be very small compared with the bottom of the vessel. By trying the experiment with pipes of different lengths and of the same diameter, beginning with the shortest and increasing them, it appears that the velocity first increases and then decreases; and this is a circumstance which has been before observed. If $r s$ be greater than C m, the quantity of fluid which flows out in a given time (the vessel being kept full) appears to be increased in proportion to the increase of $r s$, as long as the expelling force is able to keep the pipe full; but at what magnitude of $r s$ this effect ceases must be determined by experiment. If $r s$ be less than C m, the quantity which flows out is greater than if the pipe were cylindrical, and of the same diameter as $r s$.

The velocities of fluids spouting upwards through an orifice or pipe has not been considered by BERNOUILLI; but the following experiments will show the effects in this case. Let A B C D E F (Tab. II. fig. 4.) be a vessel filled with a fluid, r an orifice, x, y, z, three pipes each an inch long, having their tops on an horizontal line with the orifice; x is cylindrical, of the same diameter as that of the orifice; y is conical, increasing upwards, of the same diameter at the bottom as the orifice; z decreases upwards, of the same diameter at the top as the orifice. In 12″, the quantities which run out through the orifice and pipes x, y, z, (the vessel being kept full) were found to be in the ratio of 7, 9.4, 11.2 and 10.7. Hence the ratio of the velocities through the orifice and pipe x appears to be very nearly in the ratio of 3 to 4, agreeable to what was found to take place for an orifice and short pipe at the bottom. The quantity which run out of the pipe y increased by increasing the diameter at the top, in proportion to that area as nearly as could be ascertained, as long as the expelling force could keep it full; and a greater quantity run out of the pipe z than through the orifice. All this is agreeable to what was found to take place under similar circumstances when the orifice and pipes were inserted at the bottom. So far therefore as the theory can be applied when the fluid descends perpendicularly, it appears to be applicable also to the case when it spouts upwards.

At the bottom of the vessel A B C D (Tab. II. fig. 5.) having an orifice *r s*, I inserted a pipe *a x y z w v* conical at the top and cylindrical downwards from it, having the diameter of the cylindrical part equal to that of the orifice, and directly under it. I then stopped the orifice *s r* within, and filled the vessel, and expected, that as there was now no pipe immediately connected with the orifice, the fluid would form the *vena contracta* as if there was no pipe, and that the velocity at the orifice would be the same as through a simple orifice; whereas I found the velocity to be greater, very nearly in the ratio of $\sqrt{2}$ to 1, the length of the pipe being equal to the depth of the cylinder. It appears therefore to flow out with about the same velocity as if the pipe had been continued to the orifice. The fluid therefore must have flowed from the orifice in a cylindrical form, for the pipe was observed to be filled. I see no cause which could prevent the *vena contracta* from being formed. I then stopped the pipe at the bottom *y z*, and filled the vessel and pipe, and found the circumstances to be exactly the same.

In order to determine whether there was any pressure of the fluid against the sides of the pipes as it passed through in all their different situations, I pierced some small holes in them at different parts. In the cylindrical pipes, and those in the form of increasing cones, the fluid passed by the holes without being projected out, or without having the least tendency to issue through them; but in the decreasing cones the fluid spouted out at the holes. In the former cases therefore there was no pressure against the sides of the pipes, but in the latter case there was.

In respect to the motion of the fluid through any of the pipes, I found no difference whether I stopped the pipe at the end of the tube which enters into the vessel, in which case the motion began when the tubes were empty, or whether at the other end, in which case they were full at the commencement of the motion. That the fluid should flow into the top of the pipe faster than it would through an orifice, may probably, in part at least, be owing to the adhesion of the fluid to the pipe, and be thus explained. Although the horizontal motion of the fluid towards the orifice accelerates the velocity after it escapes from the vessel by contracting the stream, yet it must diminish the velocity at the orifice; that is, if the same perpendicular motion were to take place without the horizontal motion, the fluid would flow out faster; for as any motion in a fluid is immediately communicated in every direction, the horizontal motion will produce a motion upwards, and in some degree obstruct the descent of the fluid. If therefore this horizontal motion could be taken away, or any how diminished, the fluid would flow out with a greater velocity. Now if a pipe be fixed, the fluid at the bottom of the vessel flowing towards the orifice will, by its adherence to the vessel, continue to adhere to the sides of the pipe as soon as it arrives there, and by this means almost all the horizontal motion will be destroyed, and converted into a perpendicular motion, for the horizontal motion arises principally from the fluid which flows from and very near to the bottom, where the whole motion is very nearly in that direction. This motion therefore being thus nearly destroyed, the fluid will be less interrupted at the orifice, and consequently will flow out with a greater velocity. But why the velocity should also be increased either by increasing the length of the pipe, or making it an increasing cône, under certain limitations, is a circumstance which, I confess, I can give no satisfactory reason for.

The abovementioned experiments were made principally with a view to ascertain how far the theory of the motion of fluids can be applied; and the inquiry has led to several circumstances which, I believe, have not been observed before. That the theory is not applicable in all cases is manifest; but that it brings out conclusions in many instances which agree very well with experiment is undoubtedly true. This tends to show, either that the common principles of motion cannot be applied to fluids, and that the agreement is accidental; or that under certain circumstances and restrictions the application is just. Which of these is the case is not, perhaps, easy for the mind to satisfy itself about. Nothing however which is here said is done with any view to detract from the merit of these celebrated authors. They have manifested uncommon penetration, and carried their inquiries upon the subject to an extent, that nothing further can be hoped for or expected; and if they had done nothing else in science, this alone would have ranked them amongst the very first mathematicians. The fault has been *non artificis sed artis.*

Mr. MACLAURIN, in his Treatise on Fluxions, has given a most admirable illustration of the theory of Sir ISAAC NEWTON. It is there a very principal inquiry to determine the ratio of the force which generates the velocity of the descending surface of the fluid to the force of gravity. Now according to that theory, the pressure on the bottom of the vessel is wholly taken off at the instant of time at which the water begins to flow; and as this conclusion cannot be admitted, we may from hence learn, says the author, that this theory is not to be considered as perfectly exact. It appears therefore to be an important point to determine, what is the pressure of the fluid upon the bottom of a vessel compared with its whole weight at the time the fluid is running out. This may be determined to a great degree of accuracy by experiments constructed in the following manner.

Let A B C D (Tab. III. fig. 6.) be a pair of scales, and O the fulcrum; at the end of the arm C suspend a cylinder E, having an orifice *r s*, immediately under which place a weight *w*, so that the upper surface may be in the *vena contracta*, or at so small a distance below it that gravity can have produced no sensible effect upon the effluent fluid. Stop the orifice *r s*, and fill the cylinder with a fluid, and balance it by a weight W in the other scale. Then open the orifice, and the fluid will run out and strike *w*, and then be caught in the scale D. Now when the orifice is opened and the fluid flows out, the pressure upon the bottom of the cylinder is diminished, part of the fluid now not being supported, notwithstanding which the equilibrium is still continued; which shows that the action of the fluid against *w* is exactly equal to the loss of weight in the cylinder by the motion of the fluid through the orifice. In order therefore to find the diminution of the weight upon the bottom of the cylinder, we have only to find a weight equivalent to the momentum of the fluid against *w*.

Let A B (fig. 7.) be a lever flat on the upper side, suspended by an horizontal axis C D; L a scale hanging from it, which is to be balanced by a weight W; E is the cylinder suspended to something immoveable at M, having its orifice *r s* as far distant from A B as before it was from the weight in the scale; and let the orifice and scale be equidistant from C D. Stop the orifice, and fill the cylinder; and upon opening the orifice, let one person, by means of a cock at *v* upon a pipe which goes into a reservoir *x y z*, keep the fluid in the cylinder exactly at the same altitude, and another put such a weight *w* into the scale L as shall keep A B exactly in the same position; then the weight *w* is equivalent to the momentum of the fluid against A B, together with the momentum of the fluid entering the top of the cylinder through the pipe. To determine what weight is equivalent to this latter momentum, take away the cylinder E and weight *w*, and bring A B up to the pipe, and let the fluid act upon it, and find what weight (*v*) put into the scale will now keep A B horizontal, and this weight (*v*) will be equivalent to the momentum of the fluid flowing into the cylinder; hence $w - v$ is a weight equivalent to the momentum of the fluid issuing out of the cylinder at the *vena contracta*, and consequently equivalent to the diminution of the pressure upon the bottom after the opening of the orifice. In order to keep the fluid accurately at the same altitude, I should propose to have a floating gage *v* (fig. 8.) with a wire standing perpendicularly upon it, and entering a cylinder *w* attached to the side of the vessel, and of a bore just large enough to give it a free motion; then the cock must be opened and adjusted to give it such an aperture as will keep the top of the wire on a level with the top of the cylinder.

Or we may find the diminution of the pressure upon the bottom on opening the orifice in this manner. In fig. 6, take away the scale D and balance the cylinder when filled, and let the end C of the beam be made flat at the point from which the vessel is suspended. Then open the orifice of the vessel, having the same provision as before to keep it filled to the same altitude, and place such a weight at C as shall preserve the equilibrium during the time the fluid is in motion, and this weight is equivalent to w in the former case. This method is the most simple of the two; but the other includes a circumstance of some consequence, that is, that the momentum of the effluent fluid is exactly equivalent to the weight which the vessel loses. Having thus examined all the circumstances which I proposed respecting the emptying of vessels, I proceed next to the consideration of the doctrine of the resistance of bodies moving in fluids.

When a body moves in a fluid, each particle, in theory, is supposed to act upon it undisturbed by the rest, or the fluid is conceived to act as if each particle, after the stroke, were annihilated, in which case the following particles would exert their force uninterruptedly. This supposition is very far from being true in fact, and accordingly we find very little agreement between theory and experiment. To experiments therefore we must have recourse for any thing satisfactory upon this subject. I therefore constructed the machine which is here described, whereby both the absolute quantity of resistance in all cases may be very accurately determined, and the law of its variation under different degrees of velocity.

A B, C D (Tab. IV. fig. 9.) are two cross pieces of wood firmly connected together, with screws at each end, so that it may be fixed upon any plane; E G F is a frame fixed upon A B; *m n* a small cylindrical well polished iron axis, having the lower end made conical, and an hollow conical piece to receive it, the upper end passing through G in a polished nut of iron just big enough to give it a free motion; on the top of this axis there are fixed four arms *a, b, c, d,* having each a plane *h, g, f, e,* which may be either of pasteboard or tin, and are thus fixed on. A wire has one end made very flat to which the plane is fixed, and the other end is left round and passes under two small staples made of wire, fixed into the arm so tight that you can but just turn it, so that if you fix the plane in any position it will remain there without any hazard of changing it. Two fine silk lines are wound together round the axis, one leaving the axis on one side and the other on the opposite side, and each, passing over a pulley, is connected to a scale; by this means the lines when drawn by weights put into the scales will give the axis a rotatory motion, and will act in opposite directions, and therefore if equal weights be put into the scales they will destroy each other's effects, so far as regard the position of the axis, so that neither the friction at the bottom nor at the nut at the top will be at all affected by whatever additional weights may be thus added. In respect to any additional friction at the pullies by the increase of weight, that may be diminished so as to become insensible, by increasing the radius of the pullies, and making the ends of their axes conical and letting them turn in a conical orifice, so that they may rest just at their points. If we allow the friction at the axis to be one-fifth of the weight added, which is certainly a great allowance for such an axis well polished, and the radius of the pulley be to the radius of that conical part of the axis where it rests as one hundred to one, then the effect of the friction would be only the five hundredth part of the whole weight; and even this might be diminished one hundred times more by using friction wheels; but this is a degree of accuracy which, I think, can never be required. We might also diminish the friction at the nut, if required, by letting the axis on those two sides towards which the lines act rest between two friction wheels. If the arms should be very long, it may be necessary to fix an upright piece upon K, and connect the extremity of the sails to the top thereof by a string or wire. When this machine is applied to find the resistance of water, the axis mn must be produced up above K, and the string applied to that part; the machine must be immersed in a large reservoir of water, leaving the part of the axis to which the string is applied above the surface. Before we proceed to the application, we must investigate a point called the centre of resistance.

Def. If a plane body revolve in a resisting medium about an axis by means of a weight acting therefrom, that point into which if the whole plane were collected it would suffer the same resistance, I call the *centre of resistance.*

Let a be the area of the plane, and $\dot{a}$ the fluxion of the area at any variable distance x from the centre of the axis, and d the distance of the centre of resistance from that of the axis. Now the effect of the resistance of $\dot{a}$ to oppose the weight is, from the property of the lever, as the resistance multiplied into its distance from the axis, or as $x\dot{a}$; but the resistance is supposed to vary as the square of the velocity (which is found by experiment to be true under certain limitations), or as the square (x^2) of its distance from the axis; hence the effect of the resistance of $\dot{a}$ to oppose the weight is as $x^3\dot{a}$; therefore the whole effect is as the fluent of $x^3\dot{a}$. For the same reason the effect of the resistance of the whole plane a at the distance d is as $d^2 a$; hence $d^2 a =$ flu. $x^2 \dot{a}$, consequently $d = \sqrt[2]{\frac{\text{flu.}\, x^2 \dot{a}}{a}}$.

If the plane be a parallelogram, two of whose sides are parallel to the arms, and m and n the least and greatest distances of the other two sides from the axis, then $d = \sqrt[2]{\frac{n^4 - m^4}{4n - 4m}} = \sqrt[2]{\frac{\overline{n^2 + m^2} \times \overline{n + m}}{4}}$.

Now to find the resistance of the planes striking the fluid perpendicularly, first set them parallel to the horizon, so that they may move edge-ways, or in their own plane, and let two equal weights be put, one into each scale, such as to give the arms an uniform velocity, and then these weights together (w) will be just equivalent to the friction of the axis and the resistance of the arms. Then place the planes perpendicular to the horizon by a plumb-line, and put in two more equal weights, one into each scale, making together W, so as to give the planes the same uniform velocity as before. Then, from what has been already observed, there is no additional friction, and therefore this weight W must be equivalent to the resistance of the planes. But this equivalent weight W acts only at the distance of the radius r of the axis from the centre of motion, whereas the resistance is to be considered as acting at the distance d of the centre of resistance from the centre of motion; hence $d : x :: W : \frac{x}{d} \times W$ the weight acting at the distance d, which is equivalent to the resistance acting at the same distance, and consequently it must be equal to the absolute resistance against all the planes. And to find the velocity, let C feet be the circumference described by the centre of resistance, and let the sails make one revolution in t seconds; then the velocity will be $\frac{c}{t}$ feet in a second.

To find the resistance when the fluid strikes the planes at any angle, set them to that angle, and find the resistance in the very same manner as before. But here we must set two of the opposite planes inclined one way and two the other, so that the fluid may strike the two former on their upper sides, and the two latter on their under sides, but both at the same angle. This caution is necessary in order to prevent any alteration in the pressure, and consequently in the friction upon the axis in the direction thereof; for the fluid striking the planes obliquely, part of the force will be employed in resisting the motion, and part will act perpendicular thereto, or in the direction of the axis, and this latter effect will manifestly be destroyed by the above disposition of the planes, because this force will act *upwards* against two of the planes, and *downwards* against the other two, and being equal, they will destroy each other's effects. The planes may be set to any angle thus: Take a small quadrant divided into degrees; let $m n$ (Tab. IV. fig. 10) be the outward inclined edge of the plane; suspend a plumb-line A B so as just to touch it at n, and at n apply the centre of the quadrant, and let the radius passing through 90° coincide with A B, and turn the plane till $n m$ coincides with that degree at which you would have the plane strike the fluid, and the plane stands right for that angle.

To find the resistance of a solid, we must have two such solids equal to each other, and put on at the opposite ends of two of the arms, for with one only its centrifugal force will increase the friction against the nut, whereas with two opposite to each other this effect will be destroyed. We must also get two thin pieces of lead with the edges feathered off, and of the same weight with the two solids. These must first be put upon the opposite arms, and a weight w found as before. Then the leads are to be taken off, and the solids put on in their place, with that side to go foremost whose resistance is required, and then find W as in the case of the planes; and the absolute resistance will be $\frac{x}{2d} \times W$ upon one of the solids.

By this machine we may find the absolute resistance upon the planes in a direction *perpendicular* to that of their motion. For let the lower end of the axis, instead of resting upon the base of the frame, stand upon one end of an horizontal lever, like that in figure the seventh, and let it be balanced by a weight in a scale hanging at the same distance on the other side of the fulcrum, when the sails have acquired an uniform motion, with the planes horizontal, or when moving edge-ways. Then turn the planes to any angle, and add equal weights to the scales R and T, until the planes have acquired the same uniform velocity as before, and put a weight P into the scale at the other end of the lever, which shall now just balance it, and P will be the absolute resistance of the fluid in a direction perpendicular to the motion of the planes.

The law of resistance, when the velocity varies, may be thus found. Let w, as before, be the sum of the two equal weights which will give the planes an uniform horizontal motion when they move edge-ways. Then set them perpendicular to the horizon, and let W be the sum of the two equal weights, put one into each scale, in order to give the sails the same uniform velocity. Take out these two equal weights, and put in two other equal weights, together equal to Q, such as shall give the planes an uniform velocity double to that before given; then the

Fig. 1.

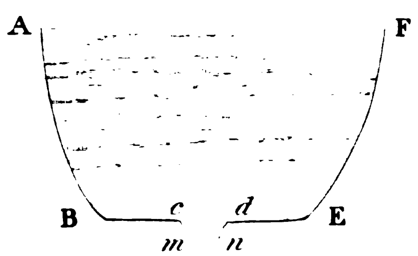


Fig. 2.

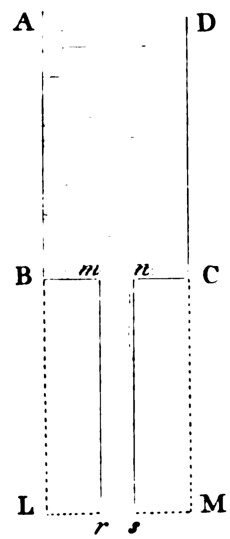


Fig. 3.

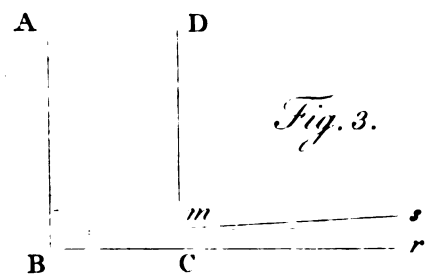


Fig. 4.

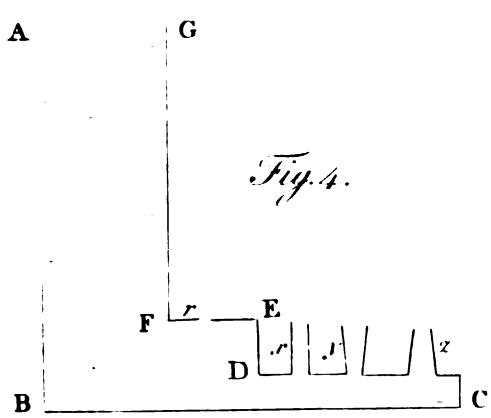
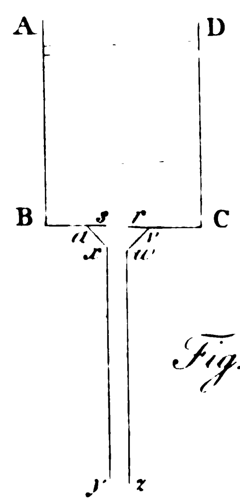


Fig. 5.



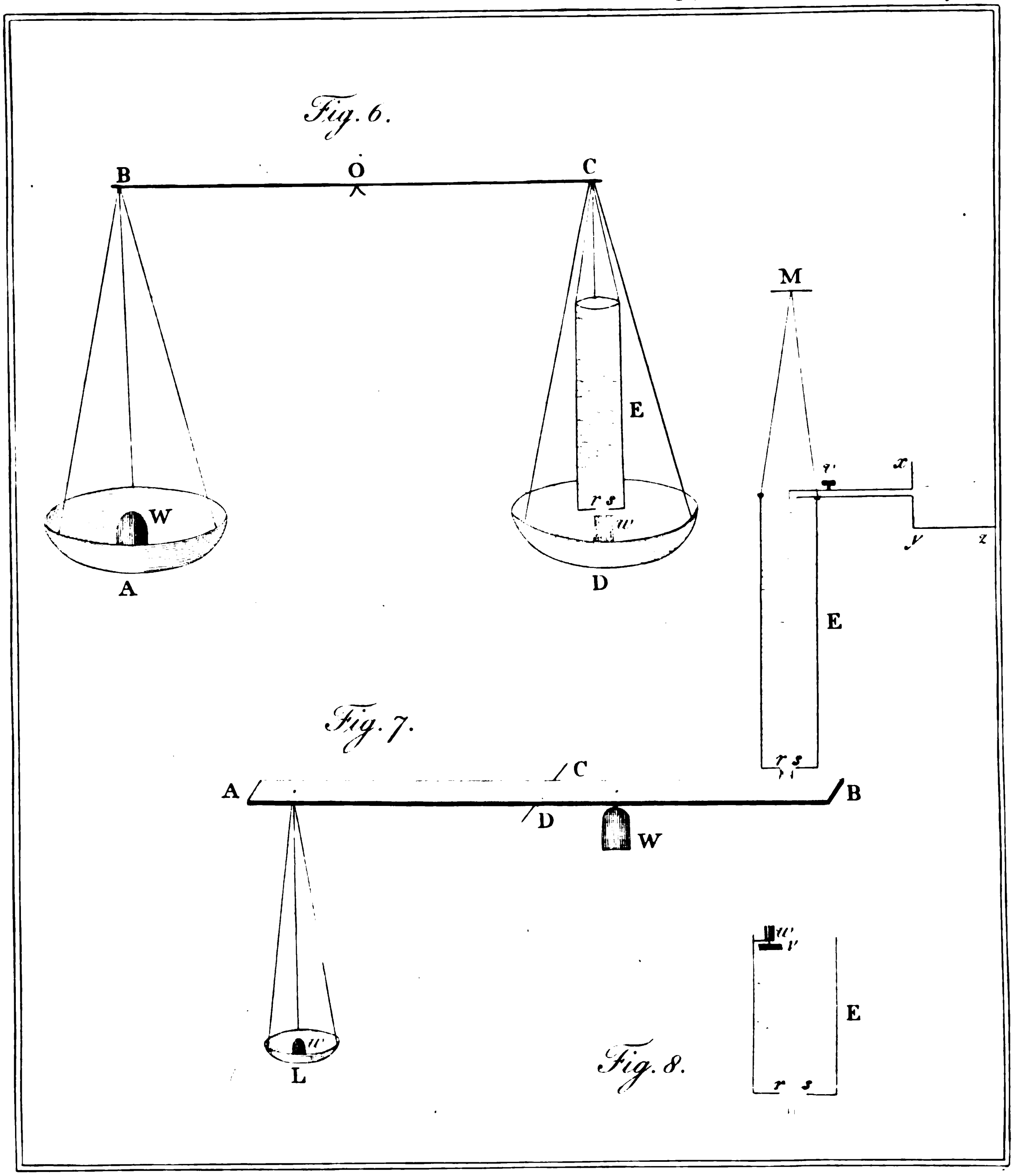


Fig. 6.
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Fig. 7.
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Fig. 8.
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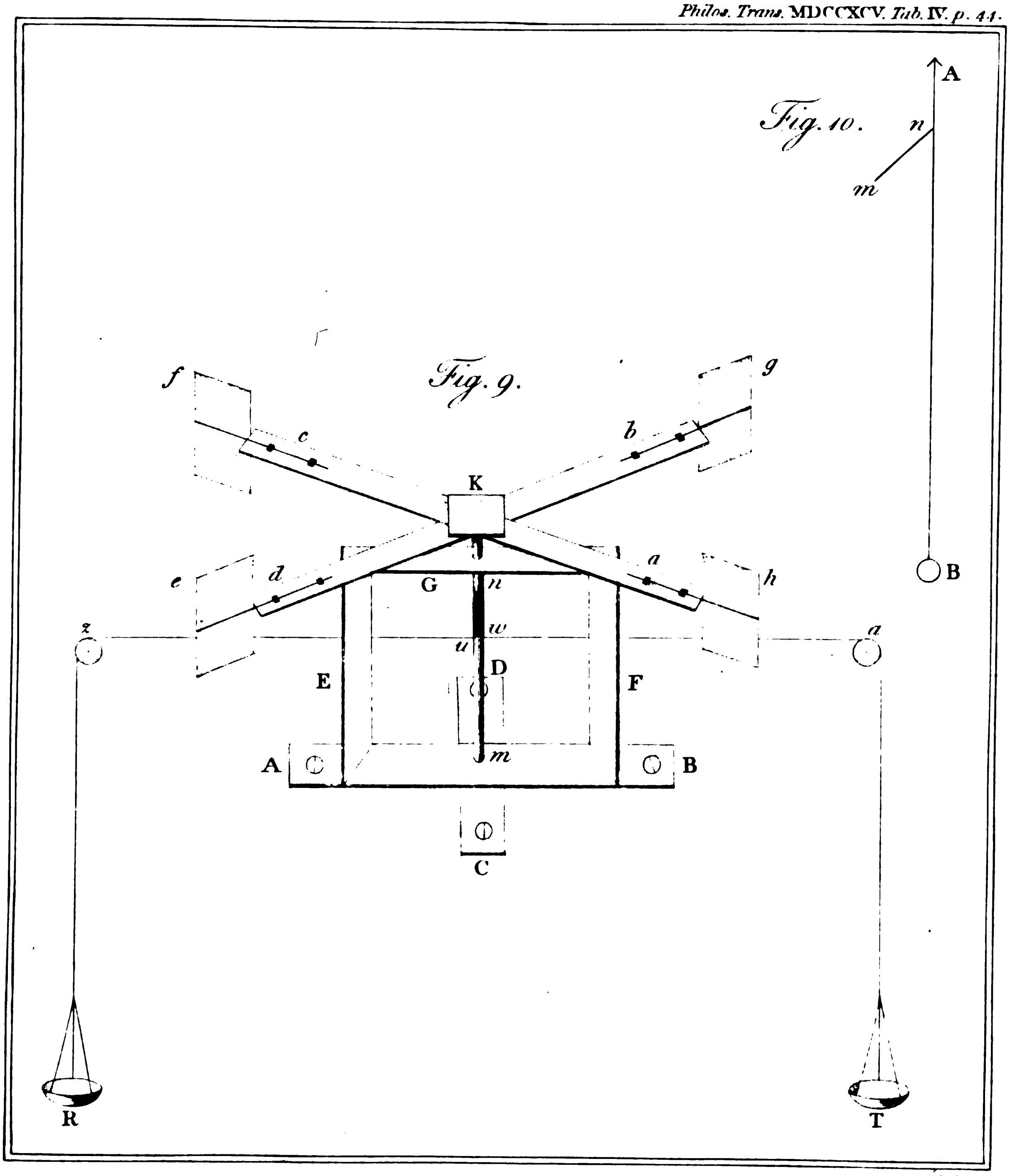
Fig. 10.
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Fig. 9.
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resistances with these two velocities of 1 : 2 will be as W : Q. If R be the sum of the two equal weights put into the scales to give an uniform velocity three times as great as that of the first, then with velocities as 1 : 3 the resistances will be as W : R; and so on. This method was proposed by Mr. ROBINS, in order to determine the law of resistance in terms of the velocity. If the planes be set at any angle, we can by this means get, in terms of the velocity, the law of resistance not only in the direction of the motion of the planes, but also in a direction perpendicular to that of their motion. An account of all the experiments which can be made by this machine, some of which I believe have never yet been attempted, I shall lay before the Royal Society at a future opportunity.

III. *On the Nature and Construction of the Sun and fixed Stars.* *By* William Herschel, *LL.D. F. R. S.*

Read December 18, 1794.

Among the celestial bodies the sun is certainly the first which should attract our notice. It is a fountain of light that illuminates the world! it is the cause of that heat which maintains the productive power of nature, and makes the earth a fit habitation for man! it is the central body of the planetary system; and what renders a knowledge of its nature still more interesting to us is, that the numberless stars which compose the universe, appear, by the strictest analogy, to be similar bodies. Their innate light is so intense, that it reaches the eye of the observer from the remotest regions of space, and forcibly claims his notice.

Now, if we are convinced that an inquiry into the nature and properties of the sun is highly worthy of our notice, we may also with great satisfaction reflect on the considerable progress that has already been made in our knowledge of this eminent body. It would require a long detail to enumerate all the various discoveries which have been made on this subject; I shall, therefore, content myself with giving only the most capital of them.

Sir Isaac Newton has shewn that the sun, by its attractive power, retains the planets of our system in their orbits. He has also pointed out the method whereby the quantity of matter it contains may be accurately determined. Dr. BRADLEY has assigned the velocity of the solar light with a degree of precision exceeding our utmost expectation. GALILEO, SCHEINER, HEVELIUS, CASSINI, and others, have ascertained the rotation of the sun upon its axis, and determined the position of its equator. By means of the transit of Venus over the disc of the sun, our mathematicians have calculated its distance from the earth; its real diameter and magnitude; the density of the matter of which it is composed; and the fall of heavy bodies on its surface.

From the particulars here enumerated, it is sufficiently obvious, that we have already a very clear idea of the vast importance, and powerful influence of the sun on its planetary system. And if we add to this the beneficent effects we feel on this globe from the diffusion of the solar rays; and consider that, by well traced analogies, the same effects have been proved to take place on other planets of this system; I should not wonder if we were induced to think that nothing remained to be added in order to complete our knowledge: and yet it will not be difficult to shew that we are still very ignorant, at least with regard to the internal construction of the sun. The various conjectures, which have been formed on this subject, are evident marks of the uncertainty under which we have hitherto laboured.

The dark spots in the sun, for instance, have been supposed to be solid bodies revolving very near its surface. They have been conjectured to be the smoke of volcanoes, or the scum floating upon an ocean of fluid matter. They have also been taken for clouds. They were explained to be opaque masses, swimming in the fluid matter of the sun; dipping down occasionally. It has been supposed that a fiery liquid surrounded the sun, and that, by its ebbing and flowing, the highest parts of it were occasionally uncovered, and appeared under the shape of dark spots; and that, by the return of this fiery liquid, they were again covered, and in that manner successively assumed different phases. The sun itself has been called a globe of fire, though perhaps metaphorically. The waste it would undergo by a gradual consumption, on the supposition of its being ignited, has been ingeniously calculated. And in the same point of view, its immense power of heating the bodies of such comets as draw very near to it has been assigned.

The bright spots, or faculæ, have been called clouds of light, and luminous vapours. The light of the sun itself has been supposed to be directly invisible, and not to be perceived unless by reflection; though the proofs, which are brought in support of that opinion, seem to me to amount to no more than, what is sufficiently evident, that we cannot see when rays of light do not enter the eye.

But it is time to profit by the many valuable observations that we are now in possession of. A list of successive eminent astronomers may be named, from GALILEO down to the present time; who have furnished us with materials for examination.

In supporting the ideas I shall propose in this paper, with regard to the physical construction of the sun, I have availed myself of the labours of all these astronomers, but have been induced thereto only by my own actual observation of the solar phænomena; which, besides verifying those particulars that had been already observed, gave me such views of the solar regions as led to the foundation of a very rational system. For, having the advantage of former observations, my latest reviews of the body of the sun were immediately directed to the most essential points; and the work was by this means facilitated, and contracted into a pretty narrow compass.

The following is a short extract of my observations on the sun, to which I have joined the consequences I now believe myself entitled to draw from them. When all the reasonings on the several phænomena are put together, and a few additional arguments, taken from analogy, which I shall also add, are properly considered, it will be found that a general conclusion may be made which seems to throw a considerable light upon our present subject.

In the year 1779, there was a spot on the sun which was large enough to be seen with the naked eye. By a view of it with a 7-feet reflector, charged with a very high power, it appeared to be divided into two parts. The largest of the two, on the 19th of April, measured 1′ 8″,06 in diameter; which is equal, in length, to more than 31 thousand miles. Both together must certainly have extended above 50 thousand.

The idea of its being occasioned by a volcanic explosion, violently driving away a fiery fluid, which on its return would gradually fill up the vacancy, and thus restore the sun, in that place, to its former splendour, ought to be rejected on many accounts. To mention only one, the great extent of the spot is very unfavourable to that supposition. Indeed a much less violent and less pernicious cause may be assigned, to account for all the appearances of the spot. When we see a dark belt near the equator of the planet Jupiter, we do not recur to earthquakes and volcanoes for its origin. An atmosphere, with its natural changes, will explain such belts. Our spot in the sun may be accounted for on the same principles. The earth is surrounded by an atmosphere, composed of various elastic fluids. The sun also has its atmosphere, and if some of the fluids which enter into its composition should be of a shining brilliancy, in the manner that will be explained hereafter, while others are merely transparent, any temporary cause which may remove the lucid fluid will permit us to see the body of the sun through the transparent ones. If an observer were placed on the moon, he would see the solid body of our earth only in those places where the transparent fluids of our atmosphere would permit him. In others, the opaque vapours would reflect the light of the sun, without permitting his view to penetrate to the surface of our globe. He would probably also find that our planet had occasionally some shining fluids in its atmosphere; as, not unlikely, some of our northern lights might not escape his notice, if they happened in the unenlightened part of the earth, and were seen by him in his long dark night. Nay, we have pretty good reason to believe, that probably all the planets emit light in some degree; for the illumination which remains on the moon in a total eclipse cannot be entirely ascribed to the light which may reach it by the refraction of the earth's atmosphere. For instance in the eclipse of the moon, which happened October 22, 1790, the rays of the sun refracted by the atmosphere of the earth towards the moon, admitting the mean horizontal refraction to be 30′ 50″,8, would meet in a focus above 189 thousand miles beyond the moon; so that consequently there could be no illumination from rays refracted by our atmosphere. It is, however, not improbable, that about the polar regions of the earth there may be refraction enough to bring some of the solar rays to a shorter focus. The distance of the moon at the time of the eclipse would require a refraction of 54′ 6″ equal to its horizontal parallax at that time, to bring them to a focus so as to throw light on the moon.

The unenlightened part of the planet Venus has also been seen by different persons, and not having a satellite, those regions that are turned from the sun cannot possibly shine by a borrowed light; so that this faint illumination must denote some phosphoric quality of the atmosphere of Venus.

In the instance of our large spot on the sun, I concluded from appearances that I viewed the real solid body of the sun itself, of which we rarely see more than its shining atmosphere.

In the year 1783, I observed a fine large spot, and followed it up to the edge of the sun's limb. Here I took notice that the spot was plainly depressed below the surface of the sun; and that it had very broad shelving sides. I also suspected some part, at least, of the shelving sides to be elevated above the surface of the sun; and observed that, contrary to what usually happens, the margin of that side of the spot, which was farthest from the limb, was the broadest.

The luminous shelving sides of a spot may be explained by a gentle and gradual removal of the shining fluid, which permits us to see the globe of the sun. As to the uncommon appearance of the broadest margin being on that side of the spot which was farthest from the limb when the spot came near the edge of it, we may surmise that the sun has inequalities on its surface, which may possibly be the cause of it. For, when mountainous countries are exposed, if it should chance that the highest parts of the landscape are situated so as to be near that side of the margin, or penumbra of the spot, which is towards the limb, it may partly intercept our view of it, when the spot is seen very obliquely. This would require elevations at least five or six hundred miles high; but considering the great attraction exerted by the sun upon bodies at its surface, and the slow revolution it has upon its axis, we may readily admit inequalities to that amount. From the centrifugal force at the sun's equator, and the weight of bodies at its surface, I compute that the power of throwing down a mountain by the exertion of the former, balanced by the superior force of keeping it in its situation of the latter, is near six and a half times less on the sun than on our equatorial regions; and as an elevation similar to one of three miles on the earth would not be less than 334 miles on the sun, there can be no doubt but that a mountain much higher would stand very firmly. The little density of the solar body seems also to be in favour of the height of its mountains; for, *cæteris paribus*, dense bodies will sooner come to their level than rare ones. The difference in the vanishing of the shelving side, instead of explaining it by mountains, may also, and perhaps more satisfactorily, be accounted for from the real difference of the extent, the arrangement, the height, and the intensity of the shining fluid, added to the occasional changes that may happen in these particulars, during the time in which the spot approaches to the edge of the disc. However, by admitting large mountains on the surface of the sun, we shall account for the different opinions of two eminent astronomers; one of whom believed the spots depressed below the sun, while the other supposed them elevated above it. For it is not improbable that some of the solar mountains may be high enough occasionally to project above the shining elastic fluid, when, by some agitation or other cause, it is not of the usual height; and this opinion is much strengthened by the return of some remarkable spots, which served CASSINI to ascertain the period of the sun's rotation. A very high country, or chain of mountains, may oftener become visible, by the removal of the obstructing fluid, than the lower regions, on account of its not being so deeply covered with it.

In the year 1791, I examined a large spot in the sun, and found it evidently depressed below the level of the surface; about the dark part was a broad margin, or plane of considerable extent, less bright than the sun, and also lower than its surface. This plane seemed to rise, with shelving sides, up to the place where it joined the level of the surface.

In confirmation of these appearances, I carefully remarked that the disc of the sun was visibly convex; and the reason of my attention to this particular, was my being already long acquainted with a certain optical deception, that takes place now and then when we view the moon; which is, that all the elevated spots on its surface will seem to be cavities, and all cavities will assume the shape of mountains. But then, at the same time the moon, instead of having the convex appearance of a globe, will seem to be a large concave portion of an hollow sphere. As soon as, by the force of imagination, you drive away the fallacious appearance of a concave moon, you restore the mountains to their protuberance, and sink the cavities again below the level of the surface. Now, when I saw the spot lower than the shining matter of the sun, and an extended plane, also depressed, with shelving sides rising up to the level, I also found that the sun was convex, and appeared in its natural globular state. Hence I conclude that there could be no deception in those appearances.

How very ill would this observation agree with the ideas of solid bodies bobbing up and down in a fiery liquid? with the smoke of volcanoes, or scum upon an ocean? And how easily it is explained upon our foregoing theory. The removal of the shining atmosphere, which permits us to see the sun, must naturally be attended with a gradual diminution on its borders; an instance of a similar kind we have daily before us, when through the opening of a cloud we see the sky, which generally is attended by a surrounding haziness of some short extent; and seldom transits, from a perfect clearness, at once to the greatest obscurity.

Aug. 26, 1792. I examined the sun with several powers, from 90 to 500. It appears evidently that the black spots are the opaque ground, or body of the sun; and that the luminous part is an atmosphere, which, being interrupted or broken, gives us a transient glimpse of the sun itself. My 7-feet reflector, which is in high perfection, represents the spots, as it always used to do, much depressed below the surface of the luminous part.

Sept. 2, 1792. I saw two spots in the sun with the naked eye. In the telescope I found they were clusters of spots, with many scattered ones besides. Every one of them was certainly below the surface of the luminous disc.

Sept. 8, 1792. Having made a small speculum, merely brought to a perfect figure upon hones, without polish, I found, that by stifling a great part of the solar rays, my object speculum would bear a greater aperture; and thus enabled me to see with more comfort, and less danger. The surface of the sun was unequal; many parts of it being elevated, and others depressed. This is here to be understood of the shining surface only, as the real body of the sun can probably be seldom seen, otherwise than in its black spots.

It may not be impossible, as light is a transparent fluid, that the sun's real surface also may now and then be perceived; as we see the shape of the wick of a candle through its flame, or the contents of a furnace in the midst of the brightest glare of it; but this, I should suppose, will only happen where the lucid matter of the sun is not very accumulated.

Sept. 9, 1792. I found one of the dark spots in the sun drawn pretty near the preceding edge. In its neighbourhood I saw a great number of elevated bright places, making various figures: I shall call them faculæ, with Hevelius; but without assigning to this term any other meaning than what it will hereafter appear ought to be given to it. I see these faculæ extended, on the preceding side, over about one-sixth part of the sun; but so far from resembling torches, they appear to me like the shrivelled elevations upon a dried apple, extended in length, and most of them are joined together, making waves, or waving lines.

By some good views in the afternoon, I find that the rest of the surface of the sun does not contain any faculæ, except a few on the following, and equatorial part of the sun. Towards the north and south I see no faculæ; there is all over the sun a great unevenness in the surface, which has the appearance of a mixture of small points of an unequal light; but they are evidently an unevenness or roughness of high and low parts.

Sept. 11, 1792. The faculæ, in the preceding part of the sun, are much gone out of the disc, and those in the following are come on. A dark spot also is come on with them.

Sept. 13, 1792. There are a great number of faculæ on the equatorial part of the sun, towards the preceding and following parts. I cannot see any towards the poles; but a roughness is visible every where.

Sept. 16, 1792. The sun contains many large faculæ, on the following side of its equator, and also several on the preceding side. I perceive none about the poles. They seem generally to accompany the spots, and probably, as the faculæ certainly are elevations, a great number of them may occasion neighbouring depressions: that is to say, dark spots.

The faculæ being elevations, very satisfactorily explains the reason why they disappear towards the middle of the sun, and re-appear on the other margin; for, about the place where we lose them, they begin to be edge-ways to our view; and if between the faculæ should lie dark spots, they will most frequently break out in the middle of the sun, because they are no longer covered by the side views of these faculæ.

Sept. 22, 1792. There are not many faculæ in the sun, and but few spots; the whole disc, however, is very much marked with roughness, like an orange. Some of the lowest parts of the inequalities are blackish.

Sept. 23, 1792. The following side of the sun contains many faculæ, near the limb. They take up an arch of about 50 degrees. There are, likewise, some on the preceding side. The north and south is rough as usual; but differently disposed. The faculæ are ridges of elevations above the rough surface.

Feb. 23, 1794. By an experiment I have just now tried, I find it confirmed that the sun cannot be so distinctly viewed with a small aperture and faint darkening glasses, as with a large aperture and stronger ones; this latter is the method I always use.

One of the black spots on the preceding margin, which was greatly below the surface of the sun, had, next to it, a protuberant lump of shining matter, a little brighter than the rest of the sun.

About all the spots, the shining matter seems to have been disturbed; and is uneven, lumpy, and zig-zagged in an irregular manner.

I call the spots black, not that they are entirely so, but merely to distinguish them; for there is not one of them, to-day, which is not partly, or entirely, covered over with whitish and unequally bright nebulosity, or cloudiness. This, in many of them, comes near to an extinction of the spot; and in others, seems to bring on a subdivision.

Sept. 28, 1794. There is a dark spot in the sun on the following side. It is certainly depressed below the shining atmosphere, and has shelving sides of shining matter, which rise up higher than the general surface, and are brightest at the top. The preceding shelving side is rendered almost invisible, by the overhanging of the preceding elevations; while the following is very well exposed: the spot being apparently such in figure as denotes a circular form, viewed in an oblique direction.

Near the following margin are many bright elevations, close to visible depressions. The depressed parts are less bright than the common surface.

The penumbra, as it is called, about this spot, is a considerable plane, of less brightness than the common surface, and seems to be as much depressed below that surface as the spot is below the plane.

Hence, if the brightness of the sun is occasioned by the lucid atmosphere, the intensity of the brightness must be less where it is depressed; for light, being transparent, must be the more intense the more it is deep.

Oct. 12, 1794. The whole surface of the sun is diversified by inequality in the elevation of the shining atmosphere. The lowest parts are every where darkest; and every little pit has the appearance of a more or less dark spot.

A dark spot, which is on the preceding side, is surrounded by very great inequalities in the elevation of the lucid atmosphere; and its depression below the same is bounded by an immediate rising of very bright light.

Oct. 13, 1794. The spot in the sun I observed yesterday is drawn so near the margin, that the elevated side of the following part of it hides all the black ground, and still leaves the cavity visible, so that the depression of the black spots, and the elevation of the faculæ, are equally evident.

It will now be easy to bring the result of these observations into a very narrow compass. That the sun has a very extensive atmosphere cannot be doubted; and that this atmosphere consists of various elastic fluids, that are more or less lucid and transparent, and of which the lucid one is that which furnishes us with light, seems also to be fully established by all the phænomena of its spots, of the faculæ, and of the lucid surface itself. There is no kind of variety in these appearances but what may be accounted for with the greatest facility, from the continual agitation which we may easily conceive must take place in the regions of such extensive elastic fluids.

It will be necessary, however, to be a little more particular, as to the manner in which I suppose the lucid fluid of the sun to be generated in its atmosphere. An analogy that may be drawn from the generation of clouds in our own atmosphere, seems to be a very proper one, and full of instruction. Our clouds are probably decompositions of some of the elastic fluids of the atmosphere itself, when such natural causes, as in this grand chemical laboratory are generally at work, act upon them; we may therefore admit that in the very extensive atmosphere of the sun, from causes of the same nature, similar phænomena will take place; but with this difference, that the continual and very extensive decompositions of the elastic fluids of the sun, are of a phosphoric nature, and attended with lucid appearances, by giving out light.

If it should be objected, that such violent and unremitting decompositions would exhaust the sun, we may recur again to our analogy, which will furnish us with the following reflections. The extent of our own atmosphere, we see, is still preserved, notwithstanding the copious decompositions of its fluids, in clouds and falling rain; in flashes of lightning, in meteors, and other luminous phænomena; because there are fresh supplies of elastic vapours, continually ascending to make good the waste occasioned by those decompositions. But it may be urged, that the case with the decomposition of the elastic fluids in the solar atmosphere would be very different, since light is emitted, and does not return to the sun, as clouds do to the earth when they descend in showers of rain. To which I answer, that in the decomposition of phosphoric fluids every other ingredient but light may also return to the body of the sun. And that the emission of light must waste the sun, is not a difficulty that can be opposed to our hypothesis. For as it is an evident fact that the sun does emit light, the same objection, if it could be one, would equally militate against every other assignable way to account for the phænomenon.

There are moreover considerations that may lessen the pressure of this alleged difficulty. We know the exceeding subtilty of light to be such, that in ages of time its emanation from the sun cannot very sensibly lessen the size of this great body. To this may be added, that, very possibly, there may also be ways of restoration to compensate for what is lost by the emission of light; though the manner in which this can be brought about should not appear to us. Many of the operations of nature are carried on in her great laboratory, which we cannot comprehend; but now and then we see some of the tools with which she is at work. We need not wonder that their construction should be so singular as to induce us to confess our ignorance of the method of employing them, but we may rest assured that they are not a mere *lusus naturæ*. I allude to the great number of small telescopic comets that have been observed; and to the far greater number still that are probably much too small for being noticed by our most diligent searchers after them. Those six, for instance, which my sister has discovered, I can from examination affirm had not the least appearance of any solid nucleus, and seemed to be mere collections of vapours condensed about ~~a centre.~~ Five more, that I have also observed, were nearly of the same nature.

This throws a mystery over their destination, which seems to place them in the allegorical view of tools, probably designed for some salutary purposes to be wrought by them; and, whether the restoration of what is lost to the sun by the emission of light, the possibility of which we have been mentioning above, may not be one of these purposes, I shall not presume to determine. The motion of the comet discovered by Mr. MESSIER in June, 1770, plainly indicated how much its orbit was liable to be changed, by the perturbations of the planets; from which, and the little agreement that can be found between the elements of the orbits of all the comets that have been observed, it appears clearly that they may be directed to carry their salutary influence to any part of the heavens.

My hypothesis, however, as before observed, does not lay me under any obligation to explain how the sun can sustain the waste of light, nor to shew that it will sustain it for ever; and I should also remark that, as in the analogy of generating clouds I merely allude to their production as owing to a decomposition of some of the elastic fluids of our atmosphere, that analogy, which firmly rests upon the fact, will not be less to my purpose to whatever cause these clouds may owe their origin. It is the same with the lucid clouds, if I may so call them, of the sun. They plainly exist, because we see them; the manner of their being generated may remain an hypothesis; and mine, till a better can be proposed, may stand good; but whether it does or not, the consequences I am going to draw from what has been said will not be affected by it.

Before I proceed, I shall only point out, that according to the above theory, a dark spot in the sun is a place in its atmosphere which happens to be free from luminous decompositions; and that faculæ are, on the contrary, more copious mixtures of such fluids as decompose each other. The penumbra which attends the spots, being generally depressed more or less to about half way between the solid body of the sun and the upper part of those regions in which luminous decompositions take place, must of course be fainter than other parts. No spot favourable for taking measures having lately been on the sun, I can only judge, from former appearances, that the regions in which the luminous solar clouds are formed, adding thereto the elevation of the faculæ, cannot be less than 1843, nor much more than 2765 miles in depth. It is true that in our atmosphere the extent of the clouds is limited to a very narrow compass; but we ought rather to compare the solar ones to the luminous decompositions which take place in our *aurora borealis*, or luminous arches, which extend much farther than the cloudy regions. The density of the luminous solar clouds, though very great, may not be exceedingly more so than that of our *aurora borealis*. For, if we consider what would be the brilliancy of a space two or three thousand miles deep, filled with such corruscations as we see now and then in our atmosphere, their apparent intensity, when viewed at the distance of the sun, might not be much inferior to that of the lucid solar fluid.

From the luminous atmosphere of the sun I proceed to its opaque body, which by calculation from the power it exerts upon the planets we know to be of great solidity; and from the phænomena of the dark spots, many of which, probably on account of their high situations, have been repeatedly seen, and otherwise denote inequalities in their level, we surmise that its surface is diversified with mountains and vallies.

What has been said enables us to come to some very important conclusions, by remarking, that this way of considering the sun and its atmosphere, removes the great dissimilarity we have hitherto been used to find between its condition and that of the rest of the great bodies of the solar system.

The sun, viewed in this light, appears to be nothing else than a very eminent, large, and lucid planet, evidently the first, or in strictness of speaking, the only primary one of our system; all others being truly secondary to it. Its similarity to the other globes of the solar system with regard to its solidity, its atmosphere, and its diversified surface; the rotation upon its axis, and the fall of heavy bodies, leads us on to suppose that it is most probably also inhabited, like the rest of the planets, by beings whose organs are adapted to the peculiar circumstances of that vast globe.

Whatever fanciful poets might say, in making the sun the abode of blessed spirits, or angry moralists devise, in pointing it out as a fit place for the punishment of the wicked, it does not appear that they had any other foundation for their assertions than mere opinion and vague surmise; but now I think myself authorized, *upon astronomical principles*, to propose the sun as an inhabitable world, and am persuaded that the foregoing observations, with the conclusions I have drawn from them, are fully sufficient to answer every objection that may be made against it.

It may, however, not be amiss to remove a certain difficulty, which arises from the effect of the sun's rays upon our globe. The heat which is here, at the distance of 95 millions of miles, produced by these rays, is so considerable, that it may be objected, that the surface of the globe of the sun itself must be scorched up beyond all conception.

This may be very substantially answered by many proofs drawn from natural philosophy, which shew that heat is produced by the sun's rays only when they act upon a calorific medium; they are the cause of the production of heat, by uniting with the matter of fire, which is contained in the substances that are heated: as the collision of flint and steel will inflame a magazine of gunpowder, by putting all the latent fire it contains into action. But an instance or two of the manner in which the solar rays produce their effect, will bring this home to our most common experience.

On the tops of mountains of a sufficient height, at an altitude where clouds can very seldom reach, to shelter them from the direct rays of the sun, we always find regions of ice and snow. Now if the solar rays themselves conveyed all the heat we find on this globe, it ought to be hottest where their course is least interrupted. Again, our aëronauts all confirm the coldness of the upper regions of the atmosphere; and since, therefore, even on our earth the heat of any situation depends upon the aptness of the medium to yield to the impression of the solar rays, we have only to admit, that on the sun itself, the elastic fluids composing its atmosphere, and the matter on its surface, are of such a nature as not to be capable of any excessive affection from its own rays; and, indeed, this seems to be proved by the copious emission of them; for if the elastic fluids of the atmosphere, or the matter contained on the surface of the sun, were of such a nature as to admit of an easy, chemical combination with its rays, their emission would be much impeded.

Another well known fact is, that the solar focus of the largest lens, thrown into the air, will occasion no sensible heat in the place where it has been kept for a considerable time, although its power of exciting combustion, when proper bodies are exposed, should be sufficient to fuse the most refractory substances.*

It will not be necessary to mention other objections, as I can think of none that may be made, but what a proper consideration of the foregoing observations will easily remove; such as may be urged from the dissimilarity between the luminous atmosphere of the sun and that of our globe will be touched upon hereafter, when I consider the objections that may be assigned against the moon's being an inhabitable satellite.

I shall now endeavour, by analogical reasonings, to support the ideas I have suggested concerning the construction and purposes of the sun; in order to which, it will be necessary to begin with such arguments as the nature of the case will admit, to shew that our moon is probably inhabited. This satellite is of all the heavenly bodies the nearest, and therefore most within the reach of our telescopes. Accordingly we find, by repeated inspection, that we can with perfect confidence give the following account of it.

It is a secondary planet, of a considerable size; the surface of which is diversified, like that of the earth, by mountains and vallies. Its situation, with respect to the sun, is much like that of the earth; and, by a rotation on its axis, it enjoys an agreeable variety of seasons, and of day and night. To the moon, our globe will appear to be a very capital satellite;

* The subject of light and heat has been very ably discussed by Mr. DE LUC, in his excellent work, *Idées sur la Météorologie*, Tome I. part 2, chap. 2, section 2, *De la Nature du Feu*; and Tome II. part 3, chap. 6, section 2, *Des Rapports de la Lumière avec la Chaleur dans l'Atmosphère*.

undergoing the same regular changes of illuminations as the moon does to the earth. The sun, the planets, and the starry constellations of the heavens, will rise and set there as they do here; and heavy bodies will fall on the moon as they do on the earth. There seems only to be wanting, in order to complete the analogy, that it should be inhabited like the earth.

To this it may be objected, that we perceive no large seas in the moon; that its atmosphere (the existence of which has even been doubted by many) is extremely rare, and unfit for the purposes of animal life; that its climates, its seasons, and the length of its days, totally differ from ours; that without dense clouds (which the moon has not), there can be no rain; perhaps no rivers, no lakes. In short, that, notwithstanding the similarity which has been pointed out, there seems to be a decided difference in the two planets we have compared.

My answer to this will be, that that very difference which is now objected, will rather strengthen the force of my argument than lessen its value: we find, even upon our globe, that there is the most striking difference in the situation of the creatures that live upon it. While man walks upon the ground, the birds fly in the air, and fishes swim in water; we can certainly not object to the conveniences afforded by the moon, if those that are to inhabit its regions are fitted to their conditions as well as we on this globe are to ours. An absolute, or total sameness, seems rather to denote imperfections, such as nature never exposes to our view; and, on this account, I believe the analogies that have been mentioned fully sufficient to establish the high probability of the moon's being inhabited like the earth.

To proceed, we will now suppose an inhabitant of the moon, who has not properly considered such analogical reasonings as might induce him to surmise that our earth is inhabited, were to give it as his opinion that the use of that great body, which he sees in his neighbourhood, is to carry about his little globe, that it may be properly exposed to the light of the sun, so as to enjoy an agreeable and useful variety of illumination, as well as to give it light by reflection from the sun, when direct daylight cannot be had. Suppose also that the inhabitants of the satellites of Jupiter, Saturn, and the Georgian planet, were to look upon the primary ones, to which they belong, as mere attractive centres, to keep together their orbits, to direct their revolution round the sun, and to supply them with reflected light in the absence of direct illumination. Ought we not to condemn their ignorance, as proceeding from want of attention and proper reflection? It is very true that the earth, and those other planets that have satellites about them, perform all the offices that have been named, for the inhabitants of these little globes; but to us, who live upon one of these planets, their reasonings cannot but appear very defective; when we see what a magnificent dwelling place the earth affords to numberless intelligent beings.

These considerations ought to make the inhabitants of the planets wiser than we have supposed those of their satellites to be. We surely ought not, like them, to say " the sun (that " immense globe, whose body would much more than fill the " whole orbit of the moon) is merely an attractive centre to " us." From experience we can affirm, that the performance of the most salutary offices to inferior planets, is not inconsistent with the dignity of superior purposes; and, in consequence of such analogical reasonings, assisted by telescopic views, which plainly favour the same opinion, we need not hesitate to admit that the sun is richly stored with inhabitants.

This way of considering the sun is of the utmost importance in its consequences. That stars are suns can hardly admit of a doubt. Their immense distance would perfectly exclude them from our view, if the light they send us were not of the solar kind. Besides, the analogy may be traced much farther. The sun turns on its axis. So does the star Algol. So do the stars called β Lyræ, δ Cephei, η Antinoi, o Ceti, and many more; most probably all. From what other cause can we so probably account for their periodical changes? Again, our sun has spots on its surface. So has the star Algol; and so have the stars already named; and probably every star in the heavens. On our sun these spots are changeable. So they are on the star o Ceti; as evidently appears from the irregularity of its changeable lustre, which is often broken in upon by accidental changes, while the general period continues unaltered. The same little deviations have been observed in other periodical stars, and ought to be ascribed to the same cause. But if stars are suns, and suns are inhabitable, we see at once what an extensive field for animation opens itself to our view.

It is true that analogy may induce us to conclude, that since stars appear to be suns, and suns, according to the common opinion, are bodies that serve to enlighten, warm, and sustain a system of planets, we may have an idea of numberless globes that serve for the habitation of living creatures. But if these suns themselves are primary planets, we may see some thousands of them with our own eyes; and millions by the help of telescopes; when at the same time, the same analogical reasoning still remains in full force, with regard to the planets which these suns may support.

In this place I may, however, take notice that, from other considerations, the idea of suns or stars being *merely* the supporters of systems of planets, is not absolutely to be admitted as a general one. Among the great number of very compressed clusters of stars, I have given in my catalogues, there are some which open a different view of the heavens to us. The stars in them are so very close together, that, notwithstanding the great distance at which we may suppose the cluster itself to be, it will hardly be possible to assign any sufficient mutual distance to the stars composing the cluster, to leave room for crowding in those planets, for whose support these stars have been, or might be, supposed to exist. It should seem, therefore, highly probable that they exist for themselves; and are, in fact, only very capital, *lucid*, primary planets, connected together in one great system of mutual support.

As in this argument I do not proceed upon conjectures, but have actual observations in view, I shall mention an instance in the clusters, No. 26, 28, and 35, VI. class, of my catalogue of nebulæ, and clusters of stars. (See Phil. Trans. Vol. LXXIX. Part II. p. 251.) The stars in them are so crowded, that I cannot conjecture them to be at a greater apparent distance from each other than five seconds; even after a proper allowance for such stars, as on a supposition of a globular form of the cluster, will interfere with one another, has been made. Now, if we would leave as much room between each of these stars as there is between the sun and Sirius, we must place these clusters 42104 times as far from us as that star is from the sun. But in order to bring down the lustre of Sirius to that of an equal star placed at such a distance, I ought to reduce the aperture of my 20-feet telescope to less than the two-and-twenty hundredth part of an inch; when certainly I could no longer expect to see any star at all.

The same remark may be made, with regard to the number of very close double stars; whose apparent diameters being alike, and not very small, do not indicate any very great mutual distance. From which, however, must be deducted all those where the different distances may be compensated by the real difference in their respective magnitudes.

To what has been said may be added, that in some parts of the milky way, where yet the stars are not very small, they are so crowded, that in the year 1792, Aug. 22, I found by the gages that, in 41 minutes of time, no less than 258 thousand of them had passed through the field of view of my telescope.*

It seems, therefore, upon the whole not improbable that, in

* The star-gages ran thus:

From $19^h\ 35'$	to $19^h\ 51'$	600	stars in the field
19 51	— 19 57	440	
19 57	— 20 12	360	
20 12	— 20 16	260	

The breadth of the sweep was $2^\circ\ 35'$, the diameter of the field $15'$, and the mean polar distance $73^\circ\ 54'$. Then let

F, be the diameter of the field of view,
S, the number of stars in each field,
B, the breadth of the sweep, plus F,
T, the length of the sweep expressed in minutes of space,
φ, the sine of the mean polar distance,
C, the constant fraction ,7854,

and the stars in these four successive short sweeps will be found by the expression $\frac{B\,T\,S\,\varphi}{F^2\,C}$ equal to 133095. 36601. 74866. 14419. or in all 258981.

many cases, stars are united in such close systems as not to leave much room for the orbits of planets, or comets; and that consequently, upon this account also, many stars, unless we would make them mere useless brilliant points, may themselves be lucid planets, perhaps unattended by satellites.

POSTSCRIPT.

The following observations, which were made with an improved apparatus, and under the most favourable circumstances, should be added to those which have been given. They are decisive with regard to one of the conditions of the lucid matter of the sun.

Nov. 26, 1794. Eight spots in the sun, and several subdivisions of them, are all equally depressed.

The sun is mottled every where.

The mottled appearance of the sun is owing to an inequality in the level of the surface.

The sun is equally mottled at its poles and at its equator; but the mottled appearances may be seen better about the middle of the disc than towards the circumference, on account of the sun's spherical form.

The unevenness arising from the elevation and depression of the mottled appearance on the surface of the sun, seems, in many places, to amount to as much, or to nearly as much as the depression of the penumbræ of the spots below the upper part of the shining substance; without including faculæ, which are protuberant.

The lucid substance of the sun is neither a liquid, nor an elastic fluid; as is evident from its not instantly filling up the cavities of the spots, and of the unevenness of the mottled parts. It exists, therefore, in the manner of lucid clouds swimming in the transparent atmosphere of the sun; or rather, of luminous decompositions taking place within that atmosphere.

IV. *An Account of the late Eruption of Mount* Vesuvius. *In a Letter from the Right Honourable Sir* William Hamilton, *K. B. F. R. S. to Sir* Joseph Banks, *Bart. P. R. S.*

Read January 15, 1795.

SIR, Naples, August 25th, 1794.

EVERY day produces some new publication relative to the late tremendous eruption of mount Vesuvius, so that the various phænomena that attended it will be found on record in either one or other of these publications, and are not in that danger of being passed over and forgotten, as they were formerly, when the study of natural history was either totally neglected, or treated of in a manner very unworthy of the great Author of nature. I am sorry to say, that even so late as in the accounts of the earthquakes in Calabria in 1783, printed at Naples, nature is taxed with being malevolent, and bent upon destruction. In a printed account of another great eruption of Mount Vesuvius in 1631, by ANTONIO SANTORELLI, doctor of medicine, and professor of natural philosophy in the university of Naples, and at the head of the fourth chapter of his book, are these words: *Se questo incendio sia opera de' demonii? Whether this eruption be the work of devils?* The account of an eruption of Vesuvius in 1737, published at Naples by Doctor SERAO, is of a very different cast, and does great honour to his memory. All great eruptions of volcanoes must naturally produce nearly the same phænomena, and in Serao's book almost all the phænomena we have been witness to during the late eruption of Vesuvius, are there admirably described, and well accounted for. The classical accounts of the eruption of Vesuvius, which destroyed the towns of Herculaneum and Pompeii, and many of the existing printed accounts of its great eruption in 1631 (although the latter are mixed with puerilities) might pass for an account of the late eruption by only changing the date, and omitting that circumstance of the retreat of the sea from the coast, which happened in both those great eruptions, and not in this; and I might content myself by referring to those accounts, and assuring you at the same time, that the late eruption, after those two, appears to have been the most violent recorded by history, and infinitely more alarming than either the eruption of 1767, or that of 1779, of both of which I had the honour of giving a particular account to the Royal Society. However, I think it my duty rather to hazard being guilty of repetition than to neglect the giving you every satisfaction in my power, relative to the late formidable operation of nature.

You know, Sir, that with the kind assistance of the Father Antonio Piaggi, of the order of the *Scole Pie*, who has resided many years at Resina, on the very foot of Mount Vesuvius, and in the full view of it, I am in possession of an exact diary of that volcano, from the year 1779 to this day, and which is also accompanied with drawings. It is plain, from some remarks in that diary, previous to this eruption, that a great one was expected, and that we were apprehensive of the mischief that might probably attend the falling in of the crater, which had been much contracted within these two years past, by the great emission of scoriæ and ashes from time to time, and which had also increased the height of the volcano, and nearly filled up its crater. The frequent slight eruptions of lava for some years past have issued from near the summit, and ran in small channels in different directions down the flanks of the mountain, and from running in covered channels, had often an appearance as if they came immediately out of the sides of Vesuvius, but such lavas had not sufficient force to reach the cultivated parts atthe foot of the mountain. In the year 1779, the whole quantity of the lava in fusion having been at once thrown up with violence out of the crater of Vesuvius, and a great part of it falling, and cooling on its cone, added much to the solidity of the walls of this huge natural chimney, if I may be allowed so to call it, and has not of late years allowed of a sufficient discharge of lava to calm that fermentation, which by the subterraneous noises heard at times, and by the explosions of scoriæ and ashes, was known to exist within the bowels of the volcano; so that the eruptions of late years, before this last, have, as I have said, been simply from the lava having boiled over the crater, the sides being sufficiently strong to confine it, and oblige it to rise and overflow. The mountain had been remarkably quiet for seven months before its late eruption, nor did the usual smoke issue from its crater, but at times it emitted small clouds of smoke, that floated in the air in the shape of little trees. It was remarked by the Father Antonio di Petrizzi, a capuchin friar (who has printed an account of the late eruption) from his convent close to the unfortunate town of Torre del Greco, that for some days preceding this eruption a thick vapour was seen to surround the mountain, about a quarter of a mile beneath its crater, as it was remarked by him, and others at the same time, that both the sun and the moon had often an unusual reddish cast.

The water of the great fountain at Torre del Greco began to decrease some days before the eruption, so that the wheels of a corn-mill, worked by that water, moved very slowly; it was necessary in all the other wells of the town and its neighbourhood to lengthen the ropes daily, in order to reach at the water; and some of the wells became quite dry. Although most of the inhabitants were sensible of this phænomenon, not one of them seems to have suspected the true cause of it. It has been well attested, that eight days before the eruption, a man and two boys, being in a vineyard above Torre del Greco (and precisely on the spot where one of the new mouths opened, from whence the principal current of lava that destroyed the town issued), were much alarmed by a sudden puff of smoke that came out of the earth close to them, and was attended with a slight explosion.

Had this circumstance, with that of the subterraneous noises heard at Resina for two days before the eruption (with the additional one of the decrease of water in the wells, as above-mentioned) been communicated at the time, it would have required no great foresight to have been certain that an eruption of the volcano was near at hand, and that its force was directed particularly towards that part of the mountain.

On the 12th of June, in the morning, there was a violent fall of rain, and soon after the inhabitants of Resina, situated directly over the ancient town of Herculaneum, were sensible of a rumbling subterraneous noise, which was not heard at Naples.

From the month of January to the month of May last, the atmosphere was generally calm, and we had continued dry weather. In the month of May we had a little rain, but the weather was unusually sultry. For some days preceding the eruption, the Duke DELLA TORRE, a learned and ingenious nobleman of this country, and who has published two letters upon the subject of the late eruption, observed by his electrometers that the atmosphere was charged in excess with the electric fluid, and continued so for several days during the eruption: there are many other curious observations in the duke's account of the late eruption.

About 11 o'clock at night of the 12th of June, at Naples we were all sensible of a violent shock of an earthquake; the undulatory motion was evidently from east to west, and appeared to me to have lasted near half a minute. The sky, which had been quite clear, was soon after covered with black clouds. The inhabitants of the towns and villages, which are very numerous at the foot of Vesuvius, felt this earthquake still more sensibly, and say, that the shock at first was from the bottom upwards, after which followed the undulation from east to west. This earthquake extended all over the Campagna Felice; and their Sicilian Majesties were pleased to tell me, that the royal palace at Caserta, which is 15 miles from this city, and one of the most magnificent and solid buildings in Europe (the walls being 18 feet thick), was shook in such a manner as to cause great alarm, and that all the chamber bells rang. It was likewise much felt at Beneventum, about 30 miles from Naples; and at Ariano in Puglia, which is at a much greater distance; both these towns have been often afflicted with earthquakes.

On Sunday the 15th of June, soon after 10 o'clock at night, another shock of an earthquake was felt at Naples, but did not appear to be quite so violent as that of the 12th, nor did it last so long; at the same moment a fountain of bright fire, attended with a very black smoke and a loud report, was seen to issue, and rise to a great height, from about the middle of the cone of Vesuvius; soon after another of the same kind broke out at some little distance lower down; then, as I suppose by the blowing up of a covered channel full of red-hot lava, it had the appearance as if the lava had taken its course directly up the steep cone of the volcano. Fresh fountains succeeded one another hastily, and all in a direct line tending, for about a mile and a half down, towards the towns of Resina and Torre del Greco. I could count 15 of them, but I believe there were others obscured by the smoke. It seems probable, that all these fountains of fire, from their being in such an exact line, proceeded from one and the same long fissure down the flanks of the mountain, and that the lava and other volcanic matter forced its way out of the widest parts of the crack, and formed there the little mountains and craters that will be described in their proper place. It is impossible that any description can give an idea of this fiery scene, or of the horrid noises that attended this great operation of nature. It was a mixture of the loudest thunder, with incessant reports, like those from a numerous heavy artillery, accompanied by a continued hollow murmur, like that of the roaring of the ocean during a violent storm; and added to these was another blowing noise, like that of the going up of a large flight of sky-rockets, and which brought to my mind also that noise which is produced by the action of the enormous bellows on the furnace of the Carron iron foundery in Scotland, and which it perfectly resembled. The frequent falling of the huge stones and scoriæ, which were thrown up to an incredible height from some of the new mouths, and one of which having been since measured by the Abbé TATA (who has published an account of this eruption), was 10 feet high, and 35 in circumference, contributed undoubtedly to the concussion of the earth and air, which kept all the houses at Naples for several hours in a constant tremor, every door and window shaking and rattling incessantly, and the bells ringing. This was an awful moment! The sky, from a bright full moon and star-light, began to be obscured; the moon had presently the appearance of being in an eclipse, and soon after was totally lost in obscurity. The murmur of the prayers and lamentations of a numerous populace forming various processions, and parading in the streets, added likewise to the horror. As the lava did not appear to me to have yet a sufficient vent, and it was now evident that the earthquakes we had already felt had been occasioned by the air and fiery matter confined within the bowels of the mountain, and probably at no small depth (considering the extent of those earthquakes), I recommended to the company that was with me, who began to be much alarmed, rather to go and view the mountain at some greater distance, and in the open air, than to remain in the house, which was on the sea side, and in the part of Naples that is nearest and most exposed to Vesuvius. We accordingly went to Posilipo, and viewed the conflagration, now become still more considerable, from the sea side under that mountain; but whether from the eruption having increased, or from the loud reports of the volcanic explosions being repeated by the mountain behind us, the noise was much louder, and more alarming than that we had heard in our first position, at least a mile nearer to Vesuvius. After some time, and which was about two o'clock in the morning of the 16th, having observed that the lavas ran in abundance freely, and with great velocity, having made a considerable progress towards Resina, the town which it first threatened, and that the fiery vapours which had been confined had now free vent, through many parts of a crack of more than a mile and a half in length, as was evident from the quantity of inflamed matter and black smoke, which continued to issue from the new mouths abovementioned without any interruption, I concluded that at Naples all danger from earthquakes, which had been my greatest apprehension, was now totally removed, and we returned to our former station at S. Lucia at Naples.

All this time there was not the smallest appearance of fire or smoke from the crater on the summit of Vesuvius; but the black smoke and ashes issuing continually from so many new mouths, or craters, formed an enormous and dense body of clouds over the whole mountain, and which began to give signs of being replete with the electric fluid, by exhibiting flashes of that sort of zig-zag lightning, which in the volcanic language of this country is called *ferilli*, and which is the constant attendant on the most violent eruptions. From what I have read and seen, it appears to me, that the truest judgment that can be formed of the degree of force of the fermentation within the bowels of a volcano during its eruption, would be from observing the size, and the greater or less elevation of those piles of smoky clouds, which rise out of the craters, and form a gigantic mass over it, usually in the form of a pine tree, and from the greater or less quantity of the *ferilli*, or volcanic electricity, with which those clouds appear to be charged.

During thirty years that I have resided at Naples, and in which space of time I have been witness to many eruptions of Vesuvius, of one sort or other, I never saw the gigantic cloud abovementioned replete with the electric fire, except in the two great eruptions of 1767, that of 1779, and during this more formidable one. The electric fire, in the year 1779, that played constantly within the enormous black cloud over the crater of Vesuvius, and seldom quitted it, was exactly similar to that which is produced, on a very small scale, by the conductor of an electrical machine communicating with an insulated plate of glass, thinly spread over with metallic filings, &c. when the electric matter continues to play over it in zigzag lines without quitting it. I was not sensible of any noise attending that operation in 1779; whereas the discharge of the electrical matter from the volcanic clouds during this eruption, and particularly the second and third days, caused explosions like those of the loudest thunder; and indeed the storms raised evidently by the sole power of the volcano, resembled in every respect all other thunder-storms; the lightning falling and destroying every thing in its course. The house of the Marquis of Berio at S. Iorio, situated at the foot of Vesuvius, during one of these volcanic storms was struck with lightning, which having shattered many doors and windows, and damaged the furniture, left for some time a strong smell of sulphur in the rooms it passed through. Out of these gigantic and volcanic clouds, besides the lightning, both during this eruption and that of 1779, I have, with many others, seen balls of fire issue, and some of a considerable magnitude, which bursting in the air, produced nearly the same effect as that from the air-balloons in fireworks, the electric fire that came out having the appearance of the serpents with which those firework balloons are often filled. The day on which Naples was in the greatest danger from the volcanic clouds, two small balls of fire, joined together by a small link like a chain-shot, fell close to my *casino*, at Posilipo; they separated, and one fell in the vineyard above the house, and the other in the sea, so close to it that I heard a splash in the water; but, as I was writing, I lost the sight of this phænomenon, which was seen by some of the company with me, and related to me as above. The Abbé Tata, in his printed account of this eruption, mentions an enormous ball of this kind which flew out of the crater of Vesuvius whilst he was standing on the edge of it, and which burst in the air at some distance from the mountain, soon after which he heard a noise like the fall of a number of stones, or of a heavy shower of hail.

During the eruption of the 15th at night, few of the inhabitants of Naples, from the dread of earthquakes, ventured to go to their beds. The common people were either employed in devout processions in the streets, or were sleeping on the quays and open places; the nobility and gentry, having caused their horses to be taken from their carriages, slept in them in the squares and open places, or on the high roads just out of the town. For several days, whilst the volcanic storms of thunder and lightning lasted, the inhabitants at the foot of the volcano, both on the sea side and the Somma side, were often sensible of a tremor in the earth, as well as of the concussions in the air, but at Naples only the earthquakes of the 12th and 15th of June were distinctly and universally felt: this fair city could not certainly have resisted long, had not those earthquakes been fortunately of a short duration. Throughout this eruption, which continued in force about ten days, the fever of the mountain, as has been remarked in former eruptions, shewed itself to be in some measure periodical, and generally was most violent at the break of day, at noon, and at midnight.

About four o'clock in the morning of the 16th, the crater of Vesuvius began to shew signs of being open, by some black smoke issuing out of it; and at daybreak another smoke, tinged with red, issuing from an opening near the crater, but on the other side of the mountain, and facing the town of Ottaiano, shewed that a new mouth had opened there, and from which, as we heard afterwards, a considerable stream of lava issued, and ran with great velocity through a wood, which it burnt; and having run about three miles in a few hours, it stopped before it had arrived at the vineyards and cultivated lands. The crater, and all the conical part of Vesuvius, was soon involved in clouds and darkness, and so it remained for several days; but above these clouds, although of a great height, we could often discern fresh columns of smoke from the crater, rising furiously still higher, until the whole mass remained in the usual form of a pine tree; and in that gigantic mass of heavy clouds the *ferilli*, or volcanic lightning, was frequently visible, even in the day time. About five o'clock in the morning of the 16th we could plainly perceive, that the lava which had first broke out from the several new mouths on the south side of the mountain, had reached the sea, and was running into it, having overwhelmed, burnt, and destroyed the greatest part of Torre del Greco, the principal stream of lava having taken its course through the very centre of the town. We observed from Naples, that when the lava was in the vineyards in its way to the town, there issued often, and in different parts of it, a bright pale flame, and very different from the deep red of the lava; this was occasioned by the burning of the trees that supported the vines. Soon after the beginning of this eruption, ashes fell thick at the foot of the mountain, all the way from Portici to the Torre del Greco; and what is remarkable, although there were not at that time any clouds in the air, except those of smoke from the mountain, the ashes were wet, and accompanied with large drops of water, which, as I have been well assured, were to the taste very salt; the road, which is paved, was as wet as if there had been a heavy shower of rain. Those ashes were black and coarse, like the sand of the sea shore, whereas those that fell there, and at Naples some days after, were of a light-grey colour, and as fine as Spanish snuff, or powdered bark. They contained many saline particles; as I observed, when I went to the town of Torre del Greco on the 17th of June, that those ashes that lay on the ground, exposed to the burning sun, had a coat of the whitest powder on their surface, which to the taste was extremely salt and pungent. In the printed account of the late eruption by Emanuel Scotti, doctor of physic and professor of philosophy in the university of Naples, he supposes (which appears to be highly probable) that the water which accompanied the fall of the ashes at the beginning of the eruption, was produced by the mixture of the inflammable and dephlogisticated air, according to experiments made by Doctor Priestley and Monsieur Lavoisier.

By the time that the lava had reached the sea, between five and six o'clock in the morning of the 16th, Vesuvius was so completely involved in darkness, that we could no more discern the violent operation of nature that was going on there, and so it remained for several days; but the dreadful noise we heard at times, and the red tinge on the clouds over the top of the mountain, were evident signs of the activity of the fire underneath. The lava ran but slowly at Torre del Greco after it had reached the sea; and on the 17th of June in the morning, when I went in my boat to visit that unfortunate town, its course was stopped, excepting that at times a little rivulet of liquid fire issued from under the smoking scoriæ into the sea, and caused a hissing noise, and a white vapour smoke; at other times, a quantity of large scoriæ were pushed off the surface of the body of the lava into the sea, discovering that it was red hot under that surface; and even to this day the centre of the thickest part of the lava that covers the town retains its red heat. The breadth of the lava that ran into the sea, and has formed a new promontory there, after having destroyed the greatest part of the town of Torre del Greco, having been exactly measured by the Duke della Torre, is of English feet 1204. Its height above the sea is 12 feet, and as many feet under water; so that its whole height is 24 feet; it extends into the sea 626 feet. I observed that the sea water was boiling as in a cauldron, where it washed the foot of this new formed promontory; and although I was at least an hundred yards from it, observing that the sea smoked near my boat, I put my hand into the water, which was literally scalded; and by this time my boatmen observed that the pitch from the bottom of the boat was melting fast, and floating on the surface of the sea, and that the boat began to leak; we therefore retired hastily from this spot, and landed at some distance from the hot lava. The town of Torre del Greco contained about 18000 inhabitants, all of which (except about 15, who from either age or infirmity could not be moved, and were overwhelmed by the lava in their houses) escaped either to Castel-a-mare, which was the ancient Stabiæ, or to Naples; but the rapid progress of the lava was such, after it had altered its course from Resina, which town it first threatened, and had joined a fresh lava that issued from one of the new mouths in a vineyard, about a mile from the town, that it ran like a torrent over the town of Torre del Greco, allowing the unfortunate inhabitants scarcely time to save their lives; their goods and effects were totally abandoned, and indeed several of the inhabitants, whose houses had been surrounded with lava whilst they remained in them, escaped from them and saved their lives the following day, by coming out of the tops of their houses, and walking over the scoriæ on the surface of the red-hot lava. Five or six old nuns were taken out of a convent in this manner, on the 16th of June, and carried over the hot lava, as I was informed by the friar who assisted them; and who told me that their stupidity was such, as not to have been the least alarmed, or sensible of their danger: he found one of upwards of 90 years of age actually warming herself at a point of red-hot lava, which touched the window of her cell, and which she said was very comfortable; and though now apprized of their danger, they were still very unwilling to leave the convent, in which they had been shut up almost from their infancy, their ideas being as limited as the space they inhabited. Having desired them to pack up whatever they had that was most valuable, they all loaded themselves with biscuits and sweetmeats, and it was but by accident that the friar discovered that they had left a sum of money behind them, which he recovered for them; and these nuns are now in a convent at Naples.

At the time I landed at Torre del Greco on the 17th, I found some few of its inhabitants returned, and endeavouring to recover their effects from such houses as had not been thrown down, or were not totally buried under the lava; but alas! what was their cruel disappointment when they found that their houses had been already broke open, and completely gutted of every thing that was valuable; and I saw a scuffle at the door of one house, between the proprietors, and the robbers who had taken possession of it. The lava had passed over the centre and best part of the town; no part of the cathedral remained above it, except the upper part of a square brick tower, in which are the bells; and it is a curious circumstance that those bells, although they are neither cracked or melted, are deprived of their tone as much as if they had been cracked, I suppose by the action of the acid and vitriolic vapours of the lava. Some of the inhabitants of Torre del Greco told me, that when the lava first entered the sea, it threw up the water to a prodigious height; and particularly when two points of lava met and inclosed a pool of water, that then that water was thrown up with great violence, and a loud report: they likewise told me, that at this time, as well as the day after, a great many boiled fish were seen floating on the surface of the sea; and I have since been assured by many of the fishermen of Portici, Torre del Greco, and Torre dell' Annunziata (all of which towns are situated at the foot of Vesuvius), that they could not for many days during the eruption catch a fish within two miles of that coast, which they had evidently deserted.

When this lava is cooled sufficiently, which may not be until some months hence, I shall be curious to examine whether the centre, or solid and compact parts, of the lava that ran into the sea has taken, as it probably may, the prismatical form of basalt columns, like many other ancient lavas disgorged into the water. The exterior of this lava at present, like all others, offers to the eye nothing but a confused heap of loose scoriæ. The lava over the cathedral, and in other parts of the town, is upwards of 40 feet in thickness; the general height of the lava during its whole course is about 12 feet, and in some parts not less than a mile in breadth. I walked in the few remaining streets of the town, and I went on the top of one of the highest houses that was still standing, although surrounded by the lava; I saw from thence distinctly the whole course of the lava, that covered the best part of the town; the tops of the houses were just visible here and there in some parts, and the timbers within still burning caused a bright flame to issue out of the surface; in other parts, the sulphur and salts exhaled in a white smoke from the lava, forming a white or yellow crust on the scoriæ round the spots where it issued with the most force. Often I heard little explosions, and saw that they blew up, like little mines, fragments of the scoriæ and ashes into the air; I suppose them to have been occasioned either by rarefied air in confined cellars, or perhaps by small portions of gunpowder taking fire, as few in this country are without a gun and some little portion of gunpowder in their houses. As the church feasts are here usually attended with fireworks and crackers, a firework-maker of this town had a very great quantity of fireworks ready made for an approaching feast, and some gunpowder, all of which had been shut up in his house by the lava, a part of which had even entered one of the rooms; yet he actually saved all his fireworks and gunpowder some days after, by carrying them safely over the hot lava. I should not have been so much at my ease had I known of this gunpowder, and of several other barrels that were at the same time in the cellar of another house, inclosed by the lava, and which were afterwards brought off on women's heads, little thinking of their danger, over the scoriæ of the lava, that was red-hot underneath. The heat in the streets of the town, at this time, was so great as to raise the quicksilver of my thermometer to very near 100 degrees, and close to the hot lava it rose much higher; but what drove me from this melancholy spot was, that one of the robbers with a great pig on his shoulders, pursued by the proprietor with a long gun pointed at him, kept dodging round me to save himself; I bid him throw down the pig and run, which he did; and the proprietor, satisfied with having recovered his loss, acquainted me with my danger, by telling me that there were now thieves in every house that was left standing. I thought it therefore high time to retire, both for my own safety, and that I might endeavour to procure from Naples some protection for the doubly unfortunate sufferers of this unhappy town. Accordingly I returned to Naples in my boat, and immediately acquainted this government with what I had just seen myself;

in consequence of which a body of soldiers was sent directly to their relief by sea, the road by land having been cut off by the lava. I remarked in my way home, that there was a much greater quantity of the petroleum floating on the surface of the sea, and diffusing a very strong and offensive smell, than was usual; for at all times in calms, patches of this bituminous oil, called here petroleum, are to be seen floating on the surface of the sea between Portici and Naples, and particularly opposite a village called Pietra Bianca. The minute ashes continued falling all this day at Naples; the mountain, totally obscured by them, continued to alarm us with repeated loud explosions; the streets of this city were this day and the next constantly filled with religious and penitential processions, composed of all classes, and nothing was heard in the midst of darkness but the thunder of the mountain, and *ora pro nobis*. The sea wind increasing at times, delivered us from these ashes, which it scattered over different parts of the Campagna Felice.

On Wednesday the 18th, the wind having for a very short space of time cleared away the thick cloud from the top of Vesuvius, we discovered that a great part of its crater, particularly on the west side opposite Naples, had fallen in, which it probably did about four o'clock in the morning of this day, as a violent shock of an earthquake was felt at that moment at Resina, and other parts situated at the foot of the volcano. The clouds of smoke, mixed with the ashes which, as I have before remarked, were as fine as Spanish snuff (so much so that the impression of a seal with my coat of arms would remain distinctly marked upon them), were of such a density as to appear to have the greatest difficulty in forcing their passage out of the now widely extended mouth of Vesuvius, which certainly, since the top fell in, cannot be much short of two miles in circumference. One cloud heaped on another, and succeeding one another incessantly, formed in a few hours such a gigantic and elevated column of the darkest hue over the mountain, as seemed to threaten Naples with immediate destruction, having at one time been bent over the city, and appearing to be much too massive and ponderous to remain long suspended in the air; it was besides replete with the *ferilli*, or volcanic lightning, which was stronger than common lightning, just as PLINY the younger describes it in one of his letters to TACITUS, when he says *fulgoribus illæ et similes et majores erant.*

Vesuvius was at this time completely covered, as were all the old black lavas, with a thick coat of these fine light-grey ashes already fallen, which gave it a cold and horrid appearance; and in comparison of the abovementioned enormous mass of clouds, which certainly, however it may contradict our idea of the extension of our atmosphere, rose many miles above the mountain, it appeared like a mole-hill; although, as you know, Sir, the perpendicular height of Vesuvius from the level of the sea, is more than three thousand six hundred feet. The Abbé BRACCINI, as appears in his printed account of the eruption of Mount Vesuvius in 1631, measured with a quadrant the elevation of a mass of clouds of the same nature, that was formed over Vesuvius during that great eruption, and found it to exceed thirty miles in height. Doctor SCOTTI, in his printed account of this eruption, says that the height of this threatening cloud of smoke and ashes, measured (but he does not say how) from Naples, was found to be of an elevation of thirty degrees. All I can say is, that to my eye

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the distance from the crater of Vesuvius to the most elevated part of the cloud, appeared to me nearly the same as that of the island of Caprea from Naples, and which is about 25 miles; but I am well aware of the inaccuracy of such a sort of measurement. At the time of its greatest elevation, I engaged Signor Gatta, successor to the late ingenious Mr. Fabris, to make an exact drawing of it, which he did with great success; and a copy of that drawing on a small scale is inclosed (Tab. VII.), and will, I hope, give you a very good idea of what I have been describing.

I must own, that at that moment I did apprehend Naples to be in some danger of being buried under the ashes of the volcano, just as the towns of Herculaneum and Pompeii were in the year 79. The ashes that fell then at Pompeii were of the same fine quality as those from this eruption; having often observed, when present at the excavations of that ancient city, that the ashes, which I suppose to have been mixed with water at the same time, had taken the exact impression or mould of whatever they had inclosed; so that the compartments of the wood work of the windows and doors of the houses remained impressed on this volcanic tufo, although the wood itself had long decayed, and not an atom of it was to be seen, except when the wood had been burnt, and then you found the charcoal. Having once been present at the discovery of a skeleton in the great street of Pompeii, of a person who had been shut up by the ashes during the eruption of 79, I engaged the men that were digging to take off the piece of hardened tufo, that covered the head, with great care, and, as in a mould just taken off in plaster of Paris, we found the impression of the eyes, that were shut, of the nose, mouth, and of every feature perfectly distinct. A similar specimen of a mould of this kind, brought from Pompeii, is now in his Sicilian Majesty's museum at Portici; it had been formed over the breast of a young woman that had been shut up in the volcanic matter; every fold of a thin drapery that covered her breast is exactly represented in this mould: and in the volcanic tufo that filled the ancient theatre of Herculaneum, the exact mould or impression of the face of a marble bust is still to be seen, the bust or statue having been long since removed. Having observed these fine ashes issuing in such abundance from Vesuvius, and having the appearance of being damp or wet, as you may perceive by the drawing (Tab. VII.) that they do not take such beautiful forms and volutes as a fine dry smoke usually does, but appear in harsh and stiff little curls, you will not wonder then, that the fate of Herculaneum and Pompeii should have come again strongly into my mind; but fortunately the wind sprung up fresh from the sea, and the threatening cloud bent gradually from us over the mountain of Somma, and involved all that part of the Campagna in obscurity and danger.

To avoid prolixity and repetition, I need only say, that the storms of thunder and lightning, attended at times with heavy falls of rain and ashes, causing the most destructive torrents of water and glutinous mud, mixed with huge stones, and trees torn up by the roots, continued more or less to afflict the inhabitants on both sides of the volcano until the 7th of July, when the last torrent destroyed many hundred acres of cultivated land, between the towns of Torre del Greco and Torre dell' Annunziata. Some of these torrents, as I have been credibly assured by eye witnesses, both on the sea side and the Somma side of the mountain, came down with a horrid rushing noise; and some of them, after having forced their way through the narrow gullies of the mountain, rose to the height of more than 20 feet, and were near half a mile in extent. The mud of which the torrents were composed, being a kind of natural mortar, has completely cased up, and ruined for the present, some thousand acres of rich vineyards; for it soon becomes so hard, that nothing less than a pick-axe can break it up; I say for the present, as I imagine that hereafter the soil may be greatly improved by the quantity of saline particles that the ashes from this eruption evidently contain. A gentleman of the British factory at Naples, having filled a plate with the ashes that had fallen on his balcony during the eruption, and sowed some pease in them, assured me that they came up the third day, and that they continue to grow much faster than is usual in the best common garden soil.

My curiosity, or rather my wish to gratify that of our respectable Society, induced me to go upon Mount Vesuvius, as soon as I thought I might do it with any degree of prudence, which was not until the 30th of June, and then it was attended with some risk, as will appear in the course of this narrative. The crater of Vesuvius, except at short intervals, had been continually obscured by the volcanic clouds ever since the 16th, and was so this day, with frequent flashes of lightning playing in those clouds, and attended as usual with a noise like thunder; and the fine ashes were still falling on Vesuvius, but still more on the mountain of Somma. I went up the usual way by Resina, attended by my old Cicerone of the mountain, Bartolomeo Pumo, with whom I have been sixty-eight times on the highest point of Vesuvius. I observed in my way through the village of Resina that many of the stones of the pavement had been loosened, and were deranged by the earthquakes, particularly by that of the 18th, which attended the falling in of the crater of the volcano, and which, as they told me there, had been so violent as to throw many people down, and obliged all the inhabitants of Resina to quit their houses hastily, and to which they did not dare return for two days. The leaves of all the vines were burnt by the ashes that had fallen on them, and many of the vines themselves were buried under the ashes, and great branches of the trees that supported them had been torn off by their weight. In short, nothing but ruin and desolation was to be seen. The ashes at the foot of the mountain were about 10 or 12 inches thick on the surface of the earth, but in proportion as we ascended their thickness increased to several feet, I dare say not less than 9 or 10 in some parts; so that the surface of the old rugged lavas, that before was almost impracticable, was now become a perfect plain, over which we walked with the greatest ease. The ashes were of a light-grey colour, and exceedingly fine, so that by the footsteps being marked on them as on snow, we learnt that three small parties had been up before us. We saw likewise the track of a fox, that appeared to have been quite bewildered, to judge from the many turns he had made. Even the traces of lizards and other little animals, and of insects, were visible on these fine ashes. We ascended to the spot from whence the lava of the 15th first issued, and we followed the course of it, which was still very hot (although covered with such a thick coat of ashes), quite down to the sea at Torre del Greco, which is more than five miles. A pair of boots, to which I had for the purpose added a new and thick sole, were burnt through on this expedition. It was not possible to get up to the great crater of Vesuvius, nor had any one yet attempted it. The horrid chasms that exist from the spot where the late eruption first took place, in a straight line for near two miles towards the sea, cannot be imagined. They formed vallies more than two hundred feet deep, and from half to a mile wide; and where the fountains of fiery matter existed during the eruption, are little mountains with deep craters. Ten thousand men, in as many years, could not, surely, make such an alteration on the face of Vesuvius, as has been made by nature in the short space of five hours. Except the exhalations of sulphureous and vitriolic vapours, which broke out from different spots of the line abovementioned, and tinged the surface of the ashes and scoriæ in those parts with either a deep or pale yellow with a reddish ochre colour, or a bright white, and in some parts with a deep green and azure blue (so that the whole together had the effect of an iris), all around us had the appearance of a sandy desert. We went on the top of seven of the most considerable of the new-formed mountains, and looked into their craters, which on some of them appeared to be little short of half a mile in circumference; and although the exterior perpendicular height of any of them did not exceed two hundred feet, the depth of their inverted cone within was three times as great. It would not have been possible for us to have breathed on these new mountains near their craters, if we had not taken the precaution of tying a doubled handkerchief over our mouths and nostrils; and even with that precaution we could not resist long, the fumes of the vitriolic acid were so exceedingly penetrating, and of such a suffocating quality. We found in one a double crater, like two funnels joined together; and in all there was some little smoke and depositions of salts and sulphurs, of the various colours above mentioned, just as is commonly seen adhering to the inner walls of the principal crater of Vesuvius.

Two or three days after we had been here, one of the new mouths into which we had looked, suddenly made a great explosion of stones, smoke, and ashes, which would certainly have proved fatal to any one who might unfortunately have been there at the time of the explosion. We read of a like accident having proved fatal to more than twenty people, who had the curiosity to look into the crater of the Monte Nuovo, near Pozzuoli, a few days after its formation, in the year 1538. The 15th of August, I saw a sudden explosion of smoke and ashes, thrown to an extreme height out of the great crater of Vesuvius, that must have destroyed any one within half a mile of it; and yet on the 19th of July a party not only had visited that crater, but had descended 170 feet within it. Whilst we were on the mountain, two whirlwinds, exactly like those that form water-spouts at sea, made their appearance; and one of them that was very near us made a strange rushing noise, and having taken up a great quantity of the fine ashes, formed them into an elevated spiral column, which, with a whirling motion and great rapidity, was carried towards the mountain of Somma, where it broke and was dispersed. As there were evident signs of an abundance of electricity in the air at this time, I have no doubt of this having been also an electrical operation. One of my servants, employed in collecting of sulphur, or sal ammoniac, which crystallizes near the *fumaroli,* as they are called here (and which are the spots from whence the hot vapour issues out of the fresh lavas), found to his great surprise an exceeding cold wind issue from a fissure very near the hot *fumaroli* abovementioned upon his leg; I put my hand to the spot, and found the same; but it did not surprise me, as before on Mount Vesuvius, on the mountain of Somma, on Mount Etna, and in the island of Ischia, I had met with, on particular spots, the like currents of extreme cold air issuing from beneath the ancient lavas, and which, being constant to those spots, are known by the name of *ventoroli*. In a vineyard not in the same line with the new-formed mountains just described, but in a right line from them, at the distance of little more than a mile from Torre del Greco, are three or four more of these new-formed mountains with craters, out of which the lava flowed, and by uniting with the streams that came from the higher mouths, and adding to their heat and fluidity, enabled the whole current to make so rapid a progress over the unfortunate town, as scarcely to allow its inhabitants sufficient time to escape with their lives. The rich vineyards belonging to the Torre del Greco, and which produced the good wine called *Lacrima Christi*, that have been buried, and are totally destroyed by this lava, consisted, as I have been informed, of more than three thousand acres; but the destruction of the vineyards by the torrents of mud and water at the foot of the mountain of Somma, is much more extensive.

I visited that part of the country also a few days after I had been on Vesuvius, not being willing to relate to you any one circumstance of the late formidable eruption but what I had reason to believe was founded on truth. The first signs of a torrent that I met with, was near the village of the Madonna dell' Arco, and I passed several others between that and the town of Ottaiano; the one near Trochia, and two near the town of Somma, were the most considerable, and not less than a quarter of a mile in breadth; and as several eye witnesses assured me on the spot, were, when they poured down from the mountain of Somma, from 20 to 30 feet high; it was a liquid glutinous mud, composed of scoriæ, ashes, stones (some of which of an enormous size) mixed with trees that had been torn up by the roots. Such torrents, as you may well imagine, were irresistible, and carried all before them; houses, walls, trees, and, as they told me, not less than four thousand sheep and other cattle, had been swept off by the several torrents on that side of the mountain. At Somma they likewise told me that a team of eight oxen, that were drawing a large timber tree, had been carried off from thence, and never were more heard of.

The appearance of these torrents, when I saw them, was like that of all other torrents in mountainous countries, except that what had been mud was become a perfect cement, on which nothing less than a pick-axe could make any impression. The vineyards and cultivated lands were here much more ruined; and the limbs of the trees much more torn by the weight of the ashes, than those which I have already described on the sea side of the volcano.

The Abbé Tata, in his printed account of this eruption, has given a good idea of the abundance, the great weight, and glutinous quality of these ashes, when he says that having taken a branch from a fig-tree still standing near the town of Somma, on which were only six leaves, and two little unripe figs, and having weighed it with the ashes attached to it, he found it to be 31 ounces; when having washed off the volcanic matter, it scarcely weighed 3 ounces.

I saw several houses on the road, in my way to the town of Somma, with their roofs beaten in by the weight of the ashes. In the town of Somma, I found four churches and about seventy houses without roofs, and full of ashes. The great damage on this side of the mountain, by the fall of the ashes and the torrents, happened on the 18th, 19th, and 20th of June, and on the 12th of July. I heard but of three lives that had been lost at Somma by the fall of a house. The 19th, the ashes fell so thick at Somma (as they told me there), that unless a person kept in motion, he was soon fixed to the ground by them. This fall of ashes was accompanied also with loud reports, and frequent flashes of the volcanic lightning, so that, surrounded by so many horrors, it was impossible for the inhabitants to remain in the town, and they all fled; the darkness was such, although it was mid-day, that even with the help of torches it was scarcely possible to keep in the high road; in short, what they described to me was exactly what PLINY the younger and his mother had experienced at Misenum during the eruption of Vesuvius in the reign of TITUS, according to his second letter to TACITUS on that subject. I found that the majority of people here were convinced that the torrents of mud and water, that had done them so much mischief, came out of the crater of Vesuvius, and that it was sea-water; but there cannot be any doubt of those floods having been occasioned by the sudden dissolution of watery clouds mixed with ashes, the air perhaps having been too much rarefied to support them; and when such clouds broke, and fell heavily on Vesuvius, the water not being able to penetrate as usual into the pores of the earth, which were then filled up with the fine ashes of a bituminous and oily quality, nor having free access to the channels which usually carried it off, accumulated in pools, and mixing with more ashes, rose to a great height, and at length forced its way through new channels, and came down in torrents over countries where it was least expected, and spread itself over the fertile lands at the foot of the mountain. From what I have seen lately, I begin to doubt very much if the water, by which so much damage was done, and so many lives were lost during the terrible eruption of Vesuvius in 1631, did really, as was generally supposed, come out of the crater of the volcano: sentiments were divided then, as they are now, on that subject; and since in all great eruptions the crater of the volcano must be obscured by the clouds of ashes, as it probably was then, and certainly was during the violence of the late eruption, therefore it must be very difficult to ascertain exactly from whence that water came. The more extraordinary a circumstance is, the more it appears to be the common desire that it should be credited; from this principle, one of his Sicilian Majesty's gardeners of Portici went up to the crater of Vesuvius as soon as it was practicable, and came down in a great fright, declaring that he had seen it full of boiling water. The Chevalier Macedonio, intendant of Portici, judged very properly, that to put an end to the alarm this report had spread over the country, it was necessary to send up people he could trust, and on whose veracity he might depend. Accordingly the next day, which was the 16th of July, Signor Guiseppe Sacco went up, well attended, and proved the gardener's assertion to be absolutely false, there being only some little signs of mud from a deposition of the rain water at the bottom of the crater. According to SACCO'S account, which has been printed at Naples, the crater is of an irregular oval form, and, as he supposes (not having been able to measure it) of about a mile and an half in circumference; by my eye I should judge it to be more; the inside, as usual, in the shape of an inverted cone, the inner walls of which on the eastern side are perpendicular; but on the western side of the crater, which is much lower, the descent was practicable, and SACCO with some of his companions actually went down 176 palms, from which spot, having lowered a cord with a stone tied to it, they found the whole depth of the crater to be about 500 palms. But such observations on the crater of Vesuvius are of little consequence, as both its form and apparent depth are subject to great alterations from day to day. These curious observers certainly ran some risk at that time, since which such a quantity of scoriæ and ashes have been thrown up from the crater, and even so lately as the 15th of this month, as must have proved fatal to any one within their reach.

The 22d of July, one of the new craters, which is the nearest to the town of Torre del Greco, threw up both fire and smoke, which circumstance, added to that of the lava's retaining its heat much longer than usual, seems to indicate that there may still be some fermentation under that part of the volcano. The lava in cooling often cracks, and causes a loud explosion, just as the ice does in the Glaciers in Switzerland; such reports are frequently heard now at the Torre del Greco; and as some of the inhabitants told me, they often see a vapour issue from the body of the lava, and taking fire in air, fall like those meteors vulgarly called falling stars.

The darkness occasioned by the fall of the ashes in the Campagna Felice extended itself, and varied, according to the prevailing winds. On the 19th of June it was so dark at Caserta, which is 15 miles from Naples, as to oblige the inhabitants to light candles at mid-day; and one day during the eruption, the darkness spread over Beneventum, which is 30 miles from Vesuvius.

The Archbishop of Taranto, in a letter to Naples, and dated from that city the 18th of June, said, "We are involved in a "thick cloud of minute volcanic ashes, and we imagine that "there must be a great eruption either of Mount Etna, or of "Stromboli." The bishop did not dream of their having proceeded from Vesuvius, which is about 250 miles from Taranto. We have had accounts also of the fall of the ashes during the late eruption at the very extremity of the province of Lecce, which is still farther off; and we have been assured likewise, that those clouds were replete with electrical matter: at Martino, near Taranto, a house was struck and much damaged by the lightning from one of these clouds. In the accounts of the great eruption of Vesuvius in 1631, mention is made of the extensive progress of the ashes from Vesuvius, and of the damage done by the *ferilli*, or volcanic lightning, which attended them in their course.

I must here mention a very extraordinary circumstance indeed, that happened near Sienna in the Tuscan state, about 18 hours after the commencement of the late eruption of Vesuvius on the 15th of June, although that phænomenon may have no relation to the eruption; and which was communicated to me in the following words by the Earl of Bristol, bishop of Derry, in a letter dated from Sienna, July 12th, 1794: "In

" the midst of a most violent thunder-storm, about a dozen " stones of various weights and dimensions fell at the feet of " different people, men, women, and children; the stones are " of a quality not found in any part of the Siennese territory; " they fell about 18 hours after the enormous eruption of Ve- " suvius, which circumstance leaves a choice of difficulties in " the solution of this extraordinary phænomenon: either these " stones have been generated in this igneous mass of clouds, " which produced such unusual thunder, or, which is equally " incredible, they were thrown from Vesuvius at a distance of " at least 250 miles; judge then of its parabola. The philoso- " phers here incline to the first solution. I wish much, Sir, to " know your sentiments. My first objection was to the fact " itself; but of this there are so many eye witnesses, it seems " impossible to withstand their evidence, and now I am re- " duced to a perfect scepticism." His lordship was pleased to send me a piece of one of the largest stones, which when entire weighed upwards of five pounds; I have seen another that has been sent to Naples entire, and weighs about one pound. The outside of every stone that has been found, and has been ascertained to have fallen from the cloud near Sienna, is evidently freshly vitrified, and is black, having every sign of having passed through an extreme heat; when broken, the inside is of a light-grey colour mixed with black spots, and some shining particles, which the learned here have decided to be pyrites, and therefore it cannot be a lava, or they would have been decomposed. Stones of the same nature, at least as far as the eye can judge of them, are frequently found on Mount Vesuvius; and when I was on the mountain lately, I searched for such stones near the new mouths, but as the soil round them has been covered with a thick bed of fine ashes, whatever was thrown up during the force of the eruption lies buried under those ashes. Should we find similar stones with the same vitrified coat on them on Mount Vesuvius, as I told Lord Bristol in my answer to his letter, the question would be decided in favour of Vesuvius; unless it could be proved that there had been, about the time of the fall of these stones in the Sanese territory, some nearer opening of the earth, attended with an emission of volcanic matter, which might very well be, as the mountain of Radicofani, within 50 miles of Sienna, is certainly volcanic. I mentioned to his lordship another idea that struck me. As we have proofs during the late eruption of a quantity of ashes of Vesuvius having been carried to a greater distance than where the stones fell in the Sanese territory, might not the same ashes have been carried over the Sanese territory, and mixing with a stormy cloud, have been collected together just as hailstones are sometimes into lumps of ice, in which shape they fall; and might not the exterior vitrification of those lumps of accumulated and hardened volcanic matter have been occasioned by the action of the electric fluid on them? The celebrated Father Ambrogio Soldani, professor of mathematics in the university of Sienna, is printing there his dissertation upon this extraordinary phænomenon; wherein, as I have been assured, he has decided that those stones were generated in the air independantly of volcanic assistance.

Until after the 7th of July, when the last cloud broke over Vesuvius, and formed a tremendous torrent of mud, which took its course across the great road between Torre del Greco and the Torre dell' Annunziata, and destroyed many vineyards, the late eruption could not be said to have finished, although the force of it was over the 22d of June, since which time the crater has been usually visible. The power of attraction in mountains is well known; but whether the attractive power of a volcanic mountain be greater than that of any other mountain, is a question: all I can say is, that during this last eruption every watery cloud has been evidently attracted by Vesuvius, and the sudden dissolution of those clouds has left such marks of their destructive power on the face of the country all round the basis of the volcano as will not soon be erased. Since the mouth of Vesuvius has been enlarged, I have seen a great cloud passing over it, and which not only was attracted, but was sucked in, and disappeared in a moment.

After every violent eruption of Mount Vesuvius, we read of damage done by a mephitic vapour, which coming from under the ancient lavas, insinuates itself into low places, such as the cellars and wells of the houses situated at the foot of the volcano. After the eruption of 1767, I remember that there were several instances, as in this, of people going into their cellars at Portici, and other parts of that neighbourhood, having been struck down by this vapour, and who would have expired if they had not been hastily removed. These occasional vapours, and which are called here *mofete*, are of the same quality as that permanent one in the Grotta del Cane, near the lake of Agnano, and which has been proved to be chiefly fixed air. The vapours, that in the volcanic language of this country are called *fumaroli*, are of another nature, and issue from spots all over the fresh and hot lavas whilst they are cooling; they are sulphureous and suffocating, so much so that often the birds that are flying over them are overpowered, and fall down dead; of which we have had many examples during this eruption, particularly of wood pigeons, that have been found dead on the lava. These vapours deposite a crust of sulphur, or salts, particularly of sal ammoniac, on the scoriæ of the lava through which they pass; and the small crystals of which they are composed are often tinged with a deep or pale yellow, with a bright red like cinnabar, and sometimes with green, or an azure blue. Since the late eruption, many pieces of the scoriæ of the fresh lava have been found powdered with a lucid substance, exactly like the brightest steel or iron filings.

The first appearance of the *mofete*, after the late eruption, was on the 17th of June, when a peasant going with an ass to his vineyard, a little above the village of Resina, in a narrow hollow way, the ass dropped down, and seemed to be expiring; the peasant was soon sensible of the mephitic vapour himself, and well knowing its fatal effects, dragged the animal out of its influence, and it soon recovered. From that time these vapours have greatly increased, and extended themselves. There are to this day many cellars and wells, all the way from Portici to Torre dell' Annunziata, greatly affected by them. This heavy vapour, when exposed to the open air, does not rise much more than a foot above the surface of the earth, but when it gets into a confined place, like a cellar or well, it rises and fills them as any other fluid would do; having filled a well, it rises above it about a foot high, and then bending over, falls to the earth, on which it spreads, always preserving its usual level. Wherever this vapour issues, a wavering in the air is perceptible, like that which is produced by the burning of charcoal; and when it issues from a fissure near any plants or vegetables, the leaves of those plants are seen to move, as if they were agitated by a gentle wind. It is extraordinary, that although there does not appear to be any poisonous quality in this vapour, which in every respect resembles fixed air, it should prove so very fatal to the vineyards, some thousand acres of which have been destroyed by it since the late eruption; when it penetrates to the roots of the vines, it dries them up, and kills the plant. A peasant in the neighbourhood of Resina having suffered by the *mofete*, which destroyed his vineyards in the year 1767, and having observed then that the vapour followed the laws of all fluids, made a narrow deep ditch all round his vineyard, which communicated with ancient lavas, and also to a deep cavern under one of them; the consequence of his well reasoned operation has been, that although surrounded at present by these noxious vapours, and which lie constantly at the bottom of his ditch, they have never entered his vineyard, and his vines are now in a flourishing state, whilst those of his neighbours are perishing. Upwards of thirteen hundred hares, and many pheasants and partridges, overtaken by this vapour, have been found dead within his Sicilian Majesty's reserved chases in the neighbourhood of Vesuvius; and also many domestic cats, who in their pursuit after this game fell victims to the *mofete*. A few days ago a shoal of fish, of several hundred weight, having been observed by some fishermen at Resina in great agitation on the surface of the sea, near some rocks of an ancient lava that had run into the sea, they surrounded them with their nets, and took them all with ease, and afterwards discovered that they had been stunned by the mephitic vapour, which at that time issued forcibly from underneath the ancient lava into the sea. I have been assured by many fishermen, that during the force of the late eruption the fish had totally abandoned the coast from Portici to the Torre dell' Annunziata, and that they could not take one in their nets nearer the shore than two miles. The divers there, who fish for the *ancini* (which we call sea eggs) and other shell fish, likewise told me, that for the space of a mile from that shore, since the eruption, they have found all the fish dead in their shells, as they suppose either from the heat of the sand at the bottom of the sea, or from poisonous vapours. The divers at Naples complain of their finding also many of these shell fish, or as they are called here in general terms, *frutti di mare*, dead in their shells.

I thought that these little well attested facts might contribute to show the great force of the wonderful chemical operation of nature that has lately been exhibited here. The *mofete*, or fixed air vapours, must certainly have been generated by the action of the vitriolic acid upon the calcareous earth, as both abound in Vesuvius. The sublimations, which are visibly operating by the chemistry of nature all along the course of the last lava that ran from Vesuvius, and particularly in and about the new mouths that have been formed by the late eruption on the flanks of the volcano, having been analyzed by Signor Domenico Tomaso, an ingenious chemist of Naples, and whose experiments, and the result of them, are now published, have been found to be chiefly sal ammoniac, mixed with a small quantity of the calx of iron: but not to betray my ignorance on this subject, and pretending to nothing more than the being an exact ocular observer, I refer you to the work itself, which accompanies this letter. Many hundred weight of the Vesuvian sal ammoniac have been collected on the mountain since the late eruption by the peasants, and sold at

Naples to the refiners of metals; at first it was sold for about six pence a pound, but, from its abundance, the price is now reduced to half that money; and a much greater quantity must have escaped in the air by evaporation.

The situation of Mount Vesuvius so near a great capital, and the facility of approaching it, has certainly afforded more opportunities of watching the operations of an active volcano, and of making observations upon it, than any other volcano on the face of the earth has allowed of. The Vesuvian diary, which by my care has now been kept with great exactness, and without interruption for more than 15 years, by the worthy and ingenious Padre ANTONIO PIAGGI, as mentioned in the beginning of this letter, and which it is my intention to deposite in the library of the Royal Society, will also throw a great light upon this curious subject. But as there is every reason to believe, with SENECA,* that the seat of the fire that causes these eruptions of volcanoes is by no means superficial, but lies deep in the bowels of the earth, and where no eye can penetrate, it will, I fear, be ever much beyond the reach of the limited human understanding to account for them with any degree of accuracy. There are modern philosophers who propose, with as great confidence, the erecting of conductors to prevent the bad effects of earthquakes and volcanoes, and who promise themselves the same success as that which has attended Doctor FRANKLIN's conductors of lightning; for, as they say, all proceed from one and the same cause, *electricity*. When we reflect how many parts of the earth already inhabited have evidently been thrown up from the bottom of the

* "Non ipse ex se est, sed in aliqua inferna valle conceptus exæstuat, et alibi pascitur; *in ipso monte non alimentum habet, sed viam*."—SENECA, Epist. 79.

sea by volcanic explosions, and the probability of there being a much greater portion under the same predicament, as yet unexplored, the vain pretensions of weak mortals to counteract such great operations, carried on surely for the wisest purposes by the beneficent Author of nature, appear to me to be quite ridiculous.

Let us then content ourselves with seeing, as well as we can, what we are permitted to see, and reason upon it to the best of our limited understandings, well assured that whatever is, is right.

The late sufferers at Torre del Greco, although his Sicilian Majesty, with his usual clemency, offered them a more secure spot to rebuild their town on, are obstinately employed in rebuilding it on the late and still smoking lava that covers their former habitations; and there does not appear to be any situation more exposed to the numerous dangers that must attend the neighbourhood of an active volcano than that of Torre del Greco. It was totally destroyed in 1631; and in the year 1737 a dreadful lava ran within a few yards of one of the gates of the town, and now over the middle of it; nevertheless, such is the attachment of the inhabitants to their native spot, although attended with such imminent danger, that of 18000 not one gave his vote to abandon it. When I was in Calabria, during the earthquakes in 1783, I observed in the Calabrese the same attachment to native soil; some of the towns that were totally destroyed by the earthquakes, and which had been ill situated in every respect, and in a bad air, were to be rebuilt; and yet it required the authority of government to oblige the inhabitants of those ruined towns to change their situation for a much better.

Upon the whole, having read every account of the former eruptions of Mount Vesuvius, I am well convinced that this eruption was by far the most violent that has been recorded after the two great eruptions of 79 and 1631, which were undoubtedly still more violent and destructive. The same phænomena attended the last eruption as the two former above mentioned, but on a less scale, and without the circumstance of the sea having retired from the coast. I remarked more than once, whilst I was in my boat, an unusual motion in the sea during the late eruption. On the 18th of June I observed, and so did my boatman, that although it was a perfect calm, the waves suddenly rose and dashed against the shore, causing a white foam, but which subsided in a few minutes. On the 15th, the night of the great eruption, the corks that support the nets of the royal tunny fishery at Portici, and which usually float upon the surface of the sea, were suddenly drawn under water, and remained so for a short space of time, which indicates, that either there must have been at that time a swell in the sea, or a depression or sinking of the earth under it.

From what we have seen lately here, and from what we read of former eruptions of Vesuvius, and of other active volcanoes, their neighbourhood must always be attended with danger; with this consideration, the very numerous population at the foot of Vesuvius is remarkable. From Naples to Castel-a-mare, about 15 miles, is so thickly spread with houses as to be nearly one continued street, and on the Somma side of the volcano, the towns and villages are scarcely a mile from one another; so that for thirty miles, which is the extent of the basis of Mount Vesuvius and Somma, the population may be perhaps more numerous than that of any spot of a like extent in Europe, in spite of the variety of dangers attending such a situation.

With the help of the drawings that accompany this account of the late eruption of Vesuvius, and which I can assure you to be faithful representations of what we have seen, I flatter myself I shall have enabled you to have a clear idea of it; and I flatter myself also, that the communication of such a variety of well attested phænomena as have attended this formidable eruption, may not only prove acceptable, but useful to the curious in natural history.

I have the honour to be, &c.

WM. HAMILTON.

IN a subsequent letter from Sir WILLIAM HAMILTON to Sir JOSEPH BANKS, dated *Castel-a-mare*, anciently *Stabiæ*, Sept. 2, 1794, are the two following remarks to be added to this paper.

1. Within a mile of this place the *mofete* are still very active, and particularly under the spot where the ancient town of Stabiæ was situated. The 24th of August, a young lad by accident falling into a well there that was dry, but full of the mephitic vapour, was immediately suffocated; there were no signs of any hurt from the fall, as the well was shallow. This circumstance called to my mind the death of the elder PLINY, who most probably lost his life by the same sort of mephitic vapours, on this very spot, and which are active after great eruptions of Vesuvius.

2. Mr. JAMES, a British merchant, who now lives in this neighbourhood, assured me that on Tuesday night, the 17th of

June, which was the third day of the eruption of Mount Vesuvius, he was in a boat with a sail, near Torre del Greco, when the minute ashes, so often mentioned in my letter, fell thick; and that in the dark they emitted a pale light like phosphorus, so that his hat, those of the boatmen, and the part of the sails that were covered with the ashes, were luminous. Others have mentioned to me the having seen a phosphoric light on Vesuvius after this eruption; but until it was confirmed to me by Mr. James, I did not choose to say any thing about it.

EXPLANATION OF THE PLATES.

Tab. V. Is a view of the eruption of Mount Vesuvius on the night of the 15th of June, 1794, taken from S. Lucia at Naples, when the eruption was in its greatest force.

Tab. VI. Is a view of the lava that destroyed the town of Torre del Greco, taken from a boat on the sea near that town, about five o'clock in the morning of the 16th of June, and whilst the lava was still advancing in the sea. The rocks, on which are two figures near the boat, were formed by a lava that ran into the sea during a former eruption of Mount Vesuvius.

Tab. VII. Is a view of the enormous cloud of smoke and ashes, replete with *ferilli,* or volcanic lightning, which first threatened destruction to the town of Naples on the 18th of June; and afterwards, from the impulse of the sea wind, bent over the mountain of Somma, and poured its destructive contents on the towns situated at the foot of that mountain, beating in the roofs of the houses, and involving all the inhabitants of the Campagna Felice in darkness and danger. This view was taken from Naples, and gives a very good idea of the appearance of Mount Vesuvius, like a mole-hill, in comparison of the enormous mass that hung over it.

Tab. VIII. Is a view of Mount Vesuvius, and of Somma, taken from Posilipo July 6th, 1794, when it could be clearly distinguished; the dotted lines shew the form of the top of Vesuvius as it was before this eruption, and when the crater was only from A to B; the present wide extended crater is sufficiently plain in the drawing not to need any further explanation; the spot from whence the lava first issued the night of the 15th of June, is marked C.

These four very exact drawings were taken from nature by Signor XAVERIO GATTA, successor to Signor PIETRO FABRIS.

Tab. IX. Is a drawing made by the Padre ANTONIO PIAGGI at Resina, during the force of the eruption of the 15th at night; and being within a mile and a half of the mountain, shews many particulars that escaped us, so much farther off at Naples; but he was interrupted by the imminent danger of his situation, and his drawing is incomplete: it was with difficulty that his friends carried him off alive, being upwards of 80 years old, in the midst of a shower of heavy cinders and sulphureous ashes, an hour after the beginning of the eruption; nor was he able to return to his house for many days. Nothing is necessary to be added to his Latin references to the drawing, but that Turris VIII. is *Torre del Greco*, and Retina, now *Resina*.

A. Montis vertex innubis, compositusque.

B. ad H. Sulci rudes inhianti terræ frequenter inscripti.

D. Ignei rivi fluentes Retinam versus.

E. Nitidissima flamma in cupressus formam altitudinem montis exsuperans.

F. Saxorum tempestas in altum a voraginibus erumpentium.

G. Lenis clivus igneum flumen in Retinam minantem avertens.

H. Semita ignei torrentis incredibili rapiditate Turrim VIII. invasuri.

I. Arbores, et vineta simplici illius afflatu a longe micantia.

K. Turris VIII. quæ Herculanio successisse creditur.

L. S^{te}. Mariæ Apulianæ templum.

M. Retina templo adhærens, recenter constructa, ab illo usque ad mare.

N. Porticus: nova item constructio Neapolim versus, unum corpus cum Retina efficiens.

O. Leucopetra.

P. Massa.

Q. Trochlea.

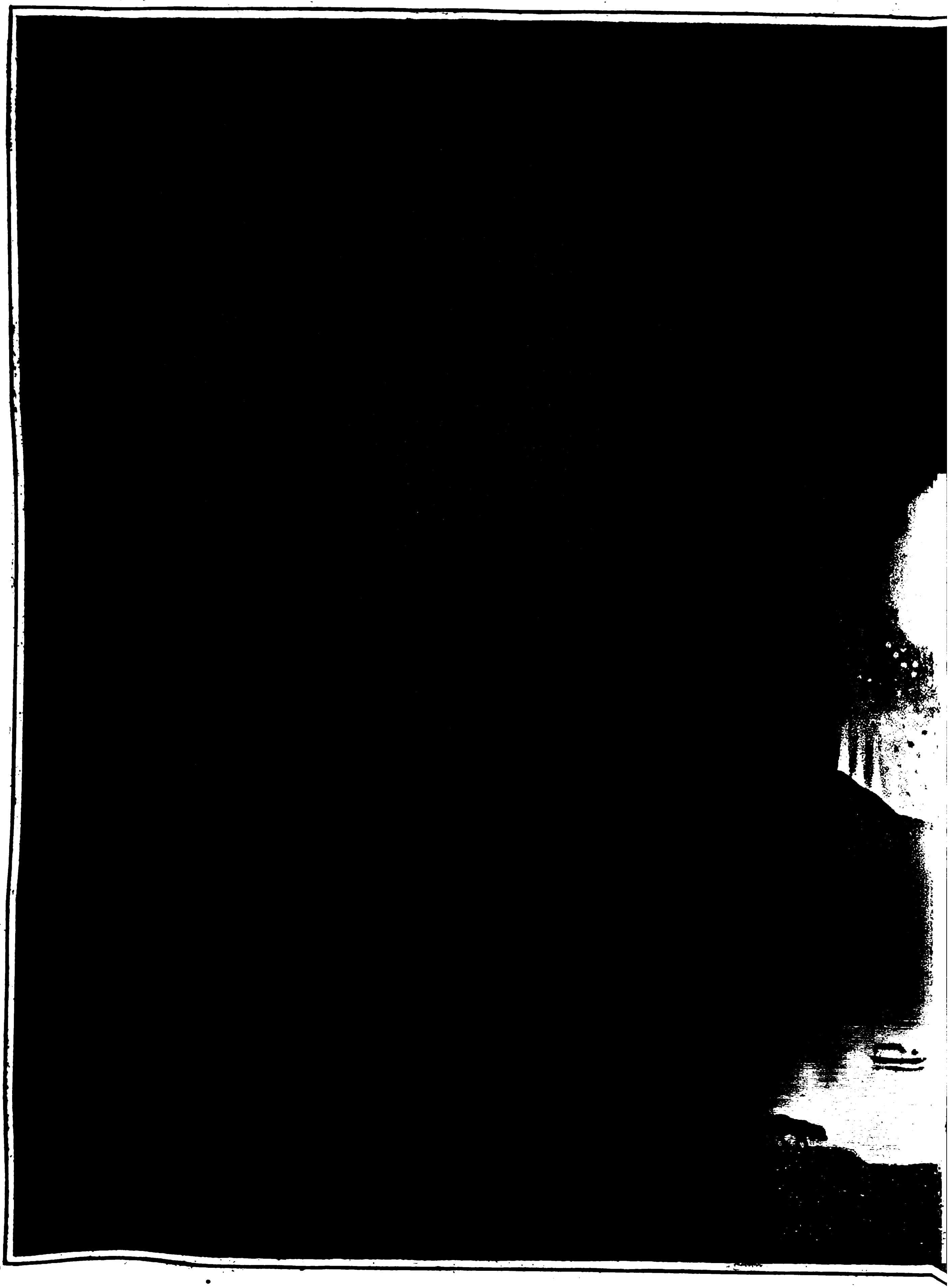
R. S^{ti}. Sebastiani vicus.

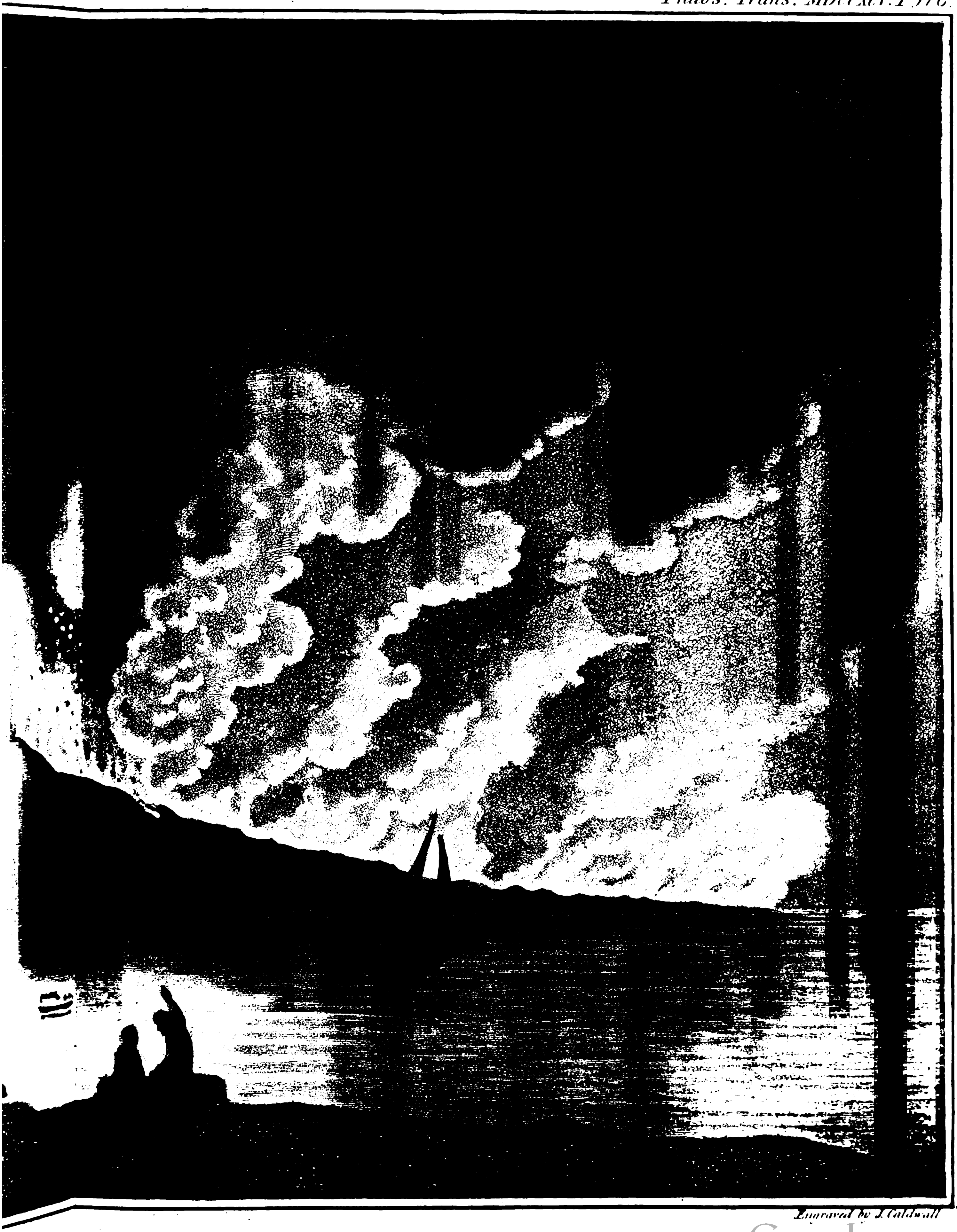
S. Fumus lapillis, asperis arenis, et aqua marina confertus in pluviam solutus.

Tab. X. Plan of the city of Torre del Greco, destroyed in great part by the lava which ran in the night of the 15th of June, 1794.

Tab. XI. Map of Mount Vesuvius and the adjacent places, with the course of the lava.

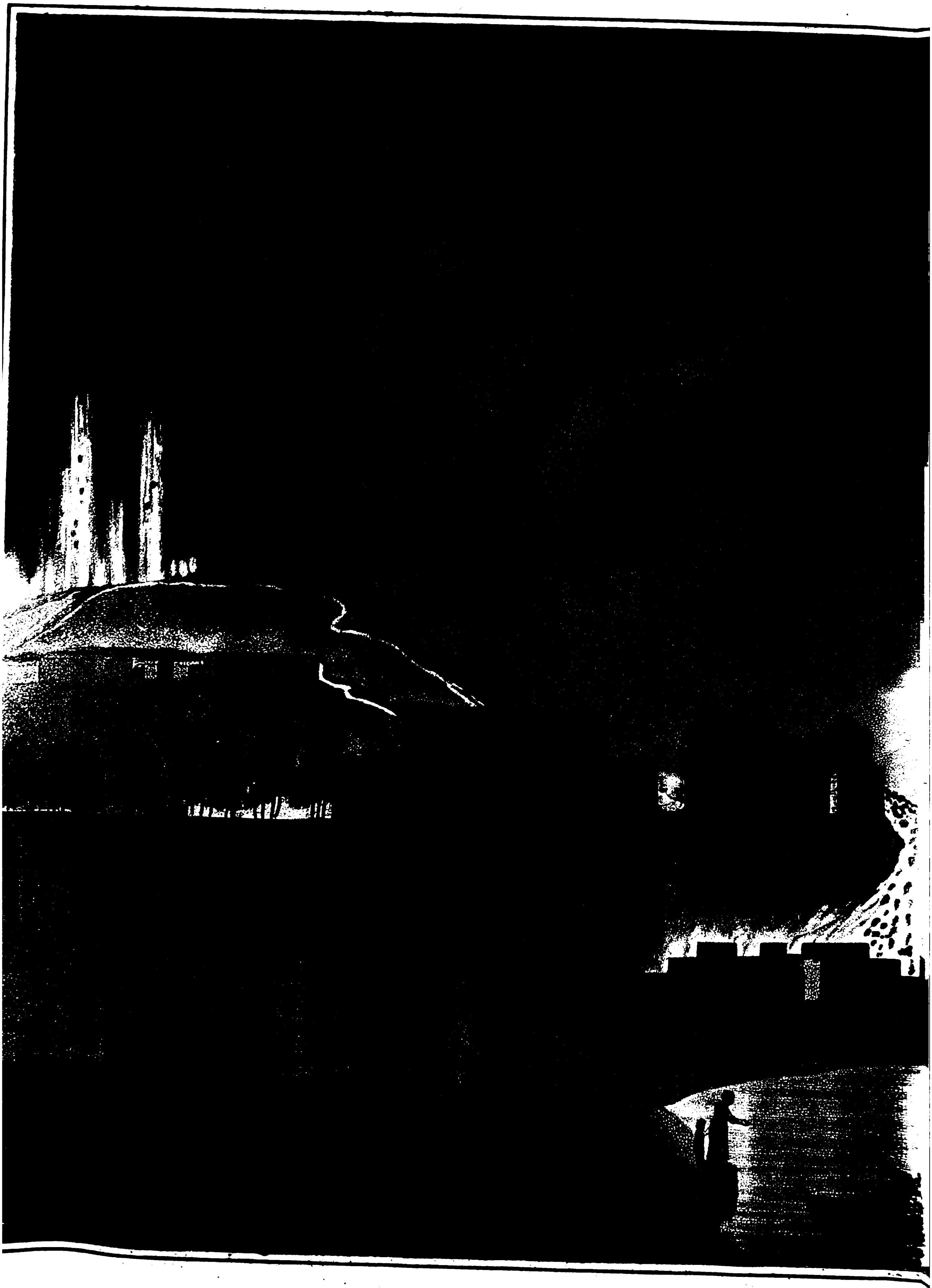
Tab. V.

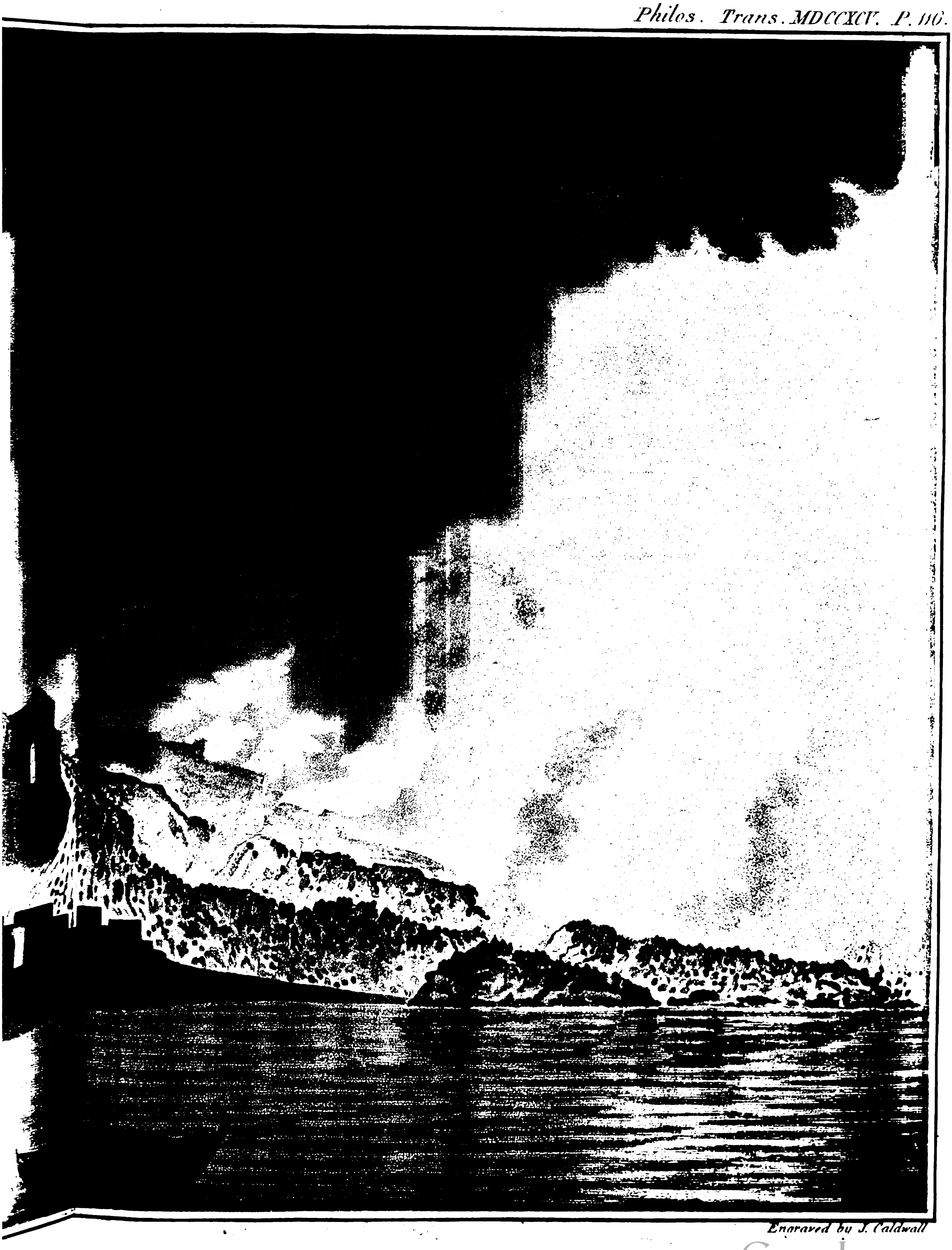




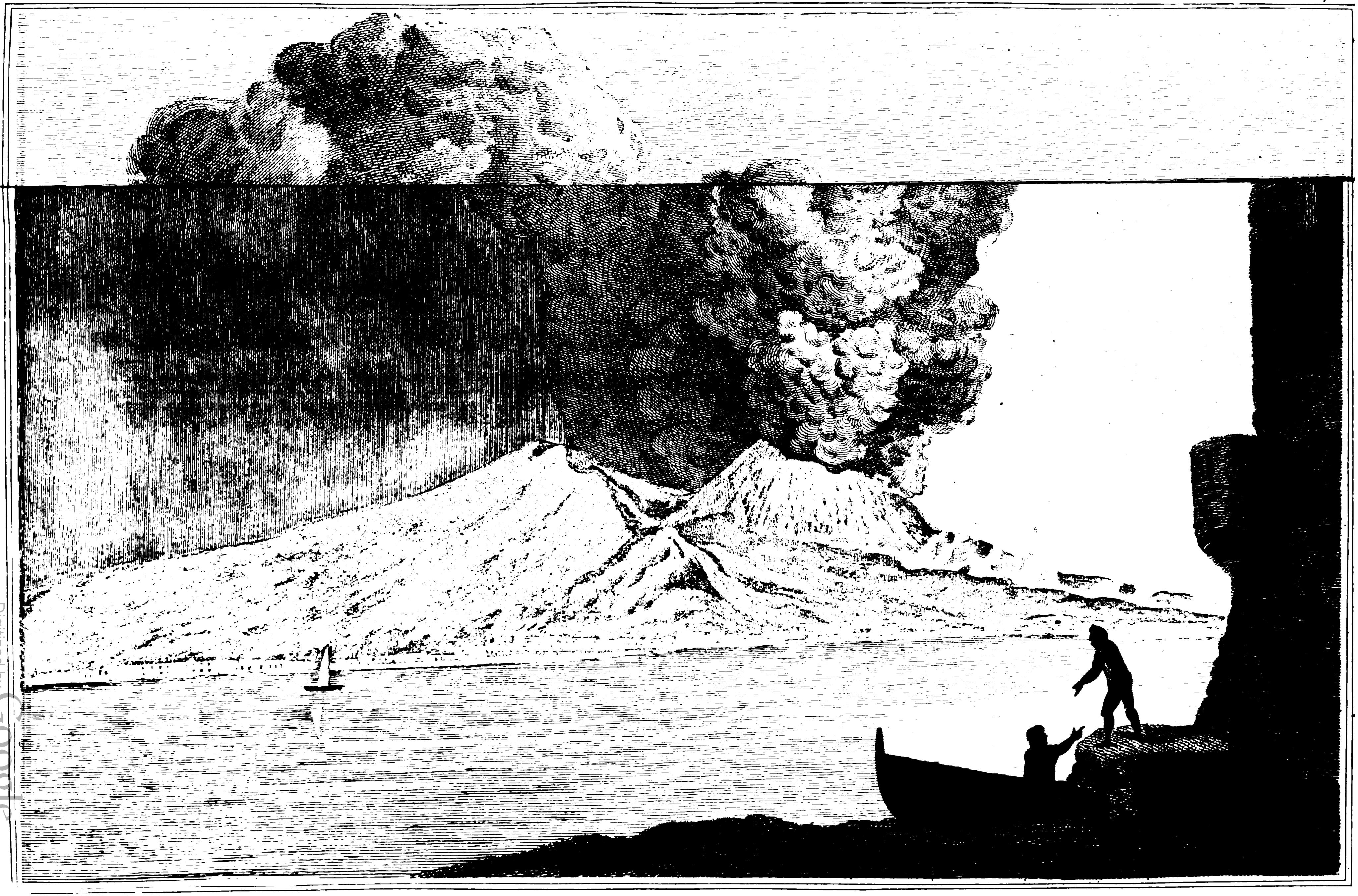
Engraved by J. Caldwall

Tab. VI

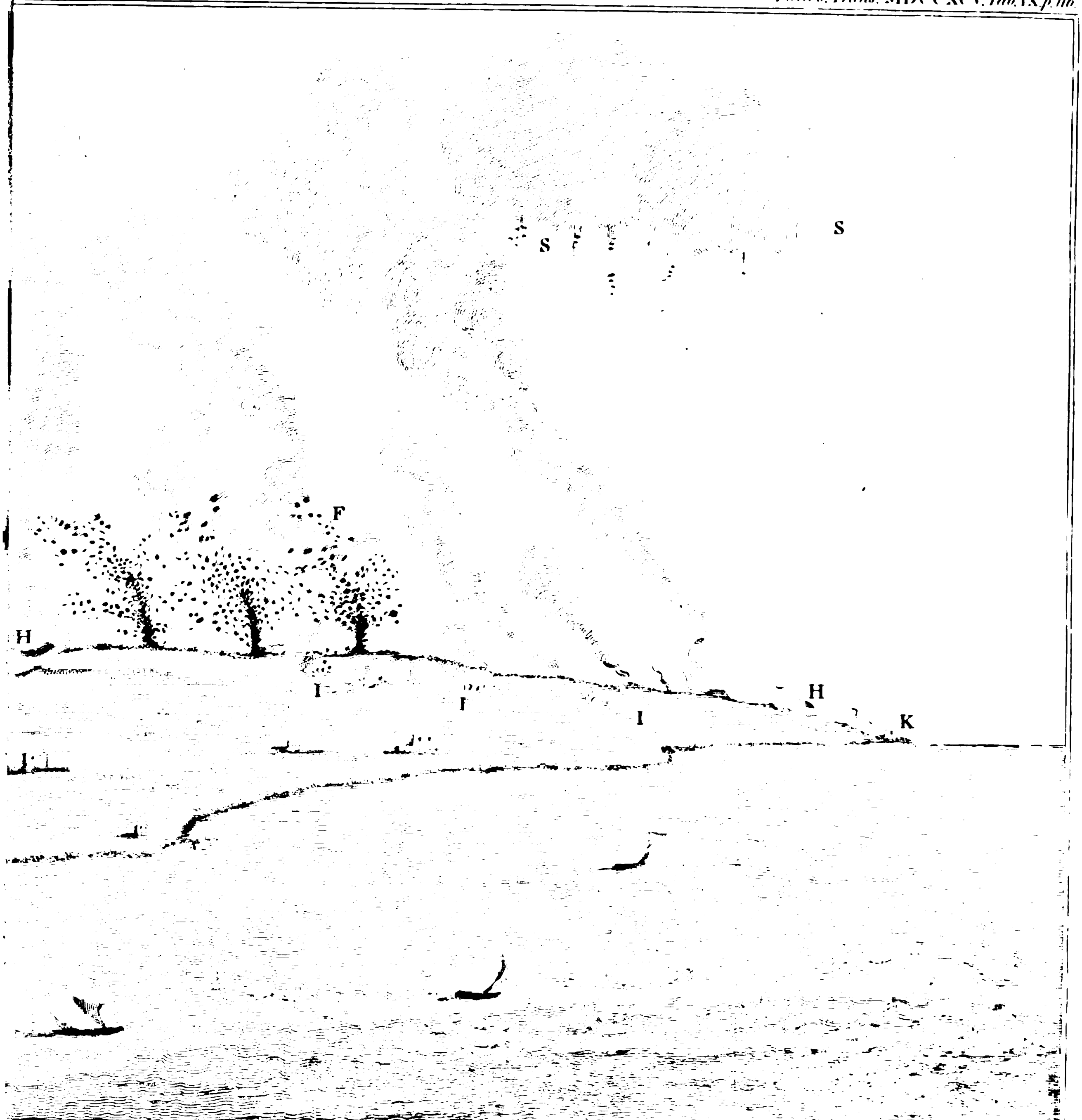


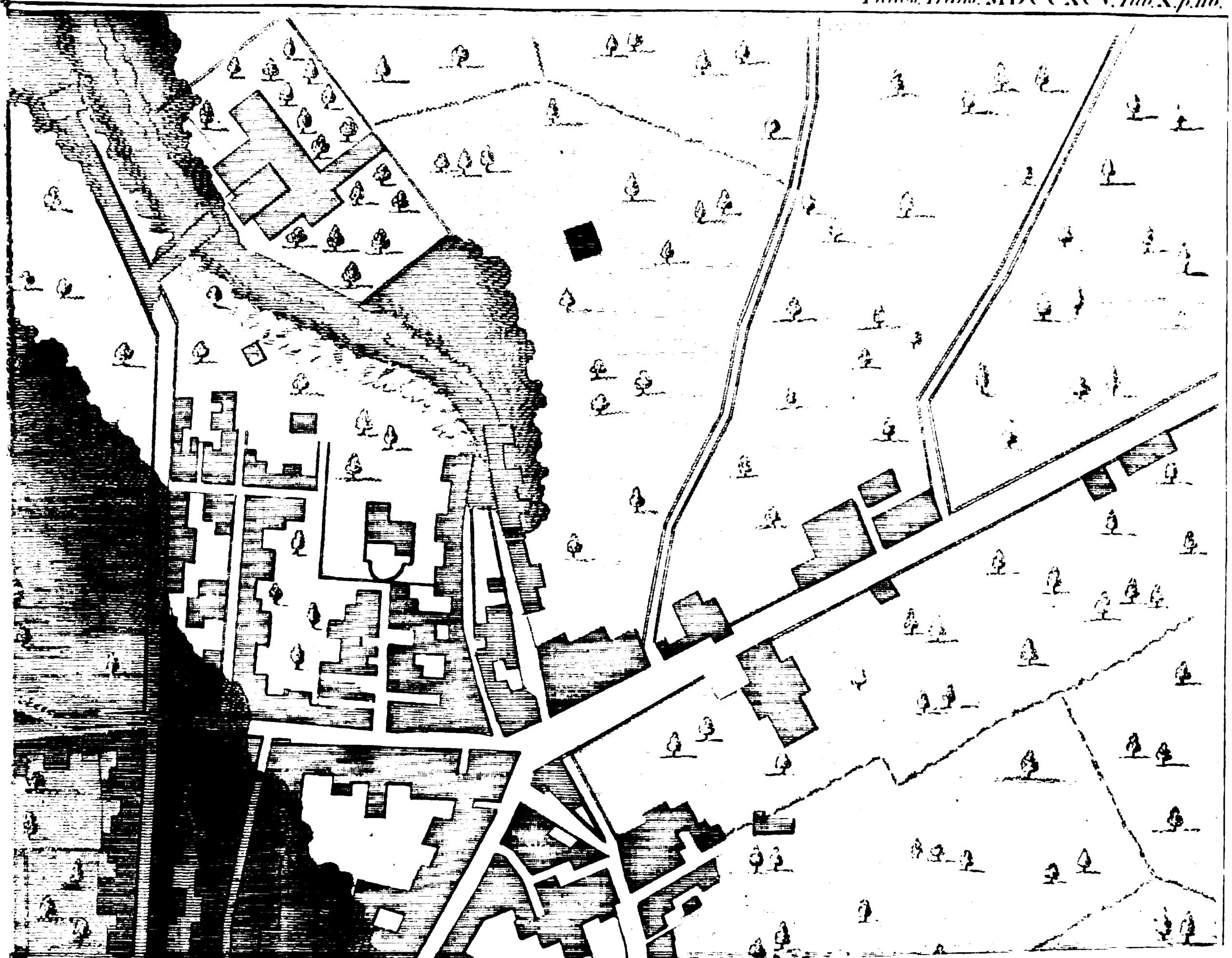


Engraved by J. Caldwall

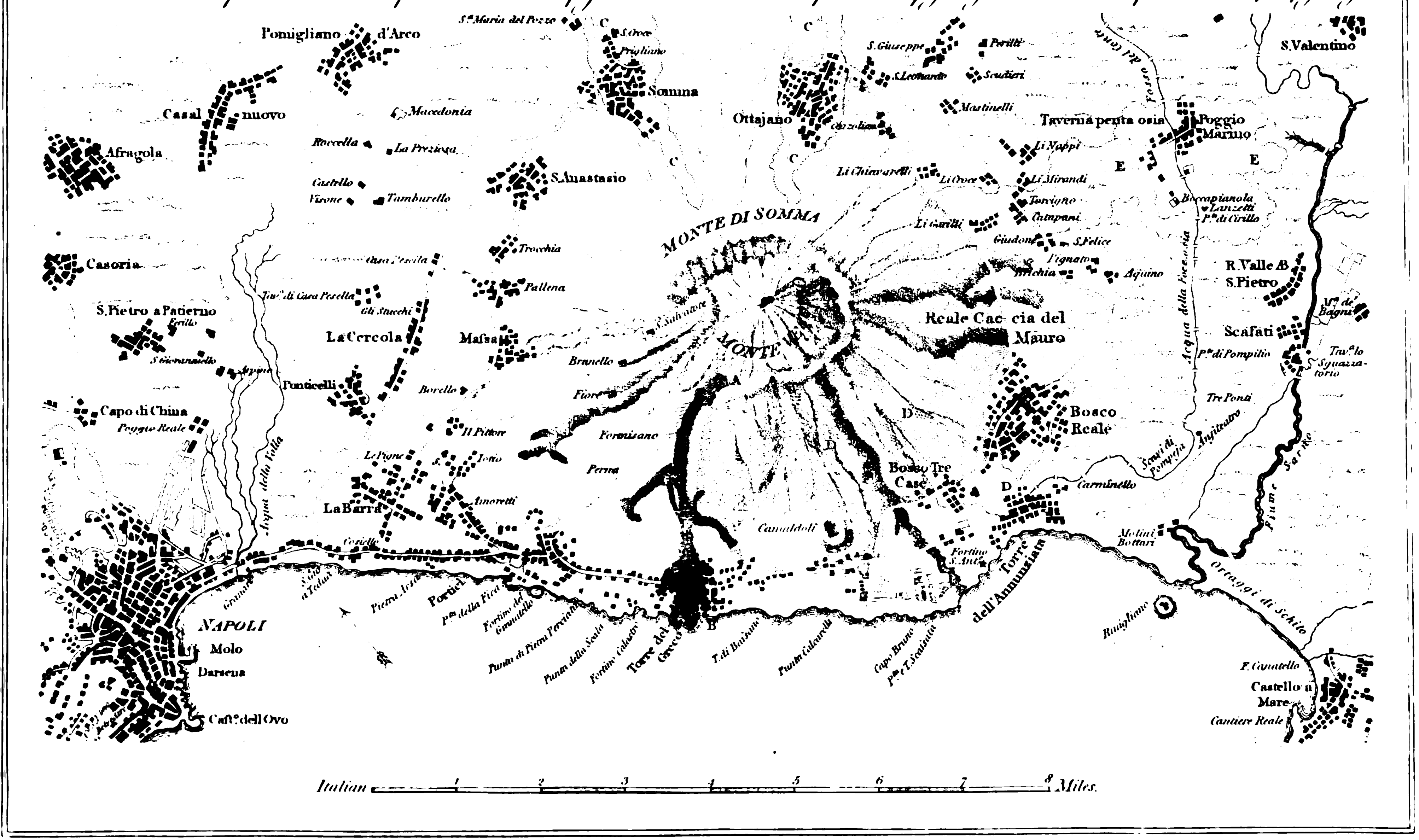



A
B


S
S
F
H
I
I
I
H
K



A.B. The late Current of Lava, with its branches, shaded darker than the course of former Currents.
C.C.C.C. Course of the Torrent of 20.th and 21.st of June. D.D.D. Torrent of the 5.th of July. E.E. Torrent of the 5.th 6.th & 7.th of July.



V. *New Observations in further Proof of the mountainous Inequalities, Rotation, Atmosphere, and Twilight, of the Planet Venus. By* John Jerome Schroeter, *Esq. Communicated by* George Best, *Esq. F. R. S.*

(Translated from the German.)

Read February 19, 1795.

PREFACE.

ALTHOUGH it is a satisfaction to me, that Dr. HERSCHEL last year found my discovery of the morning and evening twilight of Venus's atmosphere to be confirmed, as I could not hope to have obtained such an important confirmation so early, considering the excellent telescopes required, and that a favourable opportunity for such observations occurs but rarely; yet the paper *on the Planet Venus*, which this great observer has inserted in the Philosophical Transactions for 1793, contains unreserved assertions, which may be easily injurious to the truth, for the very reason that they have truth for their object, and yet rest on no sufficient foundation.

Openness, without reserve or indirect views, must guide the spirit of observation in the true inquirer into nature, and be his sole object. To this pure source alone can I ascribe what is said in the abovementioned paper, so as to reconcile it to the friendly sentiments which the author has always hitherto expressed toward me, and which I hold extremely precious;

though perhaps to others it may not have the same appearance. But this very object makes it also my duty to be equally unreserved in remarking what truth is, and demands; particularly as evident misunderstanding and error appear to have chiefly occasioned those assertions; which most probably would not have been thus made, if the author had then known of my very circumstantial memoir,* which was read at the jubilee of the university of Erfurt, in a meeting of the Electoral Academy of Sciences, and which they ordered to be printed; and could have compared the many careful observations, full of matter, contained in it. A copy of this memoir I have lately had the honour of communicating to the worthy author of the abovementioned paper.

Therefore, in order to prevent misapprehensions, let me be allowed to make some remarks, which truth requires of me, before I communicate faithfully, as I mean to do, my more recent observations, which confirm the former ones, and seem to me very important.

1. The celebrated author considers it, *with reason*, as a wonderful relation, that I should profess to have seen *appearances of spherical spots* on Saturn, without having, at the same time, determined from them the period of his rotation, which might have been done in the first hour; and he thinks that no one, who is not possessed of incomparably better sight and telescopes than he has, can have seen any thing of the kind. In that I fully agree with him, and here declare publicly, *that I have never perceived such an appearance on Saturn, however much I wished it.*

* *Beobachtungen über die sehr beträchtlichen Gebirge und Rotation der Venus*, with three copperplates. Erfurt, 1793.

In the German original of my paper, the translation of which is published in the Philosophical Transactions,* it stands thus: "On the contrary, from the circumstance that NO SUCH "EVIDENT FLATTENED SPHERICAL FORM *is perceived in this* "*planet* (namely, at its poles) *as in Jupiter and Saturn,*" &c. &c.

The author indisputably agrees with me in all the truths there asserted. He has himself observed the flattened shape of Saturn at the poles more exactly than I, and even determined the proportion of the shorter to the longer axis. But in the translation, for the words "*abgeplattete kugelgestalt des Jupiter* "*und Saturn,*" is put "*flat spherical forms,*" &c. which he understood as if I pretended to have observed spherical spots on Saturn. The author might have convinced himself of the contrary, by comparing the German original in the possession of the Royal Society.

2. He considers it as an equally wonderful relation, that I have SEEN in Venus, in the same manner as in the moon, mountains and shadows of mountains, which were four or five times higher than our Chimboraço, and that I thence pretended to have determined the rotation of this planet; on the contrary, he considers this last as hitherto undetermined, *because* HE *has never found a trace of mountains,* and all his observations, for 16 years past, have been absolutely insufficient to ascertain it, *though nothing of that kind could well have remained hid from him.*

Here it is not myself, but the truth, that I undertake to defend; and I am convinced that if my memoir above mentioned, on the Rotation of Venus, had been already known,

* Observations on the Atmospheres of Venus and the Moon; their respective Densities, perpendicular Heights, and the Twilight occasioned by them. Phil. Trans. 1792.

and the author had compared the almost innumerable and various observations contained in it, which all agree in their result, he would never have made such a declaration. *I have myself also never actually* SEEN MOUNTAINS *in Venus* AS IN THE MOON, but only *deduced* their existence and height from the observed appearances. It is even impossible to see them, according to what I have expressly asserted in my paper on the Twilight of Venus; because, on account of the thickness of her atmosphere, we can never perceive the shades of land on her surface. But if the appearances observed by me and others are true, the result deduced from them is mathematically evident.

That I have seen, *not unfrequently*, the boundary of illumination irregular, is *nothing new*, nor does it afford me any further merit than that of *confirming* with many others, *an old truth*, which DE LA HIRE, and still more ancient good astronomers, provided with the best and most powerful telescopes of their kind, had long ago discovered in perfectly similar phænomena. So early as the year 1700, DE LA HIRE observed greater inequalities in the termination of light in Venus, than in the moon;* and the Paris Academy thence concluded that planet to have higher mountains. The sole addition, as far as I know, which I have made to the older observations is, that in the crescent phase of Venus, sometimes one horn is only half as broad as the other; and that sometimes, though not often, about the period of the greatest elongation, one end of the enlightened part appears pointed, but the other rounded off: appearances which others, who had not been apprized of what they were to see, have frequently perceived as well, and

* See *Mémoires de l'Acad. des Scienc.* 1700, p. 378.

in the same manner as myself. It is here scarcely necessary to remind the reader, with respect to the ancient observations, that in all those where no extraordinary light is wanted, particularly powerful telescopes are by no means required. I should indeed be surprised that the celebrated author had not, in all the time since 1777, perceived any inequality in the boundary of light, or other appearance of that kind, tending to confirm the existence of very high mountains according to the old observations, were it not that his bold spirit of investigation has been chiefly employed in making much more extensive discoveries in the far distant regions of the heavens, where he has gathered unfading laurels. In fact, the observations which he has communicated from his journal are *much too few* to prove a negative against old and recent astronomers. Without encroaching upon truth in the least, I could certainly produce more good distinct observations during many months, from 1779, when I began to examine Venus carefully, to 1793, when my memoir on her rotation was finished, than are adduced for a period of 16 years in the abovementioned paper of my opponent: having, in the latter years, observed this planet not only daily, but, as far as the weather and her position admitted, almost hourly through the whole day and evening. This, I think, is shewn evidently enough by the memoir already mentioned, in which only the later observations appertaining to the subject are inserted: and without such steady perseverance, my trouble for so many years would have been fruitless, as was the case with other observers; *for, in almost innumerable observations, the same thing happened to me as to the author of the paper in question, namely, I perceived neither spots, nor any other remarkable appearance, except the unusually quick* *decrease of light toward the boundary of illumination, which itself was not sharply defined.*

It is right that every acute observer should be on his guard against a precipitation which often occurs, and not contradict respectable astronomers who have preceded him, if he should not at once, in a few observations, find those appearances in an object which such credible men have perceived, or deduced from their observations. The mischief thence arising may be important, and lead to more general error in proportion to the celebrity of the contradicting observer, because there are always persons enow who will adopt it as a truth without further examination. And yet there are many examples of this in the most modern history of astronomy. Thus, for instance, the old worthy selenographer HEVELIUS found some of the mountains of the moon to be more than $\frac{2}{3}$ of a (German) geographical mile in perpendicular height; and this truth stood more than 100 years in all the elementary books. Later astronomers measured only a few of those mountains, and partly not with all the requisite circumspection; yet concluded, from too few and insufficient observations, that HEVELIUS had given them much too high.* This was already received as true in the elementary books; notwithstanding which the excellent HEVELIUS was absolutely in the right, as is proved by my numerous and incontrovertible measurements.†

When, in the years 1789 and 1790, the ring of Saturn appeared as a straight line of light, I perceived only a few pro-

* See RÖSLERS *Handbuch der practischen Astronomie,* 1 Th. p. 441.—Philos. Trans. Vol. LXX.

† *Selenographische Fragmente,* § 34 to 82.

jecting luminous points on it till after October, 1789; but in February, 1790, incomparably more of them, in the frequent observations I made. These, in part at least, I considered not as satellites, but as true and large inequalities of the surface of the ring; and thence drew, on the strongest grounds of probability, the same conclusions as MESSIER and other respectable observers had done 15 and 30 years before; one of those deductions, and which seemed highly probable, was, *that the southern surface of the ring must have many more and larger inequalities than the northern.* These remarks had already been made known to the world, in the publications of the Naturalist Friends at Berlin;* when I unexpectedly read in the Philosophical Transactions a conclusion which discouraged me very much, that the astronomers who considered these projecting luminous points as inequalities of the surface, were mistaken, those appearances being occasioned by the satellites of Saturn: this conclusion was drawn from some new and excellent observations inserted in the paper itself, but which were continued only to November, 1789. However, so much the greater was my pleasure to find this assertion recalled in the next volume of the Transactions, where Dr. HERSCHEL, from those very projections, has made the important discovery of the rotation of the ring, and determined its period. Now, if there are really in the ring of Saturn such enormous inequalities, I do not see why my conclusion, deduced from so many agreeing observations, namely, *that the mountains of Venus bear nearly the same proportion in height to her diameter, as those of the moon do to the diameter of the moon*, should be thought a wonderful relation, especially since all my

* *Schriften der Naturforschenden Freunde.*

observations hitherto, as for instance those on the visible luminous spots in the dark part of the moon, on the apparent changes of the moon's surface, &c. have been confirmed by others.

From these remarks, the answer will readily present itself,

3. How the author of that paper could look upon my observations on the rotation of Venus as unfounded, though there are so many of them which agree together, and *he had not read and compared them*; and could think the period of rotation as much undetermined as before. Whoever deigns to bestow some attention on my memoir on the rotation of Venus, will soon find,

(*a*) That certainly I did not go to work carelessly, *but first* arrived *gradually* at an *approximate* estimation *by almost innumerable observations made in very different ways.*

Although I perceived, as early as in the year 1786, some luminous spots of Venus, which seemed to me to shew a period of rotation of about 24 hours, as DOM. CASSINI had also thought; yet I suffered them to lie unpublished six years, because I was doubtful whether some delusion might not have intermixed itself; until at length a favourable opportunity accidentally led me to pursue the investigation of this subject *in an entirely different manner.*

(*b*) It will also be found that the author, among his observations, which taken altogether are *but few, cannot shew a single one in which he observed at the same time with me.* But every person conversant in these subjects will agree with me, that in order to prove the inaccuracy of my observations, or at least render them doubtful, it is essentially necessary, *that an impartial observer should have* DIRECTED HIS ATTENTION WITH

EQUAL CARE TO THE SAME CIRCUMSTANCES AT THE SAME TIME, *and not have seen them the same as I have given them.* In my memoir, to which I here refer, those observations only which belong to the point in view are compared together; *but in other observations, almost innumerable, which I made partly before I had paid any particular regard to the inequality of the horns, and partly in the intervals, I did not perceive, any more than the author, either spots or any thing appertaining to the matter in question; and consequently our corresponding observations perfectly agree together.* It is, however, and will remain a truth, that there is no such thing as a monopoly of discoveries; one man may luckily observe something to which the other did not direct his attention *in the same manner*, although he viewed it at the very same moment. Thus, for instance, since HEVELIUS's time many observers, provided with sufficiently powerful telescopes, have examined the moon, without perceiving the immense southern *cordilleras* of her edge, the perpendicular height of which, by indisputable observations, amounts to something more than a geographical mile, and which I have pointed out and delineated in my Selenotopographical Fragments, under the names of Leibnitz and Doerfel. And yet these high mountains are really there, and afforded a magnificent spectacle at the commencement of the solar eclipse on the 5th of September last year, though they were not then exhibited in their greatest projection. So likewise it is true, that several of the many important discoveries, on which the author has founded his eternal fame, might have been made as well by other observers, who were furnished with good achromatic telescopes, if they had directed their attention in the same manner to the same objects, with equal acuteness and perseverance.

Having premised these remarks, I can now communicate exactly, and according to their connection, my new Observations on the Planet Venus; and that they may, in various points, be more easily and better compared with the observations of my opponent, I will at present follow the order of my journal.

New Observations, confirming the Rotation of Venus, her mountainous Inequalities, and the Twilight of her Atmosphere.

Feb. 18, 1793, 5^h 50′ *p. m.* As cloudy weather had continued uncommonly long, and as the experience of many years had already shewn that little or nothing remarkable is to be expected, when considerably more than half of Venus is illuminated, I could not till this time proceed on the observations, the planet now approaching her greatest eastern elongation. With 160 of the 7-feet SCHRADERIAN telescope, I had, with the full aperture, such an extraordinary soft and clear image as I scarcely ever found in this planet. According to fig. 1. (Tab. XII.) both ends of the boundary of light appeared equally rounded, without any perceptible difference. There was, however, again, in the middle of the enlightened part, a kind of darker nebulosity, not quite clearly to be distinguished, which seemed to consist of two very slight nebulous spots. The light decreased to extraordinary dimness toward the boundary of illumination.

Feb. 26, 5^h 15′ *p. m.* An extremely remarkable observation. With 160, 288, and 370 magnifying power of the 7-feet SCHR.

I found, the image being uncommonly fine and soft, that as usual there was no spot, but that the northern end of the boundary of light, *a*, fig. 2. was most certainly rounded off beyond all comparison more than the southern; the latter appearing to run on rather pointed, with an inequality upon it, on which a dim greyish shadow was perceived.

At 6^h 20′. In order to secure myself against deception, I desired my attendant, who came in at that time, and has remarkably good sight, with some practice, to observe whether he saw any thing particular; and what? The answer he gave, at the first sight, was, that Venus had *an evidently irregular form;* that *on the right* (southern) *end of the illumination she was pointed, the point having some shade on it, but that on the left she was oval.*

At 6^h 40′, the difference began to be less striking; and having intermitted the observation in order to recruit my eye, I found at 7^h 30′ both horns equally rounded, though with this difference, that at the southern one a small indistinct glimmering point of light, barely perceptible, often shewed itself at *a*, fig. 3. not on the rounded part, but close to it: this was seen with 288, as well as 160. At 7^h 45′ I found it still the same; and likewise afterwards with the 13-feet reflector, which also shewed me the point. Soon after, Venus became invisible.

There was no nebulosity to be perceived as on the 18th.

Feb. 27. I wished much to examine the changes which might happen in the course of all this afternoon, but high light clouds prevented me. It was very remarkable, *that at* 5^h 40′ *on this succeeding day, I saw most distinctly the same appearance as the evening before*, with 109 and 160 magnifying powers, only with this slight difference, *that the shadow, which shewed itself* *again at the southern point, as at a, fig.* 4, *entered westward a little further into the point;* and it sometimes appeared as if the shadow would penetrate all the way through, and entirely cut off the point; moreover the northern horn was not quite so much rounded as the evening before. With both magnifying powers I saw likewise again, on the illuminated part, a very faint oblong nebulosity *b*, distant only about $\frac{1}{3}$ of the semidiameter from the external edge. For greater certainty I applied a power of 288 and 370, with which I distinguished the abovementioned form of the illuminated part, extraordinarily fine and distinct; I could likewise see, with all the magnifying powers, the darker indentation of shadow *a*, but not the very slight nebulosity *b*. The indentation of shadow was in length at least $\frac{1}{10}$ of the semidiameter; and at $6^h\ 11'$ it began to pass quite through, so that the southern horn appeared rounded like the northern, and the fine point, being now separated, looked like a glimmering dot of light close to it. I saw this separate point of light repeatedly, with 209 times, among other magnifying powers, very plain and evident, the image being soft; in different observations I found it always the same, whatever was the power; and at $6^h\ 19'$ the southern end appeared fully as round as the northern. I thought it remarkable, that at $6^h\ 25'$, a power of 288 shewed it smaller than it appeared with a less power. At $7^h\ 12'$ the point of light had vanished, as I perceived with both the 7 and 13-feet SCHRADERIAN reflectors. Mr. TISCHBEIN, the instrument-maker, who came in toward the end of the observation, saw it in the same manner. Both horns at this time appeared quite equally rounded; but a new remarkable circumstance was now first discovered by Mr. TISCHBEIN. He observed with both reflectors, that at the northern horn, though rounded like the southern, *a brighter pointed small inequality* projected out from the faint boundary of light, as is expressed at *a*, fig. 5. It was difficult to distinguish, but his eye, more accustomed to microscopic objects, saw it alike with both reflectors, and *in the same place*; I perceived it also, though it was not striking. The observation was continued by both of us to 8^h 30′, when Venus being sunk too low, began to be indistinct. At this time indeed I could no longer distinguish that fine point; but in every part of the field of the instrument something brighter appeared in its *fixed* place.

Whoever is pleased to compare these two observations impartially, I doubt will not consider them as illusions. To me they rather appear, in more than one respect, convincing and important. In the first evening, the southern horn, as two observers agreed, changed its form very quickly, that is *in* 15 *minutes*, so much that the difference between it and the northern was not nearly so striking as before. In the second evening, the air being clearer, and the image excellent, this change was still quicker; for *in* 11 *minutes, during the observation itself, the end passed very* EVIDENTLY *to the form of a separate point of light.* Supposing both changes to be the same, and produced by the rotation, the alteration to a separate point of light must have happened on the first evening, *at most* 11 minutes later than 6^h 40′, when I intermitted my observation; that is, about 6^h 51′; because on the second evening it took place in 11 minutes. But on the second evening, when I noticed this striking alteration, I no longer knew the time marked the evening before, and I now noted down 6^h 11′. *Consequently this change took place the second time* VERY NEARLY *in* 24 *hours less* 40 *minutes; and* *from these two careful observations alone we may conclude, very probably, the rotation to be nearly* 23 *hours* 20 *minutes*; which agrees extremely well with the approximate period of 23 *hours* 21 *minutes*, which I have deduced from observations of two years, in my circumstantial memoir already quoted.

Feb. 28, *from* $10^h\ 50'$ *to* $11^h\ 30'$, *a. m.* With powers 95, 160, and 209 of the 7-feet SCHR. I found no spot, and both horns perfectly alike; the light decreasing toward the boundary of illumination extremely plain, and the terminating arch of both horns, but *particularly of the southern*, rather unequal and knotty.

At $3^h\ 10'$ to $26'$, with 160, 209, 370, and 632, a fine image; the decreasing light seemed at the boundary of illumination to mix itself with the colour of the heavens, becoming equally faint. *Both horns alike oval.*

At $4^h\ 36'$, the same.

$5^h\ 4'$, no difference.

$6'$, still the same.

$7'$, the southern horn began to acquire a pointed shape.

$9'$, it appeared already pointed; the northern blunt as before.

$11'$, the southern exhibited the same appearance as both evenings before; and I likewise perceived something darker making an impression into it.

$17'$, Venus behind clouds.

$19'$, through light clouds her southern horn was perceived to be pointed in comparison with the northern.

$37'$, the same in some clear intervals. The northern horn appeared always blunt.

That the decrease of light toward the boundary of illumination, whereby that part of the disc becomes extremely dim, is no deception, appeared now evidently; for whilst the planet faintly glimmered through the clouds, I could often see only $\frac{2}{3}$ of her illumined part, reckoning from the outer edge, and sometimes only half.

5^h 55'. Venus shining out for a short time between the clouds, the same appearance with full certainty; and I remarked also again a slight darker indentation at the southern horn; *but the scene was by no means so striking as both evenings before.*

5^h 59', the appearance changed; and

6^h 7', this was found to be confirmed; but I could not with certainty discover a separate point of light; sometimes, however, though but seldom, there seemed a glimpse of it at the southern horn. Immediately afterwards Venus was covered with clouds.

6^h 30' to 6^h 45'. Venus shining in a clear sky, her southern horn was again, as at 5^h 6', rounded exactly like the northern; and with powers 160, 209, and 370, and a distinct image, I found no trace of a separate point of light. Comparing this third observation with the two former ones, it agrees very well to the minute; for now the southern horn had nearly the same appearance of being like the northern, at 6^h 30', as it had the preceding evening at 7^h 12', and therefore 42 minutes earlier; but in general it was evident that the appearance remained no longer exactly the same as on the two evenings before; and this difference may be easily explained by the very probable supposition of a libration, and that it is not a single mountain which occasions the appearance, but a considerable ridge, with many high points: moreover the clearing up or thickening of the atmosphere of Venus, which according to my former observations is pretty dense, and the effects of refraction, may have a considerable influence on such phænomena. Whoever has frequently observed in the moon the very striking variety in the projections of the high ranges of mountains at her edge, namely, Leibnitz, Doerfel, or d'Alembert, will more readily comprehend such effects of a libration.

The 1st, 2d, and 3d of March, bad stormy weather.

The 4th, 6^h *to* 6^h *30', p. m.* with 160 of the 7-feet SCHR. the image being extremely fine, I found *both horns equally rounded, without any difference.*

At 7^h, the same. But at this time there appeared, in the enlightened part, a slight nebulous shade, which, as is expressed in fig. 6, extended to the boundary of light. At 6^h, in the bright twilight, I had not remarked it; and I suspected it to be a sort of dazzling, though the image appeared uncommonly soft and distinct. The bad weather which came on soon after did not allow me to apply other magnifying powers and telescopes.

March 5, at 4^h 25' to 35' *p. m.* with the same power, I found the northern horn still rounded, and the southern somewhat *pointed, but not strikingly so.*

At 4^h 40', with a power of 200, the same; and moreover a weak shadow was again perceived on the planet. *So likewise with* 288 *very distinct, and then with* 370 *extremely certain*; *but on the whole it was not striking*; for the southern horn also appeared somewhat roundish, and probably another person less accustomed to such observations, would not have remarked it.

At $5^h\ 45'$, the atmosphere being less clear, it was doubtful; and at

$6^h\ 35'$, it was quite certain that both horns appeared equally rounded, without any difference. I found neither spot nor glimmering.

From the 6th to the 10th of March, the learned and worthy Dr. CHLADNI, inventor of the euphon, observed with me; and having ascertained, by careful comparison, the extreme goodness of my reflectors, can bear witness of it.

March 6th, cloudy.

March 7th, noon and afternoon cloudy.

At 6^h *in the evening* I found, with the 7-feet SCHR. and magnifying powers from 160 almost to 400, both horns constantly the same, without any difference. So they appeared to me also with the 13-feet reflector; and with both instruments to Dr. CHLADNI.

The 8th March, at noon, the image of Venus but seldom appeared fully distinct. In the intervening moments of greater distinctness, Dr. CHLADNI remarked, *that though* both horns were roundish, yet the northern *was rather more pointed than the southern*. Afterwards I found the same thing. In the afternoon cloudy.

From 6^h *to* 7^h *in the evening*, with 95 to 288 magnifying power, I found *both horns equally round*, and no spot or any thing remarkable, though Venus did not appear perfectly distinct.

March 9th, $6^h\ 15'$, *p. m.* Venus being near her greatest eastern elongation, both horns appeared pretty pointed, with a power of 250, and a fine soft image; they were also *both alike*, but with the slight difference, that close to the southern horn *a very minute particle projected, which seemed to be rather separated from the rest of the enlightened part.*

At $8^h\ 2'$, *the air being clear, a projecting inequality shewed itself with certainty at the southern horn, as is represented in fig.* 7, (Tab. XIII.) *at b.* It was found the same with 288 of the 13-feet.

As our own atmosphere was then very clear, that of Venus also seemed to be purer than usual; for with both reflectors, and particularly with the 13-feet, *Dr.* CHLADNI, *as well as myself*, enjoyed a magnificent view of the arch of illumination, which seldom presents itself so well to the eye, *the image being uncommonly clear and distinct. To both of us the boundary of illumination, toward which the light became very dim, appeared* (be it ever so much contradicted) *not only nebulous, and not sharply terminated, though sensibly sharper than usual, but also very evidently unequal and rugged, with faint shades between,* as I have often seen it, but never so plainly. In truth, the appearance, as each declared, was *very like* the image of the moon at the time of her quadratures, only that the boundary of light was sensibly less sharp, and the faint shadows between were not almost black, but in some measure like the dark spots of the moon's surface, grey, yet darker than the other parts. This instructive observation remains still before my eyes. So delicate a picture of nature cannot well be drawn, however we both made cursory delineations of it, from which fig. 7. is copied: but at the boundary of light, soft grey shadows must be imagined, traced into the interstices at *a, b, c, d, e, f, g.*

March 11*th, from* $6^h\ 10'$ *to* $45'$, *p.m.* the weather having cleared up after snow, I found *no striking difference of the horns*, with powers of 209, 288, and 370, and a distinct image; however, the southern appeared rather less pointed, which was occasioned *by a* VERY FINE *glimmering pointed line of light, that ran on from the horn not far into the dark side, as at a, fig.* 8. *and was visible with all magnifying powers. I saw this line of light equally, whether I observed with the whole aperture, or covered a considerable part of it.*

It would be singular indeed, and most discouraging for all such observations, if so many appearances, agreeing together, and viewed with every precaution, should be merely deception, particularly as they usually and principally occurred only at the southern horn, without any reason that could be assigned if it be thought a fallacy. But if there be no deception, it follows incontrovertibly, that the surface of the southern hemisphere of Venus, like that of the moon, has the most and greatest inequalities.

March 12th, 6^h 15′ to 30′ *p.m.* no kind of difference in the horns, no spot, or any other unusual appearance, could be seen with a power of 209.

At 8^h, the same.

But on the *13th of March, from* 11^h *to* 11^h *20′ a.m.* I perceived, with the *same* magnifying power, *a very evident and remarkable difference. The northern horn appeared pointed, but the southern was rounded, with a very small knot close upon it to the south,* as at *a*, fig. 9. Thus I saw it with 160 and 288 magnifying powers; and I even distinguished it with 95, though this was too small a power for so minute an object. On the northern horn I found nothing similar, notwithstanding I compared them repeatedly. Business called me away; and the atmosphere soon afterwards became cloudy, and continued so all day.

This very remarkable observation is indeed not precisely the same as those of the 26th and 27th of February: yet the appearance is very little different from that of the abovementioned days, when the shadow, fig. 4. at length penetrated quite through, and the separated part was perceived as an insulated bright point. Now if it be considered, that on the 28th of February, only 24 hours later, this appearance recurred, but was not exactly the same; and that when a very extensive mountainous southern region forms the edge of the planet in various degrees of obliquity, according to the respective situations of Venus and the earth, the phænomena must naturally be so diversified; there cannot be the least doubt, but that the same southern range of mountains, which occasioned the similar appearances of the 26th, 27th, and 28th of February in the evening, also produced this of the forenoon about 11 o'clock, according to the rotation; especially as no intervening observation contradicts this conclusion. The effect of small differences in the position of planets, may be exemplified from the late eclipse of the sun on the 5th Sept. 1793, when the projections of the mountains Leibnitz and Doerfel, bounding the southern edge, were so different from those of the older observations, under a similar variety of circumstances. The abovementioned conclusion with respect to Venus becomes still more evident and remarkable, from its *agreeing more exactly than could be expected, according to the circumstances, with the period of 23 hours 21 minutes, which, in my memoir on the rotation of Venus, I had determined as near the truth:* for on the 27th of February that appearance took place about 40 minutes earlier than the evening before; and the middle of the time when the southernmost part of the southern horn appeared as a separated point of light (a phænomenon similar to the present), was by that observation at 6^h 29′. *From the 27th February, 1793, 6^h 29′ p.m. to the 13th March* 11^h *a.m. there are 13 days 16 hours 31 minutes, which, with the period of 23 hours and 21 minutes, are resolved into 14,04 revolutions, exact to the very inconsiderable fraction of* $\frac{4}{100}$; which is so much the more surprising, as no attention could be paid to the inequalities.

The same day at 6 p.m. I saw Venus with a power of 160, very sharp and distinct through thin clouds; and found both horns again equally pointed, and the much fainter light at the boundary of illumination very evident. And the weather *on the 14th of March*, having been bad all day, I saw, together with my attendant, the same thing on the

15th of March, at 6^h 30′. Both horns were then alike, and there was no spot.

March 16th, 2^h *15′ to 45′, both horns equally pointed*; no spot. To search with the greater certainty whether I could not discover some inequality, I took the 13-feet reflector, and still found it as before, the image being uncommonly sharp. Thus one observation gives weight to the other against fallacy.

From the 17th to the 21st of March, variable and cloudy weather.

March 21, at 7 in the evening, with powers 160, 288, and even 95, of the 7-feet, both horns were pointed, without any perceptible difference: no spot.

March 22d, 2^h *35′ p.m.* the same.

At 7^h in the evening, however, I found *a sensible alteration*, with 160, 209, 288, and 370 magnifying powers. *The northern horn constantly appeared, according to fig. 10, not pointed as before, but somewhat less obtusely rounded*, whilst *the southern* was pointed and *projecting a little* beyond the line *of the cusps*.

Between the projecting point and the enlightened side, there was often to be perceived, and equally with all magnifying powers, a light greyish shade, which seemed to divide the point. Soon after the weather became cloudy.

March 23*d*, 6^h 37′, the atmosphere having cleared up much, but the air being still not very favourable, I found, with the same magnifying powers, *an exactly similar appearance*; *but an hour afterwards, the northern horn ran out in the same manner into a point, and projected as far as the southern, so that the phænomena were no longer the same.* Soon afterwards it became cloudy.

March 26*th*, 6^h 10′ *p. m.* the weather having cleared up again, I saw *both horns equally pointed*, with the same magnifying powers.

7^h 30′, *the same.*

8^h 15′, *also the same.* This too agrees with the period of rotation, according to which the phænomena, observed on the 22d and 23d at 7^h and 6^h 37′, could not be visible again at the times here noted down.

March 27*th*, 11^h *to* 11^h 40′ *a. m. both horns equally pointed*, and, as usual, no spots. With a reference to my former remarks, I had proposed to observe Venus every hour throughout the day; but it grew cloudy.

At 6^h 30′ *p. m.* the sky having cleared in the part where Venus was, I found in like manner *both horns* equally pointed.

At 7^h 30′ *p. m.* the same.

March 28*th*, 10^h *forenoon*, with 160, 209, and 288, *both horns were pointed*, without any striking difference.

11^h 15′, with the same, both horns *equally pointed.*

5^h 30′, *the same*; even with a magnifying power of 370 times.

6^h 10′, *just the same.*

March 30th, 6^h 45′ *p.m.* with 160, both horns uncommonly sharp, and *equally pointed.*

7^h 30′, the same. No spot. Then followed rain and cloudy weather.

April 2d, 6^h 50′ *p.m.* with power 160 of the 7-feet SCHR. it struck me *with uncommon certainty and precision, after so many similar appearances of both horns, that the southern horn b, fig.* 11, *was remarkably slenderer in comparison with the northern, a; and that in general the whole southern illuminated part, c, b, d, appeared considerably smaller than the northern, c, a, d.* I tried this phase with 288 and 370, and found it to be *assuredly so*; *and with the same certainty I observed it also repeatedly confirmed with the noble* 13-*feet reflector,* till 8 o'clock. *My attendant, who knew nothing of it, made the same remark, and particularly noticed the irregular form of the arch bounding the illumination,* which, by entering in further from *d* to *e*, than from *d* to *f*, formed a slenderer horn, as often happens with the moon; and also in the same manner in its single parts, the crescent of Venus *appeared uneven, like that of the moon,* although not sharply so, but faintly and undefined. I did not now see the mountains of Venus, by their projection and shadow, as in the moon; but the appearances above described must indisputably have been occasioned by mountainous inequalities. Very often have I perceived similar phases on the moon with my naked eye.

It would be inexplicable, if different eyes, with different excellent telescopes, and various magnifying powers, should have seen for an hour together such an appearance, with equal confidence, and yet the whole be nothing but a fallacy, misleading a careless observer. Did not CASSINI, BIANCHINI, and other observers, surely not deficient in caution, perceive similar phænomena, and draw the same conclusion?

At $8^h\ 35'$, Venus presented not a clear image. She had already passed the pleiades about half a degree, and my hope of seeing perhaps an occultation was frustrated.

$10^h\ 15'$. A very instructive observation, by comparison with the preceding. Notwithstanding Venus was got near the horizon, and had some tremulous motion from the fine vapours, the sky being otherwise clear, yet her image was free from false light, and sufficiently distinct, with power 160 of the 7-feet SCHR. a reflector which almost never fails me. *I was quite surprised to perceive most evidently, at the first sight, that the abovementioned remarkable phase had changed as remarkably within 2 hours 15 minutes; and that, even whilst the instrument was screwing to its focus, in all parts of the field, the northern horn a, fig. 12, constantly appeared pointed; whereas the more slender point of the southern horn, b, had vanished, and this horn had become rounded, as it was on the 26th, 27th, and 28th of February, and the 13th of March.*

Comparing this observation with those I have here named, it becomes very remarkable and decisive, by confirming my former approximated estimate of the period of rotation. On the days just mentioned I had, at the hours noted down, observed a somewhat similar change in the southern horn, *conformably to such a period of rotation;* but had never seen it again in all the numerous observations I made since the 13th of March, at hours when, according to the rotation, it should not appear. But now it was seen again at $10^h\ 15'$ in the evening. *From 11^h in the forenoon of the 13th of March, to the*

2d of April at 10^h $15'$ *in the evening, there are* 20 *days* 11 *hours and* 15 *minutes, which, with a period of rotation of* 23^h $21'$, *divide into* 21,005 *revolutions, exact to the inconsiderable fraction of* $\frac{5}{1000}$.

April 3*d*, 5^h $40'$ *p.m.* with 160 and 370 magnifying powers, I found Venus again *irregular* in single parts of the *arch terminating the illumination.* That is, according to fig. 13, (Tab. XIV.) it sunk in somewhat, but very little, at *a*, and between *a* and *b* it protruded out a very little. Both horns, however, were pointed, and no spot could be seen.

At 6^h $48'$, the boundary of light went in a little at *d* also, according to fig. 14.

At 7^h $25'$, I found both horns alike pointed, and no striking difference whatever, as the evening before. No spot.

At 8^h $10'$, the same. No perceptible difference in the horns.

At 9^h $50'$, I found *the southern horn visibly, though not much, rounded as yesterday. Mr.* TISCHBEIN *saw it so likewise:* but Venus was already too low, and undulated in the vapours, so that we could not reckon on this observation with confidence; yet it agreed with the former.

April 4, at 5^h $50'$ *p.m.* with a magnifying power of 160 Venus appeared extraordinarily plain and fine, but without spots. The light lost itself in a dim grey at the boundary of illumination, which appeared somewhat uneven, as it did yesterday about the same time, but both horns looked equally sharp.

Without thinking of it in the least, I saw, with a power of 288, *that the southern horn was somewhat slenderer than the day before yesterday;* and this was confirmed with a power of 370, which shewed me clearly that the smaller form of the right side, according to fig. 15, was occasioned by the boundary of light running in a little more at the right horn.

6^h 25′, I found this repeatedly confirmed with 370.

At 7^h *5′ to* 10′, this difference no longer struck my eye; both horns appeared equally pointed.

8^h 24′, the same with 160. Venus was no longer distinct.

April 5th, 5^h *15′ p.m.* Both horns indeed sharp, but *all* as it was the evening before, and nothing striking, with 160.

5^h 25′, the same with 288.

6^h 38′, still the same.

7^h 38′ *to* 8^h 10′, with both magnifying powers, and afterwards with 136 of the 13-feet, no manner of inequality in the horns. With the greater telescope the decrease of the light to dimness, and the *dim unevenness of the boundary of light,* appeared extraordinarily fine.

8^h 42′. Both horns still equally pointed, with power 160 of the 7-feet. No spot.

9^h 55′, still the same. The planet being now as low as on the 2d and 3d of April about the same time, I tried, by screwing in various ways, whether I could get the southern horn to look somewhat rounded, as it did then, but in vain: both horns were equally pointed.

April 6th, 6^h 45′ *p.m.* with 160 of the 7-feet, I found no striking difference, *both horns being equally pointed.* No spot.

7^h 29′, likewise so.

8^h 10′, the same with power 288, and an extremely sharp image.

8^h 45′, the same.

$10^h\ 5'$. In this situation of Venus near the horizon, I tried again, by screwing the small speculum, and moving the image in the field, whether I could give her a false form, similar to that of the roundness of the southern horn on the 2d and 3d of April; but both horns were, and remained, pointed. Consequently the observations of the 2d and 3d April were no deception, and they agree extremely well with the period of rotation, being the 5th and 6th new repeated proofs of it.

April 7th, $6^h\ 30'$. With power 160, both horns were equally pointed: no spot, nor any sensible inequality, except the dim faintness of the boundary of light.

$6^h\ 55'$, with 288, the same.

$7^h\ 15'$, the same.

$7^h\ 55'$, still the same.

From the 7th to 12th of April cloudy weather.

April 12th, $6^h\ 30'$ *p. m.* With the same magnifying power *both horns equally pointed. However, Venus was now become too narrow a crescent for a rounded shape of either horn to be expected.*

$8^h\ 20'$, the same, without perceptible inequality.

A series of changeable bad weather.

But on *the 22d of April in the evening*, the hour not being marked in the journal, *the southern horn* appeared to be illuminated *only half as broad as the northern.*

April 23d, $5^h\ 45'$, *till after* 6^h *p. m.* With 160 and 288, Venus was *distinct, and her southern horn again much smaller than the northern, according to fig.* 16.

But at 10^h *there appeared no longer any striking difference.* However Venus was already got too low, and I would never advise a careful observer to choose such a time for investigations of this kind.

April 30th, 7^h. *Judging from the outer circle, I found the northern horn running out much longer than the southern.* At the same time the southern appeared sensibly smaller: see fig. 17. I leave these remarkable phases to the judgment of the skilful, but to me they seem inexplicable, except from real shadows of an uneven mountainous surface.

May 3d, 7^h *p. m.* After much rainy weather I saw a similar phase; for though I found both horns, at 7^h 30′, without any sensible difference in their length, yet the northern was evidently broader than the southern.

7^h 45′, still the same.

8^h 25, the southern horn was still somewhat smaller, but only a little.

9^h 45′. Venus being now near the horizon, and undulating in the vapours, I could perceive *no difference in the breadth of the horns.*

May 6th, 5^h 50′ *p. m.* with a very distinct image I found both horns perfectly alike.

May 8th, 8^h 15′ *p. m.* the same, but the image indistinct after storms. No spots; but they are not to be expected in these small phases.

I now longed for fair weather, that I might carefully attend to the twilight from the atmosphere of Venus, which I discovered in 1790, as far as should be practicable in the present less favourable circumstances.

May 9th, 6^h 25′ *p. m.* I found, with full certainty, that though both horns were equally long, the southern at *a*, fig. 18, was *scarcely half so broad as the northern* at *b;* and this was confirmed by continued attention to the object.

7^h 50′, still nearly the same.

At $8^h\ 20'$, on the contrary, the difference was no longer by far so perceptible.*

This day the first traces of Venus's twilight shewed themselves; for the points of the horns appeared to terminate beyond the illuminated hemisphere, in an extremely faint bluish-grey light.

May 10*th*, $6^h\ 40'$. A perfectly similar phase. I found, so as to be quite certain of it, the southern horn only half as broad as the northern; but both horns were equally long.

$7^h\ 30'$, still the same.

$8^h\ 15'$. With 180, 400, and 560 magnifying powers of the 13-feet reflector, and a distinct image, I found traces of the twilight which could not be mistaken. The light grew dimmer and dimmer to the point of both horns, and at the points was so dim, that it seemed to lose itself in the faint light of the sky. A still finer dimmer trace of light shewed itself twinkling at both sides, on the edge of the dark hemisphere, and including this the two horns comprehended sensibly more than a semicircle; but it was too fine and dim for me to measure its extension.

Even if I had not seen this, I should repeatedly have obtained conviction of the particular density of Venus's atmosphere, by the faint colour of the points of the horns, and of the boundary of illumination.

* It is scarcely necessary to put the reader in mind, that small, undulating, knotty inequalities of the boundary of light, in such observations, must not be taken for true inequalities, or mountains of Venus. In general, these small crescents, as the enlightened part lies obliquely to the eye, are not well suited for observing the true inequalities of the boundary line, or any spots there may happen to be. For such observations, we should be assiduous in attending to the planet, about the time of its greatest distance from the sun.

In this reflector likewise, as well as in that of 7-feet, the southern horn appeared sensibly smaller than the northern.

May 12th and 13th, I perceived again traces of the twilight of Venus; but the stormy state of the air rendered it too bad for such nice observations.

May 16th, after sunset to 8^h 40′, I had, for the third time, the pleasure of observing this crepuscular light of Venus's atmosphere, with the 13-feet reflector. Although the circumstances were not by far so favourable for such observations as when I discovered it in the year 1790, and the luminous appearance therefore came to the eye sensibly weaker and more indistinct than at that time, yet all was confirmed; and in this observation I thought it worth remarking, that the dim crepuscular light seemed to extend sensibly further on the southern than on the northern horn, though this might easily be a deception.

May 19th, after sunset, the light now coming to the eye sensibly clearer, I found the circumstance just noticed to be again the same, with 97 of the 7-feet HERSCH. and 136 of the 13-feet.

Hitherto the circumstances had not been favourable enough for a repetition of the measurement, and therefore I was eager for a better observation.

But *May 20th,* Venus was covered with clouds. However, at length I succeeded in a measurement,

May 21st, at 8^h 30′, *p. m.* six days before the inferior conjunction, and consequently just the same time as in the year 1790. Venus being rather too low for the 13-feet, and for the 7-feet HERSCH. I employed the 7-feet SCHR.; and found the crepuscular light beautiful, and sufficiently distinct. *It* *extended,* according to fig. 19, (Tab. XV.) *from the proper points of the horns a, b, a considerable way, on the edge of the dark hemisphere, to d, e; and equally far on both sides, having the appearance of a very dim, constantly decreasing light.* But I must remark, that in the present more unfavourable situation of Venus, it did not affect the eye as a bluish-grey light, which was its appearance March 12, 1790, but only as a dim grey light.

According to my usual projection-measure,* in which each decimal line of the projection table is equal to 4″ of space, I found the apparent diameter of the planet *a c b*, after repeated trials, = 15 lines = 60″; the projection of the crepuscular light running into the dark hemisphere *a d, b e* = 25 lines = 10″, *and fully so, being rather more than less.*

As the crepuscular light could be distinguished from that of the points of the horns, by its sensibly fainter colour, I was able to measure it from the points. But in order to know with certainty whether I had taken the true termination of the

* In the year 1790, as well as in 1793, I measured this crepuscular light with a projection-machine, which is nothing more than a very simple projection-micrometer, useful in many cases, both by day and night: it gives, for all magnifying powers, the measure of the projected object immediately in minutes and seconds of space, without the necessity of first measuring a fundamental line. I contrived it for my purpose of a selenotopography, and constructed it myself. After an experience of many years, I certainly would not lay it aside, in most cases, it being so quick in the use. I have described it, in all its simplicity, in my "*Beyträge zu den neuesten astronomischen Entdeckungen,*" p. 210, where the *older* lamp-micrometer of the worthy Dr. HERSCHEL is *also described* BEFORE, *p.* 138, with which this machine may be compared. It has never made pretensions to be a new invention, because projection-micrometers of many kinds, for example, accompanying microscopes, have long been known. I remember with pleasure that, even in the year 1778, the window frames were my projection-micrometer, on which I determined the proportion of magnifying powers to one another.

horns *a*, *b*, for the foundation of my measurement, I measured likewise the two lines *a f*, *b g*, perpendicular on the line of the cusps. I found the northern side $f\,a = 8$ lines $= 32''$, the southern only FULLY 7 to $7\frac{1}{2}$ lines; mean, $7{,}25 = 29''$; consequently both sides together $f\,a + b\,g = 61''$, therefore the mean of each side $= 30'',5$; but if the southern side $b\,g$ be put $= 7{,}5$ lines, the mean will be fully $= 31''$; so that, as the semidiameter, according to the first measurement, could amount only to $30''$, I probably observed the cusps as projecting $0'',5$, and perhaps something more, beyond their proper line;* and consequently *the projection of the crepuscular light, which extended into the dark hemisphere, was certainly and at least as* 1 : 6 *in proportion to the apparent diameter.*

My success in this measurement was the more lucky, as on the 22*d of May* Venus could no longer be discerned, though the air was clear.

These are my late observations, made about the time of the greatest eastern elongation, in the year 1793; and continued three months to the inferior conjunction. Under my present circumstances, I hope to be excused for giving them with such prolixity; but I should quite weary the reader, were I now to lay before him likewise my further observations, continued to the last western elongation; which, therefore, I shall rather reserve to another occasion, especially as they contain little that is interesting.

However, I must not leave unnoticed some conclusions, remarks, and explanations, which are deducible from these observations; and which have for their object, partly the moun-

* The remarks and computations that follow hereafter, will shew that the penumbra was probably included in the measurement.

tainous inequalities and period of rotation which I formerly discovered, and partly the atmosphere and crepuscule of Venus.

I. *Remarks on the Mountains and Rotation of Venus.*

1. As I have already said, I gave, in the memoir on this subject published last summer, only those observations which particularly belonged to the object, out of a *very great number* that I had made during 13 years; and I omitted the rest, because otherwise they would have amounted to a volume alone. Now with regard to those I have communicated, and which shew the real existence of considerable mountains, as well as an approximate determination of the rotation, the respectable author of the paper against me has not observed the planet Venus *once at the same time*, which might easily be the case in only 38 observations, that are adduced from a period of 15 years. But in the numerous remaining observations, I saw neither mountains, inequalities, nor spots, any more than the author; and I doubt not, that among these observations I should find many which were made at the same times as when he observed. The same holds good

2. With respect to the new observations for three months, here communicated, which amount to more than 100, and were made at various hours, and on different days. Of the 25 adduced by my opponent, there are *only* 4 made nearly at the same hour, which is the chief circumstance; and not only *in all these, but likewise in very many other observations, I saw, exactly as he did, no spot, and both horns like each other:* so that of all his observations, not one contradicts mine. And yet it would

not be a decisive contradiction, if some observations made at the same time by another person, were in opposition (though that is not the case) *to so many of mine, made in various ways, yet agreeing together;* because, when fallacy of vision is in question, it may always be doubted which of the two observers is deceived; since this depends on the goodness of sight and of instruments, but much more on care and caution.*

I confess impartially, that, before reading the observations contained in my two memoirs, I should have formed the same judgment from those of the abovementioned author that he has done; and on that account his paper is highly valuable to me, as leading to a more scrupulous examination of new truths.

3. However, that which these new observations, here communicated, clear up and confirm, in correspondence with my older ones, on the mountains and rotation, is, that the planet Venus has very considerable mountains and elevated ridges; and indeed the most and the highest in her southern hemisphere. This appears

(*a*) From the observations of the boundary of illumination, which is not sharply terminated, and seems formed of light and greyish shadow indistinctly intermingled. This is chiefly to be perceived only about the time of the greatest elongation,

* By comparing the respective times of the two observers, it appears that both of them viewed the planet on the 4th of April at $7^h\ 30'$, *p. m.*; the 5th April at $6^h\ 38'$, the 6th April at $6^h\ 38'$, and the 7th at $7^h\ 15'$, *exactly at the same time, and saw exactly the same appearance.* The comparison of these observations is the more instructive, because I did not, like my opponent, observe Venus only once, but as often as was possible each day, and *at other times, on the same days, found evident changes;* for this shews plainly enough, that whoever wishes to see the same, and as much, in Venus, must observe with equal industry, and on each day as many hours as possible, with the same care.

when the eye looks perpendicularly through the dense atmosphere of Venus, and by no means in the small crescent form of light, when the lines of vision are much longer and more oblique through that atmosphere: it is in the former position of the planet alone that it can be seen distinctly, but even then not always equally so. One of the finest scenes of this kind was afforded (for example) by the observation I have adduced of the 9th, when Dr. CHLADNI viewed the planet with me. A less striking inequality, though perfectly certain, was discovered by my learned friend Dr. OLBERS, July 31, 1793, at $11^h\ 5'$ in the forenoon, which we both observed and delineated in the same place, and exactly similar, after we had been observing since $3^h\ 15'$ in the morning, but till that time saw no inequality. Were these small indentations or darker places merely atmospherical, no reason can be perceived why they should shew themselves only in the boundary of illumination, and not in the other enlightened parts also.

(*b*) The same thing appears, moreover, from the irregular form which the arch bounding the illumination sometimes assumes, and from the phænomenon thence arising of the much smaller size of one horn, and particularly the southern, in the crescent-shaped phases of the planet; as is shewn, on the same grounds, by the observations contained in my former memoir on the rotation.

Were these observations, as is alleged of the rest, nothing but fallacy, I should wish to know the reason, why that deception happens only sometimes, continues only some hours, and almost always takes place on the southern horn only, very seldom on the northern. Whoever compares together the observations of this kind contained in my memoir on the rotation, to which I have referred, § 12 to 23, will find 14 in which the southern horn appeared much smaller than the northern, but only one or two instances of the opposite phænomenon. And, if it were merely deception, why does the smaller horn, when the planet is seen through light clouds, always disappear sooner than the broader one, and become visible again later? (See § 12. No. 4. of the memoir.)

It further appears likewise,

(*c*) From the observation, that *sometimes, though much seldomer*, one horn, and particularly the southern, is seen rounded *about the time of the elongations*, but the other pointed. And by this very circumstance chiefly is

4. The period of rotation, which I had concluded to be nearly $23^h\ 21'$, confirmed and rendered evident by the new observations given above.

Having already explained this curious circumstance when the observations themselves were stated, I will here only make the following remarks.

(*a*) If the very remarkable observations of the 26th, 27th, 28th February, 13th March, 2d and 3d April, when the southern horn appeared rounded, but the northern pointed, are compared together, the abovementioned period will be found to suit them all, during an interval of 37 days, as exactly as can possibly be expected, and indeed to very inconsiderable fractions. If, on the other hand, they are compared with the older observations of this phænomenon, namely, those of 28th December, 1789, 31st January, 1790,* the 25th, 27th, and 30th

* See *Selen. Fragm.* § 522.

Dec. 1791, and the 11th Jan. 1792,* the differences are more considerable. Thus, for example, from the most distant observation, on the 28th Dec. 1789, at 5^h *p. m.* to the 27th Feb. 1793, at $6^h\ 41'$ *p. m.* are 1157 days 1 hour and 41 minutes, which dividing into 1189,28 revolutions, might occasion some doubt. But,

(α) In each separate period, several observations correspond as well as can be desired:

(β) The period is only assigned nearly, but the interval of more than four years is very long, so that an error of seconds may occasion such an excess; and accordingly the abovementioned time would divide even with a period of $23^h\ 21'\ 19''$: and

(γ) In such computations, no regard is paid to the inequalities of the planet, nor to the middle of the duration of the phænomenon: wherefore so considerable a length of time can never be divided exactly by the period; as my observations of the rotation of Jupiter likewise could not, under similar circumstances, though the period of that rotation is sufficiently well known.†

(*b*) A like doubt might arise from the phænomenon being sometimes not at all or very doubtfully perceived, about the times of the greatest elongations, even at the hours when it was to be expected, according to the period. Hitherto, however, during more than four years, only three instances of this have occurred to me; which were in the years 1790 and 1791, and about the time of the late western elongation, in August, 1793; in which last I only *twice* perceived *barely a trace* of a somewhat rounded form on the southern horn. Moreover, as often

* See *Beob. über die sehr beträchtlichen Gebirge und Rotation der Venus,* § 26 to 30.
† *Beyträge zu den neuesten Astronom. Entd.* p. 1 to 138.

happens, the weather was not always favourable; and besides, the observations already communicated contain sufficiently evident marks of a libration, whence such cases may be easily explained. So, for example, the mountainous ridges of the moon's southern edge, *Leibnitz* and *Doerfel,* do not shew themselves quite clearly at each rotation, but only sometimes arrive at their full projection.

(*c*) But the very circumstance, that during more than four years, in so great a number of observations, I have perceived this phænomenon only ELEVEN TIMES with perfect certainty, and only a few other times uncertainly, and that in all the intervals I have expected it in vain, notwithstanding my frequent wishes, seems alone to shew, evidently enough, that I cannot have been deceived; especially as those appearances have been seen, with various magnifying powers of different telescopes, and in several instances with different eyes, perfectly alike, and with full certainty; and it is not reconcileable to our understanding, how such a fallacy should, at different times, always preserve one and the same period.

The following example, which I here take an opportunity of adducing as remarkable, may shew how cautious we ought to be, in drawing conclusions from our own observations, against the truth of those made by others. Jan. 5, I reviewed with the 13 and 25-feet reflectors the Mare Crisium (HEVEL. Palus Mæotis) in the moon, and made some observations. The following day, Dr. OLBERS of Bremen, who now pursues his observations with an extremely good 5-feet DOLLOND of $3\frac{3}{4}$ inches aperture, mentioned to me, that he had discovered *the preceding evening*, in the *Mare Crisium*, between Picard and Auzout, two small craters in the grey plain, which were both wanting in my topographical charts; and about which, therefore, the question might arise, whether they were not newly produced?—I had seen nothing of them with my more powerful instruments. Again, on the 6th, I examined the part of the surface which he had exactly pointed out, with powers 136 and 300 of the 13-feet reflector, and found nothing. The 17th, I looked for them with the 7-feet, in vain. I did the same on the 3d February, with 179 of the 25-feet, and likewise on the 6th, but found not these craters. Hence I might have concluded, with probability, that the learned observer had been exposed to some deception; and perhaps I should have been believed. *And yet Dr.* OLBERS *was perfectly in the right.* On the 6th of March, I readily found the largest of these two craters, without seeking for it long, and saw it *uncommonly sharp and clear,* with 160 and 280 of the 7-feet SCHR. It is very nearly as big as a crater which I discovered last year, lying also in the plain, between the eastern bounding mountains, where they break down; it is surrounded with a broad, and proportionably flatter, annular elevation, of little brightness; it appears to be uncommonly deep, in proportion to its breadth; and if a straight line be conceived, running from Picard* towards the middle of the southern boundary mountain, which projects inward in the shape of a wedge, it lies on this line about $\frac{2}{3}$ distant from Picard. As I have examined this tract of the Mare Crisium very often, and under the most favourable angles of illumination,† in searching for the veins of mountains, or the flat mountainous layers to be found there, but *never perceived the slightest trace* of these craters, the

* See Tab. VI. of the *Selenotop. Fragmente.*

† See Tab. XXXIII. XXXIV. and XXXV. of the same work; and § 355 to 397.

observation of Dr. OLBERS is certainly not unimportant, and it will on occasion be further explained.

If any astronomer shall think it worth the trouble to observe *Venus, not barely now and then, at whatever time of the day it may be, but continually, with the same persevering zeal, and when the weather is favourable almost hourly, about the time of her greatest distance from the sun,* I am convinced that he will certainly perceive the rare phænomenon in question, just as well as I have done. If, contrary to all reasons which hitherto appear, I should hereafter be convinced that I was deceived, I would myself, willingly and impartially, bring the offering to truth; and so much the more readily, as no indirect views have ever led me on, but I have been actuated solely by an irresistible impulse to observe; and because I certainly shall never have reason to be ashamed of the observations I have laid before the world, which have always conducted me to new truths.

II. *Further Explanation and Correspondence of Computations of the Twilight, together with Remarks on the other Properties of the Atmosphere of Venus.**

As the celebrated author of the paper so often mentioned, "on the planet Venus," though he confirmed my discovery of the twilight of Venus's atmosphere, *yet represents the computation of it, p.* 16 *and* 17,† *as not demonstrated, and positively as very inaccurate,* which may, without any foundation, be injurious to the truth, it becomes my duty to give some explana-

* Many of the explanations and remarks in this section come from Dr. OLBERS of Bremen, who, at my request, kindly undertook not only to examine the old computation, but also to compare the calculations deducible from the new observations.

† P. 214 and 215, Phil. Trans. for 1793.

tions and remarks, that persons skilled in those matters may be better able to form a right judgment of my new computation, which agrees excellently with the old one; and at the same time may determine, whether there be inaccuracy and error, and on whose side it lies.

1. The first objection is concerning the apparent diameter of the sun, as seen from Venus, which I have assumed at 44′, in the computation of the penumbra, smaller, it is alleged, than I ought to have taken it.

M. DE LA LANDE puts the diameter of the sun in the apogee $= 31' \, 31'' = 1891''$. Now the apparent diameter seen from Venus

$$= \frac{1891'' \times \textit{dist. } \odot\textit{lis in apog.}}{\textit{dist. Ven. a sole}};$$

consequently,

$$\begin{aligned} \text{log. } 1891 &= 3{,}276692 \\ \text{log. dist. } \odot &= 0{,}007231 \\ \hline & \quad 3{,}283923 \end{aligned}$$

log. of the distance of Venus *in aphel.* -	9,862318
log. of the distance of Venus *in perihel.* -	9,856337
log. of the diameter of the sun *in aphel.*	3,421605
log. of the diameter of the sun *in perihel.* -	3,427586

Diameter *in aphel.* $= 2640'',0 = 44',0$

Diameter *in perihel.* $= 2676'',6 = 44' \, 36'',6$

But if the assumed diameter of the sun in the apogee 1891″ be corrected for the irradiation, which may be put $= 6''$ (DE LA LANDE *Astron.* § 1388), we have the diameter of the sun seen from Venus

in aphel. = 43′ 51″,6

in perihel. = 44′ 28″,1

I really do not see, therefore, how the diameter of the sun seen from Venus could be expressed generally, and with respect to every part of her orbit, more accurately, than as 44′, the quantity taken for the calculation. And indeed equally unimportant, must be considered

2. The remark on my computation of the penumbra. The sense of the note on that subject, which I have given, p. 313 (Phil. Trans. for 1792), is plain enough, that, as the sun is seen in Venus under an angle of 44′, the penumbra, assuming the diameter of Venus = 60″, can amount only to 0″,38 in the middle of her disc; but that as Venus, when her diameter is so large, can only appear under the phase of a crescent, the penumbra can scarcely amount to $\frac{1}{10}$ of a second in the perpendicular diameter on the line of the cusps. Instead of 0″,38, or still more accurately 0″,384, by an error of writing or computation 0″36 was set down: but what does this inconsiderable difference, *of* $\frac{1}{50}$ *sec.* impede in the conclusion, *that the penumbra at the boundary of light on the disc, or in the perpendicular direction on the line of the horns,* is imperceptible? and how could so unimportant a matter deserve the least notice?

3. *With respect to the twilight itself of Venus's atmosphere, and the computation of it,* the paper in question contains, p. 16 and 17, three objections: (*a*) that I had *overlooked the penumbra,* which, in the projection I have given of the crepuscule 15° 19′ is said to amount to *more than* 2°$\frac{1}{3}$, or, as this error of computation was corrected in my copy, to 1° 11′ 47″,6; (*b*) that my 7-feet speculum must be tarnished, *because I have measured the*

projected extent TOO SMALL; and (*c*) that *my calculations are so full of inaccuracies, that it would be necessary to go over them again, and compare them* EXACTLY WITH THOSE MADE BY MY OPPONENT.

It requires, indeed, little examination to perceive, that all these objections are groundless.

(*a*) That I did pay attention to the penumbra, my paper "on the atmosphere of Venus" shews plainly enough; and it is readily to be conceived, that the points of the horns, illuminated by refraction and penumbra, must project beyond the enlightened semicircle into the dark side. And it would also be easy to shew, how the points must project more beyond the enlightened semicircle, in proportion as the phase of Venus is that of a sharper crescent; with regard to which, I will hereafter determine, more accurately than my opponent has done, how much the projecting excess of the arch must be. But the author has not considered *that, in my way of making the measurement, it was quite unnecessary to take the penumbra into the computation*; for I measured the faint light, of a bluish-grey colour, which ran on along the edge of the dark hemisphere, according to fig. 20, (where A D indicates a diameter of Venus, parallel to the line of the horns) not, as he did 3 years after, from A, but only from B (the extreme visible point of the horn, still faintly illuminated by refraction and the diameter of the sun) to C; *and consequently I had, by the observation itself, already deducted the penumbra.* It is indeed possible, that at B and E, where the penumbra seemed to me to terminate, it yet might not be quite at an end; but the excess must be indefinitely small, since the *whole* projection of the penumbra, from A to B, and from D to E, could not, by my calculation, amount to more than 0,63 second.

In general, such accuracy of computation avails nothing, *because the observations and measurements of such a very faint and always decreasing light, cannot be so very exact.* This is particularly shewn, and strikingly enough, in the two measurements of 20th May, 1793, given in the paper of my opponent; where the projection of this crepuscular light, taking the apparent diameter of Venus = 60″, was one time 12″,5, and the other time only 7″,7. And so much the more unimportant is it in the result of my calculation, that I assumed the crepuscular light as having been measured from A. But that in my way of measuring, in which the penumbra is abstracted by the observation itself, I have been happier and more accurate, is testified by the computations to be given presently of my two measurements of the years 1790 and 1793, which were made under different circumstances, and yet correspond uncommonly well.

(*b*) The second objection, *that I have measured the projection of the twilight too small,* is equally unfounded; for

(α) The projection found by the author must properly be somewhat larger than mine, because he did not, like me, measure the magnitude B C, but A C, fig. 20; and

(β) It will appear from the following computations, that I have found it AT LEAST AS LARGE as he did, without reckoning in the difference from A to B. He did not consider, that three years before I had observed under other circumstances, *which must make the extent of the crepuscule appear less;* and in general I do not perceive how he can form such a judgment from his two measurements, which differ from one another so very much as $\frac{1}{3}$ of the whole magnitude. I can also assure him, that the 7-feet speculum, which I obtained in the year 1786 by his friendly kindness, has continued always so precious to me, that I have kept it in perfectly good condition to the present time.

As to

(*c*) the objection, *that my calculation abounds with inaccuracies,* it is indeed true, that the observation of March 12th, 1790, was not rigorously computed, yet *its exactness was carried much further than is necessary in observations of this kind;* for no one will comprehend the use of a scholastic computation to seconds and decimal parts of seconds, when the observations themselves leave an uncertainty of many minutes. However, to remove all doubt in this respect, and to save the author the trouble of a further careful comparison with his two measurements, I will here not only repeat the calculation in all its rigour, but also add the new one for my second measurement of the 21st May, 1793, and compare both together, as well as with that of my opponent.

(α) *Calculation of my observation of the* 12*th March* 1790, $6^h\ 0'$, *p. m.*

The time of this observation may be taken, without scruple, as $6^h\ 0'$ mean Paris time; for it was made after 6 o'clock at Lilienthal. The equation of time amounts to 10′, and the difference of meridians to 26′; therefore, if the observation had been made exactly at six, this would be $5^h\ 44'$ mean time at Paris.

Now, according to the latest tables by M. DE LA LANDE, we have, for that moment,

heliocentr. long. of Venus -	=	$5^s\ 18^\circ\ 41'\ 53''$
long. of the earth -	=	5 22 22 45
difference - -	=	3 40 52
heliocentr. latit. of Venus	=	3 23 6

therefore,

log. cos. $3^\circ\ 40'\ 52''$ -	9,9991030
log. cos. 3 23 6 -	9,9992416
	9,9983446

angle at the sun -	=	$4^\circ\ 59'\ 58''$
sum of the other angles -	=	175 0 2
half sum - -	=	87 30 1
log. of the distance of the earth from the sun	=	9,997766
log. of the distance of Venus from the sun	=	9,857040
log. tang. -		10,140726
	=	$54^\circ\ 7'\ 28''$
subtract		45 0 0
remain		9 7 28
log. tang. $9^\circ\ 7'\ 28''$ - -		9,205777
log. tang. 87 30 1 - -		11,359955
log. tang.		10,565732
half difference - -	=	$74^\circ\ 47'\ 42''$
half sum - -	=	87 30 1
angle at Venus -	=	162 17 43

angle at the earth = $12° 42' 19''$
compl. of the angle at Venus = $17 \; 42 \; 17$

Now the crepuscular light of Venus, the measure being considered as a chord, extended $15° 19'$; then consequently it is,

log. sin.	$15° 19' 0''$	-	9,421857
log. sin.	$17 \; 42 \; 17$	-	9,483033
log. sin.			8,904890
			$= 4° 36' 28''$

To so much, therefore, amounts the arch of a great circle, over which the crepuscule of Venus's atmosphere extends, as far as it can be distinguished on our earth, under favourable circumstances. According to my former computation, it came to $4° 38' 30''$: *wherefore the whole difference, certainly very inconsiderable to be given as an instance of inaccuracy, amounts* ONLY TO 2 MINUTES; and it is surely quite superfluous to include seconds in a calculation, which, from the circumstances of the observation, can only be depended on to several minutes.

If it be wished to take this opportunity of determining the arch, how far the points of the horns project on account of the apparent diameter of the sun seen from Venus, put the semi-diameter of the sun seen from the earth at the abovementioned time, deducting $3''$ for irradiation, $= 16' \; 3'',3 = 963'',3$

log. 963,3	-	2,983762
log. dist. ☉	=	9,997766
		2,981528
log. dist. ♀ a ☉		9,857040

l. sin. d. ⊙ *ex* ♀ -	3,1244488 = 1332″,0 = 22′ 12″
l. sin. 17° 42′ 17″	9,4830033
	3,6414555 = 4379″,8 = 1° 12′ 59″,8.

This is the quantity which, in the paper of my opponent, was erroneously stated at more than $2°\frac{1}{3}$, because the diameter was taken instead of the semidiameter; it was afterwards corrected to 1° 11′ 47″,6; but it is highly probable, that the points of the horns project still further, *on account of refraction*. However, as we do not know the quantity of the horizontal refraction on Venus, this cannot be ascertained with any certainty. It is sufficient for me, that I measured the crepuscular arch from the point where the extremity of the horn seemed to end in my instrument, and to my eye.

(β) *Calculation of my late observation of the* 21*st May*, 1793.

The time falls on 8h 0′, mean Paris time; and therefore we have,

long. of the earth	=	8s	1°	5′	55″
heliocentr. long. of ♀	=	7	27	13	27
difference	=		3	52	8
lat. of Venus -	=		1	1	37,6

log. cos. 3° 52′ 8″ -	9,9990091
log. cos. 1 1 37 -	9,9999302
log. cos. of the angle -	9,9989393

angle at the sun	=	4°	0′	10″
sum of the other angles	=	175	59	50
half sum	=	87	59	55.

log. dist. of the earth from ☉ = 0,005628
log. dist. of Venus from ☉ = 9,860276

log. tang. 10,145352
= 54° 24′ 50″
subtract 45 0 0

remain 9 24 50

log. tang. 9° 24′ 50″ - 9,219579
log. tang. 87 59 55 - 11,456615

log. tang. 10,676194
half difference = 78° ′5 53″
half sum - = 87 59 55

angle at Venus - = 166 5 48
angle at the earth = 9 54 2
compl. of the angle at ♀ = 13 54 12

Now, as I have stated above, the projected extent of the twilight measured 10″, putting the semidiameter of Venus = 30″; and, as I then measured that extension perpendicularly on the line of the cusps, these 10″ may be considered as a sine, and so the arch will amount to 19° 28′. Then

log. sin. 13° 54′ 12″ 9,380725
log. sin. 19 28 - 9,522781

8,903506

= 4° 35′ 34″

To so much, therefore, amounts, according to my *second* observation, the arch of a great circle, over which the twilight of Venus's atmosphere extends, as far as we can discern it under favourable circumstances, and which we may put in comparison with our common twilight.

If this result be compared with that of my older observation of the 12th March, 1790, which was 4° 36′ 28″, it will be seen *that the two agree much more nearly than could have been expected in such delicate observations, namely, to the very inconsiderable difference of one minute;* and this is the more striking, as, according to the different situations of Venus, and the modifications of our own atmosphere, this crepuscular light is not likely to be ever observed, at different times, exactly of the same extent.

Great, however, as this agreement is, I am far from regarding it as any thing but a lucky accident. Whoever considers the manner of measuring, and the nature of the observed object, will be easily convinced, that we can never determine quite exactly the length of the twilight of Venus. The most accurate measurements of this kind admit errors of $\frac{1}{2}''$ in the projected extension; and this $\frac{1}{2}''$ alone would amount nearly to $\frac{1}{4}°$ in the computed arch of the great circle. Moreover the crepuscular light gradually decreases, and I only pretend to shew how far it continued visible, in my observation, with my eyes and instruments, under the state of the atmospheres of Venus and the earth at that time: the part which was thus visible to me extended, according to the computation given above, over something more than 4° $\frac{1}{2}$ of a great circle. But I am convinced that, under favourable circumstances of the weather, situation of Venus, and perfection of instruments, the atmosphere of Venus might possibly be traced something further: this, however, has not been done, at least as yet; for if we compare with these measurements and calculations, which are certainly as accurate as I could make them,

(γ) *Dr.* HERSCHEL's *observation of the 20th May*, 1793, when he measured the projection of the horns beyond a semicircle, in the evening likewise, about half past eight, *but a day earlier* than I did; it will be seen that he determines the magnitude of this projection on a mean from two measurements, *with the extreme exactness of* DECIMAL PARTS *of a second*, to be 18° 9′ 8″,2. But this mean is from two measurements which differ from each other, not barely by seconds or minutes, but by MANY DEGREES. In order to judge of the dependance to be placed on them, I will consider each of his measurements separately.

Ist measure.	log. 500	2,6989700
	log. 1195	3,0773679
		9,6216021
		= 24° 44′ 3″
IId measure.	log. 620	2,7923917
	log. 2400	3,3802112
		9,4121805
		= 14° 58′ 18″

His two measurements, therefore, give separately, the first 24° 44′, the second only 14° 58′. *An enormous difference of almost ten degrees*, which, according to my humble judgment, leaves the mean uncertain, not to seconds and their *decimal parts*, nor even to minutes, but properly to 5 degrees. It would therefore be useless to compare further with mine two examples, which are so little exact, and agree so ill together; and I must leave it to be judged by others with what reason any person, from such inaccurate measurements, could consider mine as erroneous (which besides were made under other circumstances, in the year 1793), and the calculations founded on them as extremely inexact. Nevertheless, the mean deduced from those examples, namely, 18° 9′, agrees very well with my observation; for the following day, when the projection ought to be greater, I found it 18° 28′; though when it is considered that the penumbra must be deducted from the measurement of my opponent, the mean is somewhat too small. *His observation, therefore, by no means gives the extent of Venus's twilight greater than mine, but rather something less.*

Thus, by these new measurements and computations, the general results I have already deduced in my abovementioned paper "on the atmospheres of Venus and the moon," relative to the atmosphere of Venus, are still more confirmed and justified; and there is no longer any doubt, as my opponent agreeing with me allows, *that the atmosphere of this planet is very dense, like that of the earth.* Here then I might rest with regard to those conclusions; however, I find it useful to add the following explanations, in order to avoid further misunderstanding.

1. Although, according to those results, there is no doubt, that the atmosphere of Venus is as dense as that of our earth, yet I do not see in fact, from my observations, how we can confound, against all analogy, a general density, with particular, local and accidental, temporary modifications and condensations into clouds; and *so positively deny all transparency* to this atmosphere, as to assert *that in the shining of the planet we see by*

no means the light of its body, but merely that of its atmosphere.

Notwithstanding the density of this atmosphere, we must naturally consider it as generally clear and transparent, like our own, and that of the moon, and as losing its transparency only where its matter becomes really condensed; which condensations, however, may be supposed not always to appear like darker spots to an observer on our earth, but to remain often imperceptible to him. At least, I cannot think, contrary to all analogy, that Providence would bless the inhabitants of Venus, incomparably less than ourselves, with the happiness of seeing the works of almighty power, and of discovering, like a HERSCHEL, still more and more distant regions of the universe. We must, at least, adhere to this analogy, till indisputable experiments convince us of the contrary, which, however, according to my numerous observations, is by no means the case.

2. But if the atmosphere of Venus be naturally clear and transparent, like that of our earth, except accidental condensations, we cannot well doubt, that in looking at the planet, we perceive at the same time both the light of its body, and that of its atmosphere, the latter being illuminated partly by the immediate rays of the sun, and partly by reflection from the body of the planet, and by refraction.

3. It is also equally reasonable to suppose, that, as we are ourselves enveloped in a thick atmosphere, and must look, from a great distance, through a dense illuminated atmosphere, not only our own atmosphere, but likewise particularly the density of that of Venus, and the light upon it, as also the various reflections of the light from the body of the planet, and its refractions, will put such impediments in the way, and occasion

such indistinctness, *that we never can distinguish, as we do in the moon, a projection of the land on the surface of the planet, nor even the shadows cast by its mountainous inequalities, unless it be under a combination of every favourable circumstance, and even then only in a faint undefined manner.* This will be more readily apprehended, when we consider, that the shadows on Venus must appear, from the density of her atmosphere, and its reflection and refraction of light, only *dark-grey*, like those on the earth, and not *black*, as they are on the moon.

4. Yet, in the same manner as in the moon, we discern in Venus, even under the most favourable circumstances, only those parts of her surface, which lie nearest to the boundary of illumination, at the time when we see her half enlightened, because then we look, in a shorter line, perpendicularly through her atmosphere, and moreover the reflection and refraction are much less injurious, and the shadows are longest. Only at such times, and when the atmosphere is likewise clear over such parts of her surface, can we see these shadows, which do not appear sharply terminated, but like a faint mixture of greyish shade and light, sensible enough, but not clear.

5. Granting this rational theory, so conformable at least to our experience on this earth, and to analogy, all the phænomena I have pointed out are very easily and clearly explained by it; and this experience shews at the same time the justness of the theory, and that it cannot well be otherwise.

Thus we can naturally account for,

(*a*) The soft mixture of light and shade, to be seen only near the time of the greatest elongations, *yet not always, but only sometimes,* and at those moments alone when the atmosphere there, and our own, are favourable for the purpose: to this belong also the shadows sometimes seen by me at the southern horn, and which separated the extreme point of it wholly, or in part. It is possible likewise, that the atmosphere may be clear in one place alone of the boundary of light, in which case we should see something of a shadow there only, the boundary line appearing in the other parts as usual, not streaked with shade, but only not sharply terminated: so, for instance, it was on the 31st of July last year, when Dr. OLBERS observed here with me.

(*b*) But if Venus be considerably more or less than half enlightened, the shadows are not only shorter in themselves, and less perceptible in so small an image, but likewise we see them obliquely, and in a sensibly longer line through the illuminated atmosphere of the planet, which then covering the shadows more, renders them more difficult to be distinguished, and commonly quite invisible. It is, therefore, useless to expect such appearances of shadow, in small crescent phases of Venus, although she be then vastly nearer, and her apparent diameter much larger. If there are at those times real shadows on her, we see the places, not as spots of shade, but as indentations; and to this belongs the remarkable observation, when the boundary arch of light appears irregular, sometimes in larger and sometimes in smaller parts, and the point of one horn, nay even a considerable part of the horn, is evidently slenderer than the other.

Here it will be readily understood,

(*c*) That as our own atmosphere has an influence on the distinctness of all such phænomena, so accidental condensations in the atmosphere of Venus may cause many bright parts, not lying in the shade, to assume the appearance of dark spots. This accident, however, of which indeed I have no sufficiently certain experience, must occur but seldom, because I have hitherto perceived the mixture of shade and indentation only at the boundary of light; and it would not be easily explained, *why those dark places should not be perceived further in upon the enlightened parts, unless they were true shadows of mountains, and not barely atmospherical appearances.*

Thus at least is every thing to be explained very naturally; and if the phænomena themselves are put out of all doubt by me and others, they confirm the propositions delivered above. And equally insignificant appears to me also, the doubt which

6. A phænomenon might raise, that occurred to my opponent only or chiefly in April of last year: the same, as may easily be supposed, was seen by me many years ago, but especially in 1790, and frequently since; though, not thinking it particularly instructive or remarkable, I forgot to deliver it separately in my paper "on the atmospheres of Venus and the moon."

The phænomenon in question, according to my older observations, consists in this; that the external edge, for a very small breadth, appears incomparably brighter than the rest of the enlightened part, nearer to the boundary of light; and forms a much brighter small border, which is sharply terminated at its outer edge, but on its inner side appears without any sharp boundary, losing itself in the weak light of the rest of the illuminated part; so that in general, the falling off, or gradual diminution, of the light toward the line bounding the illumination, *is perceived according to the photometrical laws, but particularly becomes chiefly striking nearer to the boundary of light.*

What seems to deserve further attention in this phænomenon, is the circumstance, *that I have seen this extremely brighter border at the edge, not only about the time of Venus's greatest digressions from the sun, when she appears to us half enlightened, or more, but also equally well very near the conjunction; and particularly plain in the year* 1790, *when she had the very smallest crescent phase, not amounting to more than from* 4 *to* 6 *seconds in breadth.*

Were it not for this remarkable circumstance, I should look for the cause solely in the greater quantity of light, which, when the planet has the phase of being half, or almost half illuminated, falls quite or nearly perpendicular through its atmosphere, on the surface which appears to us the edge, and is reflected back from this surface into the atmosphere, by which it is again reflected, and in various ways refracted, so that at the edge, against which we look by an oblique long line through the atmosphere, we see an exceeding quantity of light, *being that of the planet and its atmosphere at the same time;* but the abovementioned observation seemed to make it probable, that, as I have always believed, the appearance chiefly depends on optical fallacy, yet this still requires further investigation. However, though we are as little acquainted with the natural constitution of the ball of the planet, in respect to its power of reflecting more or less light, as with the species of the refraction there, yet it seems contrary to all analogy, that the atmosphere of this heavenly body should be an *opaque* cover, capable of reflecting more light than the solid body itself; yet that we *should see the external edge, not faintly expressed, in the manner of an atmosphere, but sharply terminated; and, on the other hand, the boundary of light, under* *favourable circumstances, streaked with shade,* exhibiting an irregular arch of termination, with indented spots, unequal horns, and so forth. I shall, therefore, at least till adequate reasons convince me otherwise, never assent to a bare hypothesis, that in this planet we merely see its atmospherical cover, and never the body itself; unless when, very rarely, a clearing up of its atmosphere allows us to get sight of a small part of its real surface, in the dark form of a cloud-like spot.

Finally, as to what the celebrated author has remarked besides, *on the apparent diameter of Venus, in the mean distance of the earth;* namely, that by a mean of the measurements he made Nov. 24th, 1791, with the 20-feet reflector, it amounts with great certainty, to 18″,79; and that therefore the planet is larger than it has been given by astronomers hitherto: this is a matter which belongs only indirectly to my object here.

I could have wished that he had not depended too much on a single instrument, having an excess of light, in which the irradiation may unobservedly extend further than in weaker telescopes, nor on a single micrometer; but had reduced his mean from many measurements, made with various and less powerful telescopes, and on many days, under very different apparent diameters, in order to his conclusion for the mean distance of the earth; because, as I only observe here previously, for want of room, I doubt very much of the dependence to be placed on those measures; and must consider this, at least, as rather too large, until I can convince myself of the contrary.

Comparing this determination with that which has been adopted hitherto, according to M. DE LA LANDE, namely 16″,7, it follows by calculation, that on the 12th March, 1790, when I found the apparent diameter 59 to 60 seconds, it should have been, by M. DE LA LANDE, 58″,58, but by this new determination, 65″,91; and on the 21st May, 1793, when I found it greater in proportion, probably because the planet was lower, and had therefore more irradiation, namely 60 seconds, it should by M. DE LA LANDE have been only 56″,75, but by the new determination 63″,85; consequently, according to the latter, I must have overlooked 4 seconds on the 21st May, 1793, and on the 12th March, 1790, when Venus appeared to my eye particularly distinct, *fully 6 seconds.* Both, and especially the last, seem to me contrary to all probability.

As the author, since the year 1780, has measured the diameter 7 different days, so have I before me no less *than* 24 *different* measurements, made since the year 1788 only: in these I took the apparent diameter of Venus, sometimes when she was at a greater, and sometimes at a less distance; not only repeating the measurement each time, but often 6, 7, or more times, with different telescopes, magnifying powers, and projection micrometers. If, out of so considerable a number of observations, the mean of the measurements made at each time be taken, and reduced to the mean distance of the earth from the sun, and then the mean of all these reductions be found, this must give the apparent diameter of Venus, at the mean distance, as exactly as possible. Having so great a number of measurements, I must reserve this subject for a particular memoir: yet I think it my duty previously to announce, that in so many observations, I have always found her apparent diameter agree, to 1 or 2 seconds, with that given in the Ephemerides for the time; and as these are computed on the determination hitherto adopted, of 16″,7, we may continue to reckon Venus of about the same size as she has hitherto been estimated.

Lilienthal,
April 1, 1794.

Fig. 1.

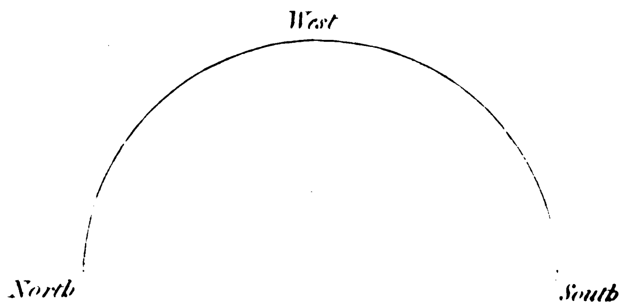
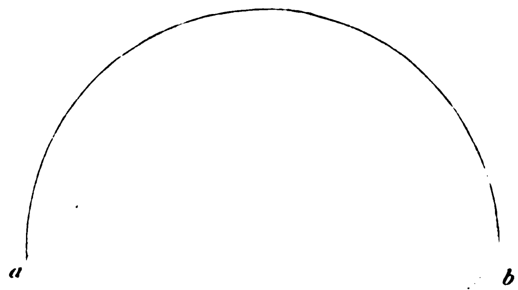


Fig. 2.



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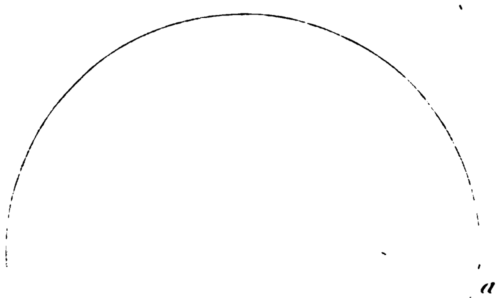

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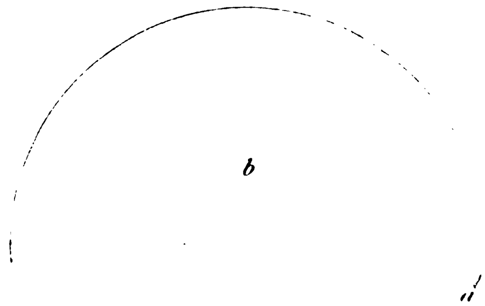

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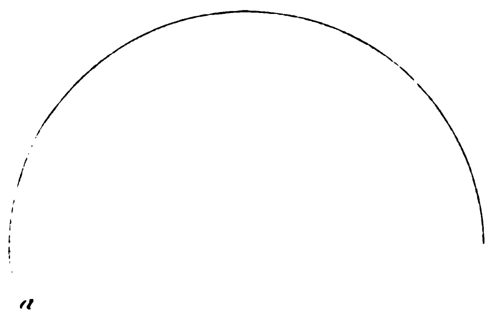

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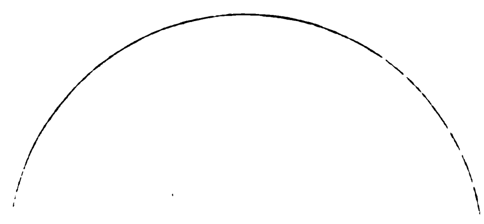

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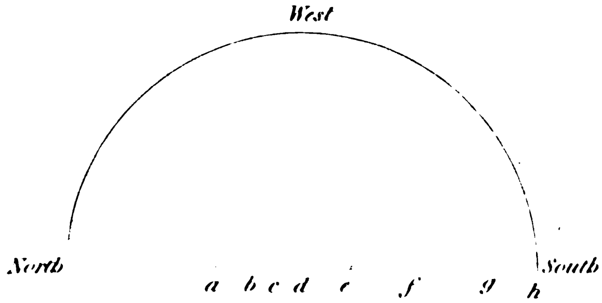

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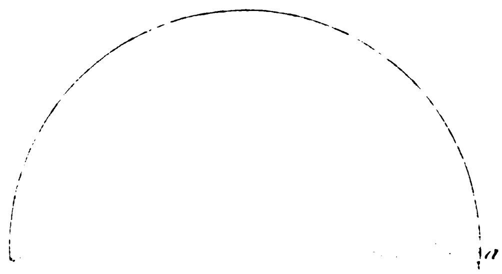

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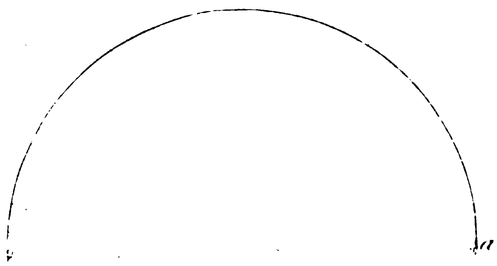

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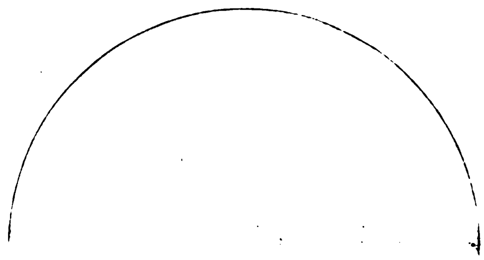

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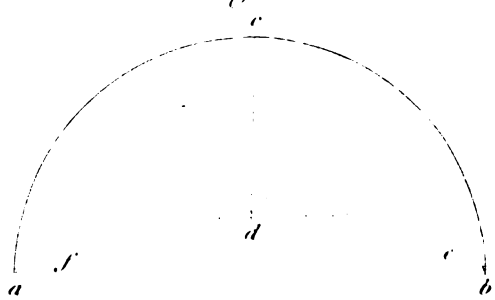

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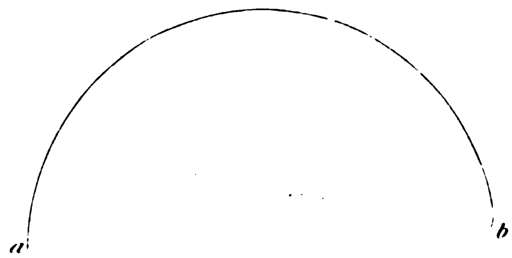

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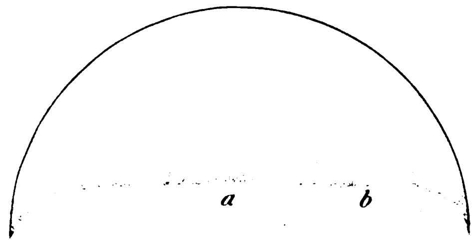

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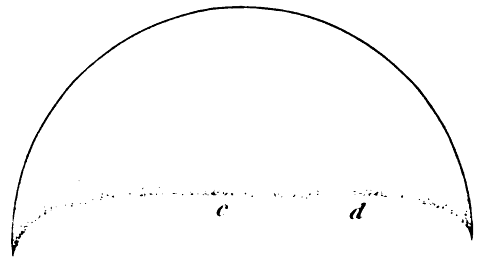

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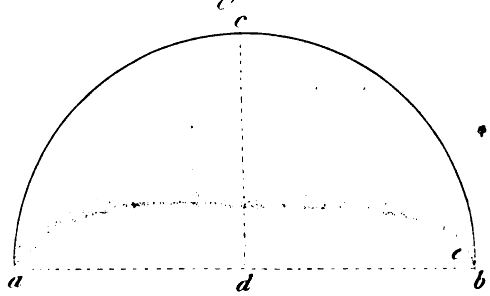
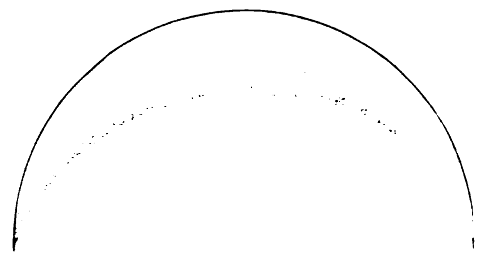


Fig. 16.



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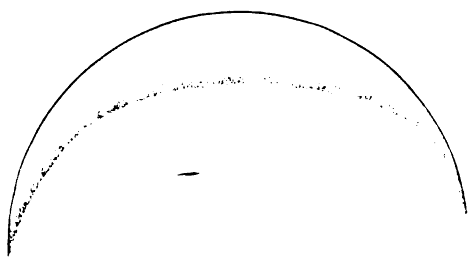

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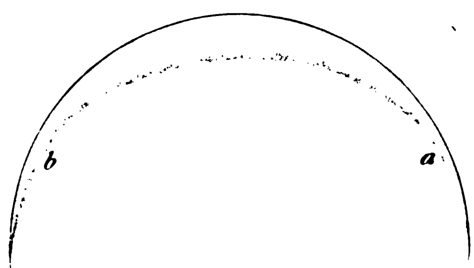

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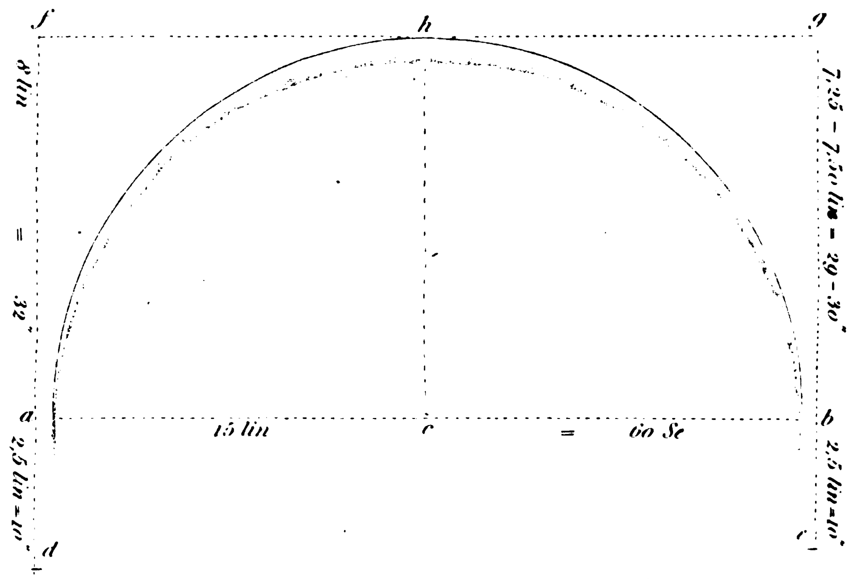
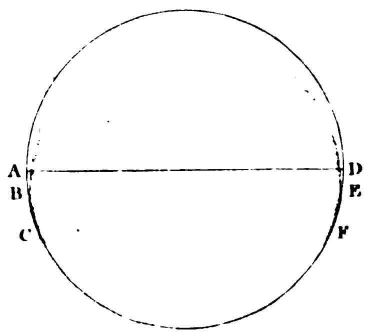


Fig. 20.



VI. *Experiments on the Nerves, particularly on their Reproduction; and on the Spinal Marrow of living Animals. By* William Cruikshank, *Esq. Communicated by the late* John Hunter, *Esq. F. R. S.*

Read June 13, 1776.

THE nerves on which these experiments were made are, the par vagum, and intercostal. The par vagum arise from the basis of the brain, pass through the basis of the skull, along with the internal jugular veins. They are distributed to the tongue, œsophagus, larynx, heart, and lungs; and, running on each side of the œsophagus, may be said to terminate in the stomach, liver, and semilunar ganglion of the intercostals, below the diaphragm; from whence they are again distributed to the viscera of the abdomen. The intercostals also arise from the basis of the brain, pass through the basis of the skull, along with the carotid arteries. They at first run by the fore part of the vertebræ of the neck, still adhering to the coats of these arteries; but having reached the chest, they leave these arteries, and run before the heads of the ribs, where, sending off branches which pass between the ribs, they have thence been named intercostals. Several of these branches uniting, form a trunk on each side, which, running forwards towards the middle of the spine, perforates the diaphragm, and then terminates in the semilunar ganglion of the intercostals. These trunks are distinguished by the name of the anterior intercostals. The original trunks continue their course by the sides of the lumbar vertebræ; after which, they run before the os sacrum, and, approaching nearer each other as they descend, terminate before the os coccygis, in the ganglion coccygeum impar of WALTHER. Their branches all go to the heart, abdominal viscera, testicles in men, and ovaria and uterus in women. The trunks of these nerves are largest in the neck. In the human species, the two nerves of each side are distinct; but in those quadrupeds which I have examined, they are so closely connected through the whole length of the neck, as to make apparently but one nerve. The intercostal is the smallest nerve, and adheres so closely to the other, as to be with difficulty separated from it. They seem to me, likewise, larger in the dog, compared with his bulk, than in the human subject. The neck was the place in which I chose to divide these nerves; it was there they could be got at with least danger, a circumstance which, by making an experiment more simple, makes it consequently more to be relied on; and, in order to put the animal to as little pain as possible, and make the operations short, I chose to divide both nerves at once, rather than take up time in separating them, and dividing them singly; so that, instead of four operations on each animal, I confined myself to two. Instead of mentioning the names of the gentlemen present at each experiment, I shall observe once for all, that two or more of the following gentlemen were present at each experiment, except experiment VII, which I performed, assisted by Mr. HUNTER's servant only:—Messrs. BARFORTH, BAYLEY, DAVIDSON, HARTLEY, HAWKINS, HOME, KUHN, NOBLE, PARRY, MARTIN, SHELDON, WHEATLY; besides others, who came in occasionally, during the time of the experiments, or who afterwards saw the animals, while the described symptoms were taking place.

EXPERIMENT I.

January 24th, 1776, I divided, in a dog, one nerve of the par vagum, with the intercostal, on the right side. The symptoms, consequent to the operation, were heaviness, and slight inflammation of the right eye; breathing with a kind of struggle, as if something stuck in his throat, which he wanted to get up; sullenness, and a disposition to keep quiet: the pulse did not seem much affected, nor had he lost his voice in the least. The unfavourable symptoms did not continue above a day or two; and on the eighth day he was in very high spirits, and seemed perfectly to have recovered.

EXPERIMENT II.

February 3d, I cut out a portion of the two nerves of the opposite side, in the same dog; the piece might be about an inch long. His eyes became instantly red and heavy; his breathing was more difficult than in the former experiment; he was sick, and vomited frequently; the saliva was increased in quantity, and flowed ropy from his mouth; his pulse in the groin was about 160 in a minute; he ate and drank, however, even voraciously at times, and had stools; he never attempted to bark or howl, probably because he did not feel great pain; and yet his attention was not so much disengaged from internal uneasiness, as to be excited with ordinary causes from without; in breathing, the inspirations were slow and deep;

the expirations were attended with repeated jerks of the abdominal muscles, as if he wanted more effectually to expel what air was contained in the lungs. The seventh day after this second operation, he was found dead, at a considerable distance from his bed. In the dead body, every thing seemed in a sound state, except the lungs: these contained little or no air; in consequence of which, they sunk to the bottom in water; they were of a red brown colour, resembling more the substance of a sound liver, than that of inflamed lungs. The inner surface of the trachea and its branches was exceedingly inflamed, and covered with a white fluid, in some places resembling pus, in others ropy, and more of the nature of mucus. The divided nerves of the right side were united by a substance of the same colour as nerve, but not fibrous; and the extremities formed by the division were still distinguished by swellings, rounded in form of ganglions. The same appearance had taken place, with respect to the nerves of the left side; though the divided extremities seemed to have been full two inches apart; the uniting substance was more bloody than that of the other side. This experiment was made, to prove that the original power of action in the thoracic and abdominal viscera was independent of the nerves. As I found the nerves regenerated, a circumstance never hitherto observed, it occurred to me, that it might be objected to the reasoning, that the two first nerves were doing their office, before the two last were divided: to obviate this objection, I made the following experiment.

EXPERIMENT III.

February 19th, I divided, at one operation, the four nerves composing the first class, in a dog. His eyes became instantly dull and heavy; he tottered as he walked; foamed at the mouth; vomited two or three times; breathed with excessive difficulty; his inspirations were long and deep, his expirations short and sudden, but not attended with the repeated jerks of the abdominal muscles as in the last animal; he barked loud every time he threw out the inspired air from the lungs; the pulse was quicker than before the operation. Next morning about half after eight, I found him apparently dead; but on examining more attentively, found he breathed still, though exceedingly slow; his pulse was gone, and he felt cold; his limbs were stretched out. On placing him near the fire, he began in a few minutes to breathe distinctly, and the heart now and then gave a pulsation; in about four hours, he seemed to have got to the same state the operation first left him in, and barked at every expiration, his pulse beating then fifty in a minute. About four in the afternoon he died, having survived the operation twenty-eight hours. The lungs in the dead body were found loaded with blood, but not so much as to carry them to the bottom in water. The trachea was not inflamed. The nerves of the right side, from which a portion had been cut out, seemed to have undergone little alteration; they were only a little more vascular than usual, and had the rounded swell where they had been divided. The nerves of the left side, which had retracted but little, and had been only divided, had their extremities covered with a plug of coagulable lymph. I suspected that the reason of the first dog's dying so soon, was, that none of the nerves had yet acquired the power of performing their former offices; and that, were the operations performed at a greater distance of time, the animal would recover. With this idea, I was led to repeat my experiments, allowing a greater interval to take place between the first and second.

EXPERIMENT IV.

March 6th, I repeated experiment I. on a large dog. His eye on the right side seemed instantly affected, looked dull and inflamed; he coughed and breathed with some difficulty; the secretions from the salivary glands were much increased; he had tremors; these, however, I attributed partly to fear, as on caressing him they disappeared. He ate and drank very well, and had stools. Most of these symptoms continued but a few days, the eye becoming more clear, and the difficulty of breathing hardly perceptible; he vomited, but only after eating, a circumstance which often takes place in dogs in perfect health, from devouring their food too greedily. Thus he continued for three weeks; the external wound had healed, almost by the first intention; he ate greedily, and had perfectly recovered: I supposed the regenerated nerves might now be performing their offices.

EXPERIMENT V.

March 27th, I repeated experiment II. on the same dog, but did not remove quite so much of the nerves. He was stupid for a minute or two, and gaped for breath; but in a few minutes more these symptoms went off; in a quarter of an hour after he ate some boiled meat, with his usual avidity; all the symptoms of the preceding operation again took place, and in the same order. The vomiting and difficulty of breathing were rather more considerable; he ate and drank notwithstanding, and had stools. The convulsive jerks of the abdominal muscles, which hardly took place in the last experiment, were observed in this, during expiration, but were not constant, as in the first dog. On the 15th of April he was nearly as well as before the operations, only he was leaner, and perhaps weaker, from the confinement, as well as from the operations. I wished to see the state of the nerves; an artery was opened in the groin, and the animal expired in a few seconds. In examining the dead body, the viscera were all, to appearance, sound. The divided nerves of the right side were firmly united; having their extremities covered with a kind of callous substance; the regenerating nerve, like bone in the same situation, converting the whole of the surrounding extravasated blood into its own substance. The nerves of the left side were also perfectly united; but the quantity of extravasated blood having been less, the regenerated nerves were smaller than the original; I observed too, that they did not seem fibrous like original nerves, but the recollection that the callus of bone is dissimilar to the original bone, quieted whatever doubts could arise from this circumstance. The tonsils were considerably inflamed, and this circumstance alone might be sufficient to account for the increased secretion of the saliva, an attendant symptom of most sore throats; though I have also seen an increase of viscid saliva, in the human species, from hypochondriac affections of the digestive powers, and also from the causes of temporary debility. The regeneration of the nerves which took place in the first dog, and which I think fully proved by this experiment, was a circumstance to me, then, unexpected and unthought of.

EXPERIMENT VI.

April 19th, I divided the spinal marrow of a dog, between the last vertebra of the neck and first of the back. The muscles of the trunk of the body, but particularly those of the hind legs, appeared instantly relaxed; the legs continued supple, like those of an animal killed by electricity. The heart, on performing the operation, ceased for a stroke or two, then went on slow and full, and in about a quarter of an hour after, the pulse was 160 in a minute. Respiration was performed by means of the diaphragm only, which acted very strongly for some hours. The operation was performed about a quarter of an hour before twelve at noon; about four in the afternoon the pulse was ninety only in a minute, and the heat of the body exceedingly abated, the diaphragm acting strongly, but irregularly. About seven in the evening, the pulse was not above twenty in a minute, the diaphragm acting strongly, but in repeated jerks. Between twelve at night and one in the morning, the dog was still alive; respiration was very slow, but the diaphragm still acted with considerable force. Early in the morning he was found dead. This operation I performed from the suggestion of Mr. HUNTER: he had observed in the human subject, that when the neck was broke at the lower part, (in which cases the spinal marrow is torn through), the patient lived for some days, breathing by the diaphragm. This experiment showed, that dividing the spinal marrow at this place on the neck, if below the origin of the phrenic nerves, would not, for many hours after, destroy the animal; it was preparatory to the following experiment.

EXPERIMENT VII.

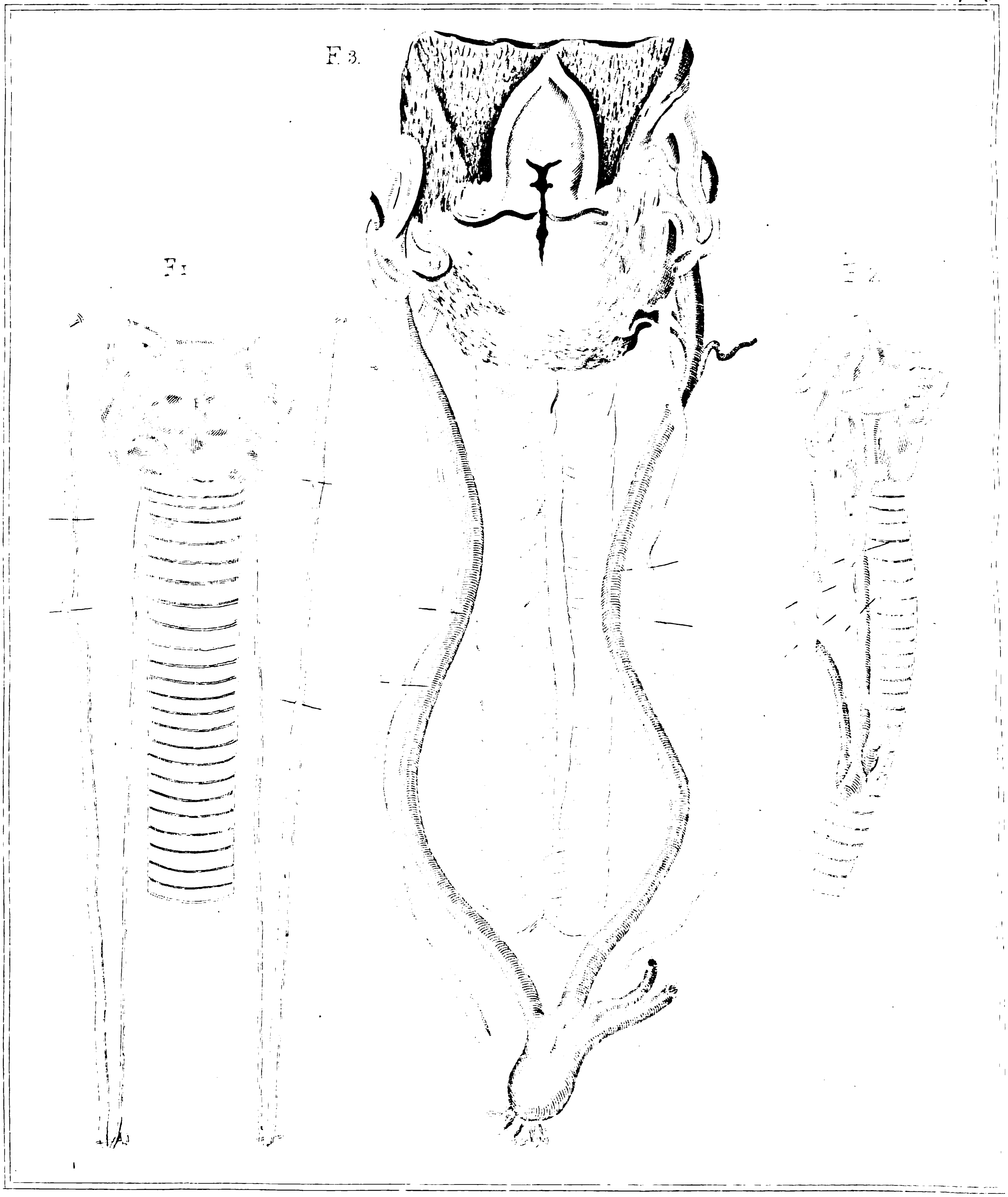
April 26th, I divided all the nerves of the first class, in a dog. The principal symptoms of experiment III. took place. Soon after, I performed on the same animal the operation of experiment VI.; the symptoms peculiar to this operation also took place, whilst those peculiar to experiment III. disappeared. His respirations were five in a minute, and more regular than in experiment III.; the pulse beat 80 in a minute. Five minutes after, I found the pulse 120 in a minute, respiration unaltered; at the end of ten minutes the pulse had again sunk to 80 in a minute, respiration as before. At the end of fifteen minutes, the pulse was again 120, respiration not altered. The operation was performed about two in the afternoon, at Mr. HUNTER's, in Jermyn-street. At three quarters of an hour after five, the respirations were increased to fifteen in a minute; the pulse beating 80 in the same time, and very regularly; the breathing seemed so free, that he had the appearance of a dog asleep. At a quarter before eight, the pulse beat 80, respirations being ten in a minute. At three quarters of an hour after ten, respiration was eight in a minute, the pulse beating 60. The animal heat was exceedingly abated: I applied heat to the chest, he breathed stronger, and raised his head a little, as if awaking from sleep. At half after twelve, Mr. HUNTER saw him; the breathing was strong, and twelve in a minute, the heart beating forty-eight in the same time, slow, but not feeble. He shut his eyelids when they were touched; shut his mouth on its being opened; he raised his head a little, but as he had not the use of the muscles which fix the chest, he did it with a jerk. Mr. HUNTER saw him again between four and five o'clock in the morning; his respirations were then five in a minute, the heart beating exceeding slow and weak. We suppose he died about six in the morning, having survived the operation sixteen hours. This experiment I made from the suggestion of Mr. HUNTER, with a view to obviate the objections raised against the reasoning drawn from the three first experiments. It was urged, that though by these experiments I had deprived the thoracic and abdominal viscera of their ordinary connection with the brain, yet, as the intercostals communicated with all the spinal nerves, some influence might be derived from the brain in this way. This experiment removed also the spinal nerves, and consequently this objection.

As I found, by the two last experiments, that dividing the spinal marrow in the lower part of the neck did not immediately kill, although instant death was universally known to be the consequence of dividing it in the upper part of the neck, I expressed my surprise to Mr. HUNTER, that the spinal marrow should, according to modern theory, be so irritable in the one place, and so much less so in the other.

He told me, that from the time he first observed, that men who had the spinal marrow destroyed in the lower part of the neck lived some days after it, he had established an opinion, that animals, who had the spinal marrow wounded in the upper part of the neck, did not die from the mere wound; but that in dividing it so high, we destroyed all the nerves of the muscles of respiration, and reduced the animal to the state of one hanged; whereas in dividing it lower, we still left the phrenic nerves, and allowed the animal to breathe by his diaphragm. If this opinion be well founded, though dividing the spinal marrow in the lower part of the neck does not kill instantly, whilst the phrenic nerves are untouched; yet if I divide the phrenic nerves first, and then divide the spinal marrow in the lower part of the neck, the consequence, I said, will be the same, as if I had divided it in the upper part.

EXPERIMENT VIII.

By detaching the scapulæ of a dog from the spine, and partly from the ribs, I got at the axillary plexus of nerves, on both sides, from behind. I separated the arteries and veins from the nerves, and passed a ligature under the nerves, close to the spine. I thought I could discern the phrenic nerves, and instantly divided two considerable nerves going off from each plexus. The action of the diaphragm seemed to cease, and the abdominal muscles became fixed, as if they had been arrested in expiration, the belly appearing contracted. His respirations were now about twenty-five in a minute, the pulse beating a hundred and twenty. As I was not willing to trust the experiment to the possibility of having divided only one of the phrenics (which I afterwards found was really the case), and some different nerve instead of the other, after carefully attending to the present symptoms, I divided all the nerves of the axillary plexus, of each side. The ribs were now more elevated in inspiration than before; respirations were increased to forty in a minute; the pulse still beating a hundred and twenty in the same time. Finding that respiration went on very easily without the diaphragm, in about a quarter of an hour after dividing the axillary plexus of each side, I divided the spinal marrow, as in experiment vi. The whole animal took the alarm, all the flexor muscles of the body seemed to contract, and instantly to relax again; he died as suddenly, as if the spinal marrow had been divided in the upper part of the neck. I then opened the chest, and found the heart had ceased its motion; I immediately introduced a large blowpipe into the trachea, below the cricoid cartilage, and inflating the lungs, imitated respiration. The heart began to move again, and in about three minutes was beating seventy in a minute. I recollected that there was still a communication between the brain, and the thoracic and abdominal viscera, that the par vagum and intercostals were entire, and turning to the carotids, divided the nerves. I then went on inflating the lungs as before; the heart, which had stopped, began to move again, beat seventy in a minute, and continued so for near half an hour after the animal had seemingly expired. These appearances were not confined to the neighbourhood of the heart; one of the gentlemen who assisted me, cried out once, that he felt the pulse in the groin. I now ceased to inflate the lungs, and presuming that I could easily reproduce the heart's action, allowed three minutes to elapse. On returning to inflate the lungs, I found the heart had now lost all power of moving; and that irritating the external surface with the point of a knife, did not produce the smallest vibration. I then irritated the phrenic nerves with the point of a knife; the diaphragm contracted strongly as often as the nerves were irritated. I irritated the stomach


F. 3.
F. 1
F. 2

and intestines, which also renewed their peristaltic motions. I then irritated the par vagum and intercostals, about an inch above the lower cervical ganglion of the intercostal; the œsophagus contracted strongly through its whole length, but the heart continued perfectly motionless. On dissection, I found a small branch of a nerve, running down from the second cervical to join the phrenic of the right side, but too insignificant to have any effect on the experiment. This experiment confirms those made by Mr. HUNTER, in which he recovered the animals by inflating the lungs, and on which his method of recovering apparently drowned people principally rests. It shews that respiration is the prime mover of the machine, and it takes off whatever objections might have been raised, from the animals, upon which he made his experiments, having the connection with the brain entire (as the par vagum and intercostals were not divided), since here the same thing took place in these experiments where nerves could have no effect.

If, in the opinion of the judicious, these experiments have a tendency to be useful to mankind, the author will forgive those censures, which unphilosophic severity may throw on him, whilst it views, only, some unavoidable circumstances attending the performance of them.

EXPLANATION OF THE PLATE (Tab. XVI.)

Fig. 1. shows the trachea, par vagum, and intercostals of the subject of experiments I. and II; the transverse bristles show the quantity of nerve lost by excision, and of course the quantity gained by regeneration.

Fig. 2. shows the same parts in the subject of experiment III.

The bristles point out the mode of reunion of the divided nerves by coagulated blood.

Fig. 3. shows both the complete reunion of the nerve after division, and its regeneration after the loss of substance, in the subject of experiments IV. and V.

VII. *An experimental Inquiry concerning the Reproduction of Nerves. By* John Haighton, *M. D. Communicated by* Maxwell Garthshore, *M. D. F. R. S.*

Read February 26, 1795.

An animate machine differs from an inanimate one in nothing more conspicuously, than in its power of repairing its injuries, and of curing its diseases.

It is wisely contrived by nature that, in many instances, the cause producing the injury lays the foundation for the cure; for as injuries, particularly those occasioned by cutting instruments, are necessarily attended with an effusion of blood, from the division of blood-vessels, this fluid, either immediately or remotely, fills up the breach. Hence every part possessed of vascularity, and consequently of blood, carries with it the principle by which it repairs its injuries; and the facility with which this process is conducted, generally bears some proportion to the freedom of the circulation in each individual part.

But it has been a subject of inquiry with anatomists and physiologists, to determine of what nature the new formed part is, and how far it may be said to possess the characters of the original part. There are few who will deny, that a bone, when fractured, fills up the chasm with a substance of its own kind; or that a tendon, when divided, repairs with a substance resembling itself. But this law of nature is not admitted as universal; and this power of repairing in kind has been denied to several of the constituent parts of an animal machine. With respect to the nerves, it has been both affirmed and denied: some assert, that the new formed substance possesses the characters of the primitive nerve; others maintain, that it is totally different; and both found their opinions on experiment.

When opinions so opposite to each other prevail on a point, which experiment seems so fully adequate to decide, we are naturally led to take a view of the manner in which the experiments were conducted, and consider the criterion to which each party appealed.*

There are only two tests which seem to offer themselves, and from which any degree of judgment can be formed. These are, either a minute and careful examination of the new formed substance in an anatomical way, and an accurate comparison of it with the original nerve; or, a cautious attention to the function of that nerve, by which we see the loss of it from the division, and the return of it from the reunion of the divided parts.

Those who have subjected this matter to the test of experiment, have made their appeal to the first criterion; and have either affirmed or denied the reproduction, according as they thought the new formed part either agreed with or differed from the original nerve.

This criterion certainly supposes, that anatomy is fully competent to determine, what is the precise structure of nerves, what are the nature and characters of ultimate nervous fibres, and by what mechanism or power they execute their allotted

* Vide FONTANA, and ARNEMANN.

function. It supposes likewise (and which by the way is not true), that anatomists are perfectly agreed upon this matter; and that those who make their appeal to anatomy, have admitted a common standard of comparison, by which they allow their experiments to be judged; but no position is more remote from fact. It is sufficient to say, that some think ultimate nervous fibres are constructed to act by tremors, whilst others believe them to be hollow tubes. Nor is the difference of opinion less, respecting the appearances which they exhibit on being viewed by a microscope. One eminent physiologist* observes, that the ultimate nervous fibres are "serpentine and convoluted, "very much resembling the winding of the seminal ducts in "the testicle, or epididymis:" but having extended his microscopical observations to other parts, he finds a similar disposition of fibre; nay, even neutral salts, in a state of crystallization, and metals, when microscopically examined, have convoluted fibrous appearances, corresponding with those of nerves. Another ingenious inquirer,† having subjected the nerves to microscopic examination, thought at one time that their fibres were composed of cylinders, with bands twined around them, in a spiral direction; but subsequent examinations convinced him, that this appearance had its origin in an optical deception, and that their true direction was that of "parallel winding fibres.". I have not yet heard whether a third examination has rectified the errors of the two former.

As it appears then, that microscopical observers neither agree with each other on this subject, nor with themselves, I think it fair to conclude, that ocular inspection cannot be admitted as a fair appeal, from which we can determine whether

* Dr. MONRO. † FONTANA.

the substance which unites the extremities of divided nerves is of the same nature as the original nerve.

Dr. ARNEMANN, of Gottingen, who has written *ex professo* on the reproduction of nerves, denies positively, from anatomical examination, that the new formed substance is of the nature of nerve; and on being shown the result of some of my experiments, he declared at the first glance of the eye, "that the medium of union did not possess the characters of "nerve;" and further, "that the true nervous substance is "never reproduced." But he had already prejudged the matter. On the other hand, I am persuaded that if the same preparations had been shown to the Abbé FONTANA, he would have seen in the new formed substance a continuation of the winding parallel fibres, agreeable to the result of his own experiments.

Such a contrariety of opinions determined me to decline an appeal so undecisive, and to submit my inquiries to a test less doubtful and fallacious: and as such a test was not to be found within the pale of anatomy, I resolved to try whether the resources of physiology could not furnish me with what I wished.

From physiology we learn, that *if the action of a nerve be suspended by a division of it, and if that action be recovered in consequence of an union of its divided extremities, such medium of union must possess the characters and properties of nerve.* I had therefore only to determine, what nerves appeared the most favourable for the experiment, and pursue the position just stated to its ultimate consequence. I know not whether my choice was judicious, but I determined on the eighth pair.

The first step I took in this inquiry, was to ascertain *what effects will arise from the division of both of these nerves, together* *with that branch of the great sympathetic nerve accompanying and strongly adhering to them.*

EXPERIMENT.

A dog being properly secured, and a convenient incision made on the fore part of the neck, I divided both the nerves of the eighth pair: he became immediately restless and uneasy, betraying symptoms of great distress upon the stomach, which continued eight hours, when he died.

Though the result of this experiment is perfectly agreeable to what other experimental physiologists have stated, I thought it of importance to the present inquiry, to give it confirmation by further experiment. I therefore repeated it on two other dogs, one of which survived it three days, the other only two.

From these experiments we learn, that the action of these nerves was suspended, and that those vital organs which received their nervous energy from this source, had their functions arrested, so that death followed as a necessary consequence.

It may be said here, by way of objection, that a violent shock had been suddenly given to the machine; and that the animal perished rather from the sudden deprivation of the nervous influence, than from its absolute loss; and that if the same quantity had been abstracted in a more gradual way, the animal might have survived it. How little validity there would be in such an objection, the following experiment will evince.

EXPERIMENT.

Another dog being procured, I divided only one of the nerves of the eighth pair. I was surprised to see how slightly he was affected from it; for, excepting a little moroseness, there was scarcely any alteration perceptible, so that in a few hours after the operation he took food as usual. On the third day, I divided the other nerve; but the same symptoms immediately supervened here as followed the division of both nerves in the former experiments: he continued in a state of restlessness and anxiety, with palpitations and tremors, until the fourth day, when he died.

The event of this experiment differs in nothing from the former, than that the fate of the animal was suspended a little longer, but the ultimate effect was exactly the same: therefore, in the first experiments, *the death of the animal is not to be imputed to the mere sudden deprivation of nervous energy, but to its absolute loss.*

Wishing next to determine whether, by lengthening the interval between the division of the two nerves, a few days more, the life of the animal could not be protracted to a greater length, or even saved, I made another experiment.

EXPERIMENT.

Having divided one of the nerves of the eighth pair, and waited the lapse of nine days, I divided the other. The same symptoms came on now as in the last experiment, but scarcely so violent. The only kind of food he would take was milk, and that in small quantities, and this always produced great uneasiness at the stomach, with symptoms of indigestion. In this state he continued thirteen days, and then died, very much emaciated.

From this dog having lingered so long, I was beginning to entertain hopes of his recovery, and had that eventually happened, I doubt much whether, even under the present uncertainty of things, I could have resisted the temptation of ascribing such recovery to the reproduction of the nerves; but the event put a stop to my speculation.

I think I have now proved my first position, (viz.) that whether the eighth pair of nerves be divided in immediate succession, so as to deprive an animal of their influence suddenly, or whether this deprivation be effected in a more gradual way, the consequences are in the end equally fatal. I must next endeavour to avail myself of this fact in the solution of the problem now before me. If the substance of nerve be reproduced, certainly a period longer than the above must be necessary for this process; but to mark the precise point of time when the line is to be drawn, would require the sacrifice of more animals than a question of mere curiosity could justify. I must, therefore, content myself with giving a general answer to the question, and inquire whether, by suspending the division of the second nerve for a much greater length of time than was done in the two last experiments, the existence of the animal could be preserved.

EXPERIMENT.

Another dog being procured, and one of the nerves of the eighth pair divided, I allowed six weeks to elapse before the other was cut through. This division of the corresponding nerve evidently deranged him; but in a much less degree than in the former experiments. For some days he refused solid food, but took milk; afterwards he ate solid food in small quantities; and near a month had passed away before he fed as usual. The actions of the stomach were for a long time evidently deranged, so that he was continually harassed with symptoms of indigestion; and six months had nearly elapsed before he recovered his health, though during five months of the time he took his usual quantity of food.

Now, to what cause are we to impute his recovery? The most probable one appears to be, that in the interval of six weeks the first nerve had been reproduced; so that the actions of those organs depending upon this nerve, though somewhat disturbed, were not suspended. But as the union of the second nerve advanced, and the reproduction of the first became more perfect, the vital organs gradually recovered their healthy state.

I kept this animal nineteen months, during the greatest part of which time he performed the office of a yard dog. And here it may be proper to observe, that in all the experiments, the voice was totally lost on the division of the second nerve. This effect anatomists will easily understand, from recollecting that the recurrent branches of the eighth pair, which are the true vocal nerves, originate below the part where the trunks of the eighth pair were cut through; consequently those nerves are themselves in effect divided. Now it deserves to be remarked, that his voice returned in proportion as his general health improved; and in about six months he could bark as strongly as before, but the pitch of his voice was evidently raised.

From this experiment, I am strongly inclined to believe that there must have been a true reproduction of the nerve; yet I do not contend, that if the part of union were examined by an anatomical eye, such reproduction would be very evident. On the contrary, I am persuaded that anatomy can determine only the presence and existence of an uniting medium; but it is the province of physiology to decide whether the medium of union possess the characters, and perform the function, of the original nerve.

The evidence of reproduction, as resting on this experiment, may not be sufficient to obviate certain doubts, which reflections upon this subject may probably suggest. There is a difficulty which naturally presents itself here, and this is, the possibility of the stomach and vocal organs having received an additional supply of nervous energy from another source. And to give an appearance of validity to this objection, it may be said that the eighth pair of nerves communicates energy to the larynx by means of the laryngeal branch, and that this branch arises from the trunk above the part where the division was made, and consequently its function received no interruption from the experiment. Again, with regard to the stomach, another apparent objection offers. This organ receives nerves from the great sympathetic, as well as the eighth pair; and nothing hitherto advanced has tended to disprove, that the defect of nervous influence from the division of the latter, has been supplied by greater exertions of the former. Lastly, the familiar analogy of the vascular system, where collateral branches are enlarged from the obliteration of a principal trunk, tends further to give weight to these doubts.

To remove these seeming difficulties by anatomical investigation, or by directing my views to any changes that might be induced on the anastomosing nervous filaments, would be an undertaking not less tedious in its execution than unsatisfactory in its result; for there would still remain room for opposite opinions: and while some would argue that these anastomosing filaments were become evidently enlarged, others would contend that they had not suffered the slightest change.

Now, I have already expressed my distrust of those decisions which are founded on an appeal to the eye, seeing that anatomy has yet to explain by what mechanism or structure these organs perform their office; and because I have frequently heard opposite opinions on my own preparations. I therefore prefer an appeal to the functions of these parts, and inquire whether, in the experiment in which the dog survived the division of the second nerve of the eighth pair after an interval of six weeks, it was effected by the reproduction of the first divided nerve, or in another way?

There are only two possible answers to such a question; these are, that either the functions of the stomach, larynx, &c. were carried on by anastomosing nerves; or that the united nerves had recovered their original importance.

If the first be contended for, this consequence ought to ensue, (viz.) that the eighth pair should now be entirely useless, and both of them may be divided a second time, without injuring any of the functions of the animal.

If the last be granted, it must of necessity follow, that the medium of union possessed the same properties as the original nerve.

I have now circumscribed the field of inquiry, and have drawn the question into so narrow a compass, that it is in the power of a single experiment to prove either the affirmative or negative. If now the eighth pair be divided a second time in immediate succession, and the animal sustain it with impunity, I conceive it right to conclude, that the actions of those organs, which originally were carried on through the means of the eighth pair, are now performed by other channels, and that the true substance of the nerve is not reproduced. But on the contrary, if the animal die in consequence of it, then I think it equally just to infer, that the new formed substance is really and truly *nerve*, because we know of no other substance which can perform the office of nerve.

I shall rely then upon the following, and consider it as my *experimentum crucis.*

EXPERIMENT.

Having the dog in my possession upon which I divided the eighth pair of nerves nineteen months before, I cut through both of them now, in immediate succession. The usual symptoms were immediately induced, and continued until the second day, when he died.

After death I carefully dissected out these nerves, and have preserved them as evidences of my success. I think I have now answered the question I proposed to myself, and can affirm that nerves are not only capable of being united when divided, but that *the new formed substance is really and truly nerve.*

I forbear to make any animadversions on the experiments of those who have formed conclusions contrary to my own: to such I can only say, that I shall always consider myself highly honoured in having the opportunity of showing them the result of my own experiments; and, as far as these will allow me, *to convince by ocular demonstration, though I should fail to persuade by argument.*

FIG. 1.

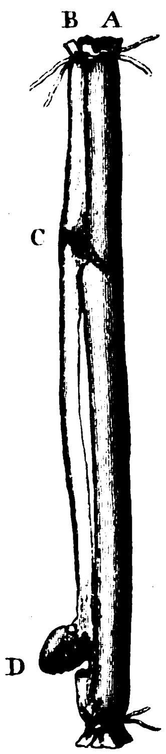
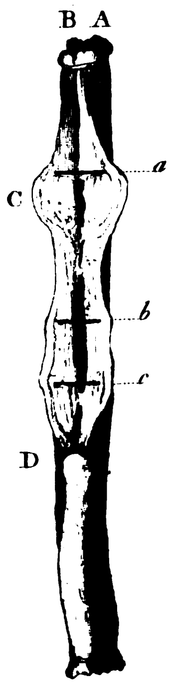


FIG. 2.

FIG. 3.



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EXPLANATION OF THE PLATE (Tab. XVII.)

The three figures are taken from preparations now in the author's possession, being the result of some of the experiments related in the paper.

In each figure the nerve is represented in connection with the carotid artery, to which it naturally adheres by cellular membrane.

Fig. 1st. A, the carotid artery.

B, one of the nerves of the eighth pair.

C, the part where the first division was made, as it appeared after nineteen months.

D, the part where the second division was made, and from which the dog died on the second day.

Fig. 2d. A and B, the carotid artery and nerve of the opposite side.

C, the union which followed the first division, forming a swell like a ganglion.

D, the second division, made two days before death.

Fig. 3d. The same nerve cut open.

a, b, c, represent bristles to keep the cut surfaces asunder.

VIII. *The* Croonian *Lecture on Muscular Motion. By* Everard Home, *Esq. F. R. S.*

Read November 1, 1790.

WHEN I recollect the many learned men who have given this lecture, I cannot but feel myself much flattered by the honour of being named to that office; I feel, at the same time, my own inability to explain many of the phænomena of muscular motion; yet more its principle, the subject to which this lecture was originally confined.

The many, and perhaps insuperable, difficulties which obstruct our progress towards that knowledge, have led the ablest anatomists and physiologists, who have been called upon by this learned Society for their observations upon muscular motion, to deviate from the original intention of the founder, and instead of attempting an investigation of the principle, to explain the anatomical structure, and various phænomena of muscles with which they were acquainted; that by this means they might furnish data for future inquiries.

I shall consider the example of such men as sufficient authority for not confining myself too closely to the subject prescribed; and content myself with giving such facts and observations respecting muscles, as have not, I believe, been already laid before this learned Society.

This lecture was given for several years by Mr. HUNTER, who still continues to prosecute the subject; and should the following observations contain any new materials, it is from that source that many of them are derived: for in my peculiar situation, I should little merit the honourable task assigned to me, were I not to avail myself of every advantage in my power, that could make the present lecture worthy the attention of this learned audience.

The principle of action in an animal, appears to be as extensive as life itself, and is almost the only criterion by which we can distinguish living matter from dead.

This action does not seem to depend so much upon structure, as upon a property connected with life, which is equally extensive in its principle, and so far as we are yet acquainted, equally concealed from the researches of human sagacity.

To acquire a sufficiently enlarged notion of this principle, we must not confine our inquiries to one set of animals, but must take into our view the whole chain of animated beings; and from a review of the different circumstances in which it occurs, and the varied structure of parts upon which it is impressed, we shall have sufficient evidence that the fasciculated fibrous structure commonly met with is not necessary to its existence, but only made use of for its support, and continuance.

The structure which produces muscular action, varies so much in different animals, that we are at a loss to conceive how the effects should have the least similarity; and it is in some cases, only from witnessing the actions that we can consider the parts as muscles; since in nothing else do they bear a resemblance to the muscular structure in the more perfect animals with which we are best acquainted.

We shall illustrate this observation by a description of the structure, and actions, of the animals called hydatids, which appear from their simplicity to be the furthest removed from the human; for as the human is the most complicated, and most perfect in the creation, the hydatid is one of the most simple, and composed of the fewest parts. It is to appearance a membranous bag, the coats of which are so thin as to be semitransparent, and to have no visible muscular structure. From the effects produced by the different parts of this bag while the animal is alive, being exactly similar to the contractions and relaxations of the muscular fibres in the human body, we must conclude that this membrane is possessed of a similar power; and consequently, has the same right to be called muscular.

The hydatid, from its apparent want of muscles, and other parts which generally constitute an animal, was for a long while denied its place in the animal world, and considered as the production of disease; we are, however, at present in possession of a sufficient number of facts, to ascertain, not only that it is an animal, but that it belongs to a genus of which there are several different species.

Hydatids are found to exist in the bodies of many quadrupeds, and often in the human; the particular parts most favourable to their support appear to be the liver, kidneys, and brain, although they are sometimes detected in other situations.

One species is globular in its form, the outer surface of the bag smooth, uniform, and without any external opening; they are seldom found single, and are contained in a cyst, or thick membranous covering, in which they appear to lie quite loose; having no visible attachment to any part of it. This species is most frequently found in the liver and kidneys, both of the quadruped and human subject. They vary in size, but those most commonly met with, are from one quarter of an inch to three quarters of an inch in diameter.

Another species is of an oval form, with a long process, or neck, continued from the smallest end of the oval, at the termination of which, by the assistance of magnifying glasses, is to be seen a kind of mouth; but whether this is intended merely for the purpose of attachment, or to receive nourishment, is not easily determined. This species is found very commonly in the brain of sheep, and brings on a disease called by farmers the staggers. It is not peculiar to any one part of the brain, but is found in very different situations, sometimes in the anterior, at others in the posterior lobe. It is inclosed in a membranous cyst like the globular kind; but differs from that species in one only being contained in the same cyst; and the bag, or body of the animal, being less turgid, appearing to be about half filled with a fluid, in which is a small quantity of white sediment; while the globular ones are in general quite full and turgid.*

This species, from its containing only a small quantity of fluid, has a more extensive power of action on the bag, and is therefore best fitted for illustrating the muscular power of these animals.

If the hydatid be carefully removed from the brain, immediately after the sheep is killed, and put into warm water, it will soon begin to act with the different parts of the body, exhibiting alternate contractions, and relaxations. These it performs to a considerable extent, producing a brisk undulation

* The species of hydatid without a neck is also met with in the brains of sheep, but is less turgid, and less of a spherical figure, than those commonly found in the liver.

of the fluid contained in it; the action is often continued for above half an hour, before the animal dies; and is exactly similar to the action of muscles in the more perfect animals. This species of hydatid, is very well known by the name *tænia hydatigenia*; it varies considerably in its size; one of those which I examined alive, was above five inches long, and nearly three inches broad at the broadest part, which makes it nine inches in the circumference.

The coats of the hydatid, in their recent state, exhibit no appearance of fibres, even when viewed in the microscope; but when dried, and examined by glasses of a high magnifying power, they resemble paper made upon a wire frame. This very minute structure is not met with in membranes in general; it may therefore be considered as the organization upon which their extensive motions depend.

The coats of the different species of hydatids had all of them the same appearance in the microscope.

The intestines, in some of the more delicately constructed animals, have a membranous appearance, similar to the bag of the hydatid, and we cannot doubt of their possessing a muscular power, since there is no other mode of accounting for the food being carried along the canal. The action of the intestines, not coming so immediately under our observation, makes them a less obvious illustration of this principle than the hydatid; we may, however, consider their having a similar structure, as a strong confirmation of it.

If we compare the structure of muscles in the human body, with that of the membranous bag, which composes the *tænia hydatigenia*, a structure evidently endowed with a similar principle of action, the theories of muscular motion, which are founded upon the anatomical structure of a complex muscle, must be overturned.

The simplicity of form, in the muscular structure of this species of hydatid, makes it evident, that the complex organization of other muscles, is not essential to their contraction and relaxation, but superadded for other purposes; which naturally leads us to suppose, that this power of action, in living animal matter, is more simple, and more extensively diffused through the different parts of the body, than has been in general imagined.

From these observations we shall find, that the inquiries hitherto made, into the principle of muscular motion, by investigating the muscles of the more perfect animals, which are most remarkable in their effects, and obviously most deserving of attention, have been too confined.

From our inquiry into the structure of muscles, in different animals, we readily discover, that those above mentioned, although the most perfect in their organization, are at the same time so complicated, for the purpose of adapting them to a variety of secondary uses, that they become of all others, the kind of muscle least fitted for the investigation of the principle itself.

In the present imperfect state of our knowledge respecting animal life and motion, a physiologist, who would select a complex muscle, with the view of discovering, from an examination of its structure, the cause of muscular contraction, would resemble a man, ignorant of mechanics, who should consider a watch as the machine best constructed to assist his inquiries respecting the elastic principle of a spring; which, at first sight, must appear absurd. For although the spring is the power by which the motions are all produced, the machine is so complicated with other important or necessary parts, that the spring itself is not within the reach of accurate observation.

To prosecute an inquiry into the cause of muscular motion, with the greatest probability of success, recourse should be had to muscles, which are in themselves the most simple; and we should endeavour to ascertain what organization, or mechanism, is essential to this action in living animal matter, by which means we should acquire a previous step to the investigation of the principle itself.

The complex muscles in the more perfect animals, from their structure and application, open a wide field of inquiry; for we shall find that it is from their different organizations, that they are enabled to perform the various actions of the body; actions too powerful and extensive for muscles to effect, unaided by such complication of structure, and the advantages derived from it.

In the present lecture, I shall confine myself to the consideration of the most important uses of the complex structure of muscles, and by this means make it evident, that they are not indebted to it for the principle upon which muscular motion depends.

These complications are necessary to supply the muscle with nourishment, for the continuance of its action; to give it strength; to enable it to vary its contraction from the standard or ordinary quantity; and to increase the effect beyond the absolute contraction of the muscle. How these different purposes are effected, I shall endeavour to explain.

A muscle receives its nourishment from the blood, with which we find it more abundantly supplied than most other parts of the body. This supply is evidently intended for the support of its action, since it is proportioned to the exertions of the muscle; and whenever a muscle is rendered incapable of acting, which frequently happens from the joints becoming stiff, the quantity of blood sent to it is very much diminished. The great vascularity of a muscle is, therefore, for the purpose of repairing the waste in the muscular fibres, occasioned by their action; and without this support, the continuance of their contractions would be of short duration.

The strength of a muscle must depend upon the number of its fibres, and most probably upon their size; since in strong muscles the fibrous appearance is very obvious, while in very weak ones no such structure is visible to the eye. A distinction of fibres has been considered as essential to the contraction of a muscle, and only those parts have been allowed to possess that power, in which fasciculi of fibres could be ascertained. But from the observations which have been made, it would perhaps be nearer the truth, to consider the circumstance of the fibres being distinct, as a proof of strength in a muscle, but not essential to the existence of muscular contraction.

There is a power inherent in a complex muscle, by which it can increase or diminish the ordinary extent of its contraction; this is very curious, and must arise from some change going on in the muscle itself, for which it is adapted by means of this very complicated organization.

The usual quantity of contraction which takes place in the fibres of a complex muscle, in the different motions of the human body, is adapted in the nicest manner to the circumstances in which the muscle is placed; and the quantity of contraction appears to be limited by the fibres having no power of becoming shorter. We find, however, from observation, that when the extent of motion in a joint, or the distance between the fixed points of the muscle, is accidentally altered, the muscle acquires a power of adapting its quantity of contraction to the new circumstances which have taken place.

This power in a muscle may be considered as a proof that the principle of contraction is independent of its particular organization; since it can undergo a complete change within itself, so that its fibres shall be shortened to one half of their original length, and still have the same contractile power as when in its original state.

The extent of this principle is well illustrated by the following case. A negro about thirty years of age, having had his arm broken above the elbow joint, the two portions of the os humeri were unfortunately not reduced into their places, but remained in the state they were left by the accident, till the callus or bony union had taken place; so that when the man recovered, the injured bone, from the position of the fractured parts, was reduced almost one half of its length. By this circumstance, the biceps flexor cubiti muscle, which bends the fore-arm, remained so much longer than the distance between its origin and insertion, that in the most contracted state it could scarcely bring itself into a straight line: this muscle, however, in time, as the arm recovered strength, adapted itself to the change of circumstances, by becoming as much shorter as the bone was diminished in length; and by acquiring a new contraction in this shortened state, it was enabled to bend the fore-arm.

Some years after this accident, the person died, and the circumstances abovementioned being known, the parts were examined with particular attention. The biceps muscles of both arms were carefully dissected out, and being measured, the one was found to be eleven inches long, the other only five; so that the muscle of the fractured arm had lost six inches, which is more than the half of its original length. These muscles are now deposited in Mr. HUNTER's collection of preparations illustrating the animal œconomy.

That muscles possessed this power, has been taken notice of by Mr. HUNTER in a former lecture; but the instance which I have given, is so striking an illustration of this principle, that I could not avoid mentioning it while upon this subject.

Muscular contraction is an operation, in whatever way performed, by which the vital stores of the animal are considerably exhausted; this is evident from the quantity of blood with which muscles whose action is frequent are supplied.

This expence would appear, from observation, to be occasioned rather by the extent of contraction, than by its frequency, or force; for if we examine the mechanism of an animal body, we shall find a variety of structures evidently intended for no other purpose than diminishing, as much as possible, the necessary extent of contraction in muscular fibres, while there is no such prevention of frequency of action.

Muscles in general are applied to the bones in such way as to act with great mechanical disadvantages as to power; but this is more than compensated by the small quantity of contraction which is required; and in the muscles of respiration, we find frequency of action is preferred to an increased quantity of muscular contraction.

The velocity of motion thus acquired, although a considerable advantage, does not seem to have been the principal object intended by such structure, but rather to procure the effect by means of short contractions, which are less fatiguing, or in some other way more in the management of the constitution, than long ones.

That long contractions in a muscle cannot be supported for any length of time, may be illustrated from the actions both of the voluntary and involuntary muscles.

While the voluntary muscles are under the command of the will, we cannot ascertain what would be the effects produced by the continuance of their contractions, since the influence of the brain communicated by the nerves becomes soon weakened, and puts a stop to their action; but when the contractions of voluntary muscles are by any circumstance rendered involuntary, the difference in the time of their continuance appears to be in the inverse proportion of the quantity of contraction; for muscles, whose usual functions consist in short contractions, can go on for a long time, while those which are performed by long contractions soon cease.

In the muscles of a paralytic arm, their action, to a certain extent, is continued for years (the times of sleeping excepted), without any effect being produced upon the constitution, or the parts themselves; but in epileptic fits, in which the actions are equally involuntary, only requiring longer contractions, they soon cease, leaving the person greatly exhausted; an effect which must arise from the quantity, not the frequency, of the contractions.

If we attend to the actions of the involuntary muscles, we find that they are continued through life, but that the quantity of contraction is very small; and if from any circumstance the quantity should be increased, it cannot be continued, the parts being unable to sustain it for any length of time. The diaphragm, and intercostal muscles, act constantly in performing the functions of respiration, but they do not exert themselves to their full extent. In laughing, which is likewise an involuntary action, the contractions of these muscles are more extensive, therefore if continued beyond a very short period become so distressing, that a cessation necessarily ensues.

Muscular contraction is never made use of in an animal body, where any other means can produce the same effect, and for this reason elastic ligaments are frequently substituted for muscles; even where muscles are employed, various means are applied to diminish the quantity of contraction.

It is curious, in tracing the different forms of muscles, and in considering the uses for which they are employed, to observe how variously the fibres are disposed, evidently for the purpose of obviating the necessity of great contractions; and the quantity of muscular action saved by this mechanism is greater, in proportion to the frequency and importance of the effect the muscle is intended to produce: this appears to be invariably the case.

Muscles only occasionally called into action, have their fibres nearly straight, which gives no mechanical advantage; the sartorius is an instance of this kind.

Muscles frequently used are more complicated, as those of the fingers are half penniform in their structure; the muscle for raising the heel in walking is penniform; that which raises the shoulder, complex penniform; and those of the ribs, cruciform.

That the two sets of intercostal muscles act at the same time, I proved by experiment in the year 1776. I removed a portion of the external intercostal muscles from the chest of a dog, and in that way saw very distinctly the two sets of muscles in action. The fibres of both sets contracted exactly at the same time.

The particular structures of these different forms of muscles, and the mechanical advantages arising out of them, have been already explained in former lectures upon this subject; but there is a form of muscle, in which the disposition of fibres produces a considerable saving of muscular contraction, that has not been at all taken notice of.

The muscle I allude to is the heart, the most important in the body, whether we consider the frequency of action, or the office in which that action is employed; and we shall find, upon examination, that the fibres are disposed differently from those of any other muscle, which disposition of fibres appears to have a superiority, in being enabled to produce their effect by a smaller quantity of contraction.

In considering the muscular structure of the heart, it is only intended to examine that part of it called the ventricles, which may be reckoned two separate muscles. The right ventricle, for sending the blood through the vessels of the lungs, called the lesser circulation; the left, to propel it through the branches of the aorta, which go to every part of the body, called the greater circulation.

If these two ventricles are superficially examined, the muscular partition by which they are united seems to belong equally to both, one half of it appearing to be a portion of the right, the other of the left ventricle.

In this view, the sides of the left ventricle, although evidently more muscular and thicker than those of the right, are by no means stronger in proportion to the difference of effects they have to produce. We find, however, upon dissection, that the septum is almost wholly a portion of the left ventricle, which gives it a great superiority over the other, and makes it capable of performing the important office of supplying the body with blood.

The left ventricle of the heart, detached from the other parts, is an oviform hollow muscle; but more pointed at its apex than the small end of a common egg. It is made up of two distinct sets of fibres, laid upon one another in the form of strata; those which compose the outer set have their origin round the root of the aorta, and in a spiral manner surrounding the ventricle to its apex, or point, where they terminate, after having made a close half turn. The fibres of the inner set, or stratum, are similar to those of the outer, in their origin, in the mode of surrounding the cavity, and in their termination, but their direction is exactly the reverse; they decussate the outer set in their whole course, and where the two sets terminate, they are both blended into one mass. There is an advantage gained by this disposition of fibres over every other in the body, which adapts the ventricle so perfectly to its office, that it would almost appear impossible to construct it in any other way, so as to answer the purposes for which it is intended.

In this muscle, the fibres, by their spiral direction, are nearly one fourth part longer than the distance between the origin and insertion; and the action of the two sets being in different directions, renders only one half the quantity of contraction in each fibre necessary, that would have been otherwise required; while the turn both sets make in opposite directions at the apex of the ventricle, fixes it and prevents lateral motion.

In the action of the ventricle, two different effects are produced; the first brings the apex nearer to the basis, by which means the *vis inertiæ* of the blood will be overcome where the resistance is least, and a direction given to its motion in the course of the aorta; the second brings the sides nearer each other, which will accelerate the motion of the blood already begun; and the spiral direction of the fibres, will render the power which is applied, more uniform through the whole of that action, than it could have been made by any other known form of muscle; the spiral action will also readily shut the valvulæ mitrales, while the apex is drawn up, which could only be effected by this particular construction.

By this beautiful mechanism, which I have endeavoured to describe, the muscular fibres of the left ventricle of the heart perform their office with a smaller quantity of contraction, compared to their length (although in themselves proportionally longer), than those of any other muscle in the body, and consequently produce a greater effect in a shorter time.

The right ventricle is situated upon the outside of the left, with which it is firmly united; it is not oviform in its shape, but triangular; nor is it uniform in its structure, being made up of two portions, whose fibres have a very different distribution.

The portion of this ventricle which makes a part of the septum of the heart, consists of only one set of fibres, similar in their direction to those of the stratum underneath, belonging to the left ventricle; but from being considerably shorter, they are more oblique than the spiral; and at the edge of the cavity they are blended with the fibres of the opposite portion.

That portion which is opposite to the septum is composed of three sets of fibres; those of the external set are nearly longitudinal; the two others, which lie under it, decussate each other, and are obliquely transverse in their direction, one passing a little upwards, the other downwards; and both terminate upon the edge of the septum.

In the structure of this muscle we find none of the mechanical advantages, so obvious in the left ventricle; the want of these, however, is in some measure compensated by its situation; for the blood contained in its cavity, will have the *vis inertiæ* overcome, and a direction given to its course by the action of the apex of the left ventricle: that motion only requiring to be continued, and accelerated, for which purpose the structure of this muscle is very well calculated; and in which it will also be assisted by the lateral swell of the septum into its cavity, in the contraction of the left ventricle.

In the course of this lecture, it has been my endeavour to show the most simple structure that is capable of muscular action; and to point out the advantages intended to be produced by the different complications which occur in an animal body.

The view which I have taken of this subject gives us an idea of the extent to which muscular action is employed in different animals; and leads to the belief, that very dissimilar structures in the more perfect animals are endowed with this principle, since the actions of the smaller arteries, as well as of the absorbent vessels, must be referred to it.

To ascertain whether any such action could be demonstrated in the membranes of the quadruped, I made the following experiments.

These experiments were made upon the internal membrane of the urinary bladder of a dog, which, in consequence of the animal dying a violent death, was in a very contracted state; the whole of its contents having been expelled in the act of dying.

The method I have adopted to ascertain the muscular power of this membrane, is similar to that taken by Mr. HUNTER in his very ingenious investigation of the structure of blood-vessels, which was laid before this Society; the same mode being equally applicable to the present subject.*

The bladder was carefully laid open, and a portion of its internal membrane, which was corrugated into folds, was dissected off. This portion was spread out, so as to be completely unfolded; it was then laid upon a piece of plate glass wetted, to prevent, as much as possible, any friction; its exact length, in this contracted state, was three quarters of an inch; it was now stretched out, and found to be $1\frac{3}{8}$ inch, upon being left to itself, it contracted so as to be only 1 inch, so that in this state it had gained $\frac{2}{8}$ of an inch, which must have been lost by some action in the living body, and entirely independent of its elasticity. This portion of membrane then had two powers of contraction, one which was muscular, and equal to $\frac{2}{8}$ of an inch, the other elastic, and equal to $\frac{3}{8}$ of an inch.

Another portion of the same membrane, $\frac{1}{2}$ an inch long and

* Mr. HUNTER's experiments on the arteries of the horse are published in his treatise on the Blood, Inflammation, and Gun-shot Wounds.

$\frac{5}{8}$ broad, was treated in the same way, and its muscular contraction was found to be $\frac{1}{8}$ of an inch, that from elasticity $\frac{4}{8}$ of an inch.

A third portion of membrane $\frac{7}{8}$ of an inch long, and $\frac{2}{8}$ broad, was ascertained to have contracted $\frac{2}{8}$ of an inch by its muscular power, and $\frac{3}{8}$ from its elasticity.

It will scarcely be necessary to mention, that the muscular contraction in this membranous structure, is very readily overcome, since this must be almost self-evident; that circumstance, however, must be particularly attended to in making similar experiments.

The internal membrane of the urethra we know to be capable of contracting, as spasmodic strictures are formed in that canal. This membrane, when dried and examined in the microscope, has not the same appearance as the coats of the hydatid; but the whole is a congeries of vessels forming a network. We must, therefore, suppose that the action is in these very minute vessels.

From these experiments and observations, membranous structures are found to exert an action hitherto denied them; and it is equally evident, that this principle is applied to the purposes of the animal œconomy in a more extensive manner than has been generally imagined.

To explain even the most obvious phænomena of muscular motion, must appear from the above observations to be attended with difficulty; how arduous then the task of investigating the principle upon which that motion depends; a principle as extensive as life itself, with which it is coeval, and indeed the only criterion we have of its existence.

An endeavour to throw light upon that principle, has not been the object of the present lecture; I have only attempted to state some circumstances respecting the mechanism employed in producing muscular motion, leaving to others the prosecution of this most intricate and difficult inquiry.

ERRATA.

Page 42, line last, *for* C, *read c.*

Page 115, line last but one, *instead of* B. ad H. *perhaps it should have been* B. ad C.

METEOROLOGICAL JOURNAL,

KEPT AT THE APARTMENTS

OF THE

ROYAL SOCIETY,

BY ORDER OF THE

PRESIDENT AND COUNCIL.

METEOROLOGICAL JOURNAL

for January, 1794.

1794	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hy-gro-me-ter.	Rain.	Winds.		Weather.
		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
Jan. 1	29	8	0	30	49	29,82	73		NE	1	Fair.
	36	2	0	36	51,5	29,87	64		ENE	1	Fine.
2	29	8	0	29,5	48,5	30,00	71		ENE	1	Fine.
	35	2	0	35	50	30,10	71		ENE	1	Cloudy.
3	30	8	0	30	47,5	30,35	70		NE	1	Fine.
	36	2	0	36	51	30,41	69		E	1	Cloudy.
4	29	8	0	29	47	30,44	70		SE	1	Cloudy.
	32	2	0	32	49	30,39	70		SE	1	Cloudy.
5	32	8	0	32	47	30,24	69		E	1	Cloudy.
	34	2	0	34	48	30,20	69		E	1	Cloudy.
6	31,5	8	0	32	46	30,12	70		E	1	Cloudy.
	34	2	0	34	49	30,07	65		E	1	Cloudy.
7	31	8	0	31	46	30,18	64		NE	1	Cloudy.
	37	2	0	37	49	30,24	65		NE	1	Cloudy.
8	30	8	0	33,5	46,5	30,40	73		ESE	1	Cloudy.
	35	2	0	35	51	30,39	74		ESE	1	Fair.
9	25	8	0	26	46	30,37	80		SW	1	Foggy.
	28	2	0	27,5	48	30,34	79		SSE	1	Foggy.
10	22	8	0	22,5	44	30,30	76		SSE	1	Cloudy.
	30,5	2	0	30,5	48	30,27	77		SSE	1	Foggy.
11	23,5	8	0	32	43	30,07	84		N	1	Cloudy.
	37	2	0	36	46,5	29,90	84		E	1	Cloudy.
12	31	8	0	32	43,5	29,78	82		SSE	1	Cloudy.
	34	2	0	34	47	29,81	75		S	1	Cloudy.
13	30,5	8	0	31	43,5	29,98	78		WSW	1	Cloudy.
	33,5	2	0	33,5	48	30,01	82		NW	1	Cloudy.
14	30,5	8	0	32,5	44	30,05	84		W	1	Fair.
	43,5	2	0	43,5	49	30,05	78		SW	1	Cloudy.
15	37,5	8	0	37,5	46	30,07	73		SSW	1	Cloudy.
	41,5	2	0	41,5	51	30,10	71		SSW	1	Cloudy.
16	31,5	8	0	31,5	47	30,30	79		W	1	Fair.
	40,5	2	0	40,5	50	30,35	74		NE	1	Fine.

METEOROLOGICAL JOURNAL

for January, 1794.

1794	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
Jan. 17	32	8	0	36,5	47	30,33	79		W	1	Cloudy.
	43	2	0	42,5	51	30,38	76		NW	1	Fair.
18	32	8	0	32,5	47	30,42	79		SW	1	Cloudy.
	43	2	0	43	51	30,40	83		SW	1	Cloudy.
19	37	8	0	37	49	30,51	86		W	1	Foggy.
	40	2	0	40	51	30,55	85		SW	1	Cloudy.
20	40	8	0	40	49	30,53	80		WSW	1	Cloudy.
	46	2	0	46	54	30,54	71		WNW	1	Fine.
21	35,5	8	0	38	50	30,54	83		W	1	Foggy.
	47	2	0	47	53	30,56	77		W	1	Cloudy.
22	42,5	8	0	43	51,5	30,53	83	0,048	W	1	Foggy.
	45	2	0	45	53	30,45	67		WSW	1	Cloudy.
23	41	8	0	43	51	30,03	70		SW	2	Cloudy.
	47	2	0	47	53	29,91	72		SW	2	Cloudy.
24	35	8	0	35	51	29,88	69		WSW	2	Fair.
	42	2	0	42	52	29,72	66		SW	2	Hazy.
25	36	8	0	36	50	29,71	70	0,105	SW	2	Fair.
	40	2	0	34	53	29,55	67		WNW	2	Sleet.
26	26,5	8	0	28	48	29,22	68		WNW	1	Cloudy.
	32	2	0	32	50	29,32	58		WNW	1	Fine.
27	26,5	8	0	31	47	28,75	86		E	1	Snow.
	31	2	0	31	48	29,10	75		NW	2	Hazy.
28	25,5	8	0	30	46	29,28	76		NW	1	Cloudy.
	34	2	0	34	50	29,46	70		NW	1	Fine.
29	31	8	0	32	46,5	29,52	78		WNW	1	Cloudy.
	37	2	0	36	50	29,58	74		W	1	Hazy.
30	31,5	8	0	32	46	29,68	76		WNW	1	Fair.
	48	2	0	39	49,5	29,82	70		WSW	1	Hazy.
31	48	8	0	48	48	29,76	88	0,250	SW	2	Cloudy.
	51	2	0	50	52	29,78	80		S	2	Cloudy.

METEOROLOGICAL JOURNAL

for February, 1794.

1794	Six's Therm. least and greatest Heat.	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Str.	Weather.
Feb. 1	46	7	0	46	51	29,68	79		SSW	2	Fair.
	50	2	0	49	56	29,61	72		SSW	2	Fine.
2	44	7	0	44	52	29,72	75		SSE	2	Fair.
	49,5	2	0	49,5	55	29,75	65		SSE	2	Cloudy.
3	42	7	0	42	53	29,90	73		S	2	Cloudy.
	49,5	2	0	48	55	30,02	67		SWb.S	1	Fair.
4	35	7	0	36	53	30,14	74		S	1	Fine.
	44	2	0	44	56,5	30,18	67		S b. W	1	Fine.
5	38	7	0	38	53	30,17	74		ESE	1	Cloudy.
	45	2	0	45	56	30,14	67		ESE	1	Cloudy.
6	40	7	0	40	53	29,98	72		E	1	Cloudy.
	43	2	0	43	55	29,93	73		ESE	1	Cloudy.
7	38	7	0	40	53	30,05	78		SSW	1	Cloudy.
	43	2	0	44	55	30,05	78		S	1	Rain.
8	41,5	7	0	42	53	30,18	75	0,076	SW	1	Cloudy.
	49	2	0	49	57	30,27	70		WSW	1	Cloudy.
9	44	7	0	44	54	30,25	78		SW	1	Cloudy.
	51	2	0	51	58	30,24	70		WSW	1	Fair.
10	44	7	0	44	55	30,13	74		WSW	1	Cloudy.
	48	2	0	48	57,5	30,14	58		W	2	Fair.
11	38	7	0	40	54	30,05	71		SW	1	Cloudy.
	47	2	0	47	57	29,83	79		SSW	1	Rain.
12	48	7	0	51	56	29,66	69	0,068	WSW	2	Cloudy.
	51	2	0	50	59	29,81	71		WNW	2	Cloudy.
13	46	7	0	46	56	29,85	75		WSW	1	Cloudy.
	50	2	0	49	58	29,81	68		SW	1	Cloudy.
14	50	7	0	50	58	29,66	73		WSW	1	Cloudy.
	56	2	0	56	60	29,69	70		W	2	Cloudy.
15	50	7	0	51	58	29,52	76		W	2	Cloudy.
	55	2	0	54	62	29,57	59		W	2	Fair.
16	44	7	0	46	58	29,65	75	0,294	E	1	Cloudy.
	51,5	2	0	51	58	29,62	75		SW	1	Cloudy.

METEOROLOGICAL JOURNAL

for February, 1794.

1794	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
Feb. 17	47	7	0	47	58	29,70	80	0,015	SSW	1	Rain.
	49	2	0	48	59	29,69	70		SSW	2	Cloudy.
18	42	7	0	43	57	29,84	72		NE	1	Cloudy.
	44	2	0	44	58	29,94	70		ENE	1	Cloudy.
19	39	7	0	39	56	30,01	70		E	1	Cloudy.
	46,5	2	0	46,5	57	29,99	67		E	1	Cloudy.
20	44	7	0	46	56	29,77	74		S	1	Cloudy.
	52	2	0	52	58	29,66	77		SSW	2	Cloudy.
21	50	7	0	52	57	29,58	78	0,037	SSW	2	Cloudy.
	55	2	0	55	59,5	29,51	75		SSW	2	Cloudy.
22	48	7	0	51	57,5	29,78	79		SSW	2	Cloudy.
	53	2	0	53	60	29,65	75		SSW	2	Cloudy.
23	45	7	0	45	58,5	29,40	74		SSW	2	Fine.
	55	2	0	55	60	29,47	60		SSW	2	Fair.
24	47	7	0	48	58	29,51	75	0,048	SSW	2	Cloudy.
	56	2	0	53	60	29,51	75		SSW	2	Cloudy.
25	49	7	0	51	59	29,68	77	0,062	SW	1	Cloudy.
	55	2	0	54	61	29,65	74		SSW	2	Cloudy.
26	40	7	0	40	57	30,00	68	0,032	W	1	Fair.
	48	2	0	47,5	59,5	30,11	58		WNW	2	Fair.
27	38,5	7	0	40	57	30,29	70		WNW	1	Cloudy.
	49	2	0	49	59	30,24	62		WNW	1	Hazy.
28	43,5	7	0	44	56	29,87	76	0,023	SSW	2	Rain.
	51	2	0	51	59	29,75	64		WNW	2	Fair.

METEOROLOGICAL JOURNAL

for March, 1794.

1794	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
Mar. 1	35	7	0	36	55	29,94	72	0,032	WNW	1	Hazy.
	46,5	2	0	46	58	29,83	67		SW	1	Cloudy.
2	38	7	0	40	56	29,63	74	0,088	WNW	1	Rain.
	47	2	0	47	57	29,73	66		WNW	1	Cloudy.
3	38	7	0	44	55	29,85	73	0,022	S	2	Cloudy.
	49	2	0	49	56,5	29,80	75		S	2	Cloudy.
4	49	7	0	49	56	29,90	56	0,045	S	2	Cloudy.
	54	2	0	53	59	29,96	70		S	2	Fair.
5	47	7	0	48	57	29,90	67		S	1	Hazy.
	50	2	0	50	58,5	30,02	56		S	1	Fair.
6	37	7	0	39	54,5	30,18	66		SW	1	Fair.
	50	2	0	50	58	30,13	65		S	1	Cloudy.
7	41,5	7	0	42	56	30,11	74		S	1	Fine.
	54	2	0	54	60	30,11	57		S	1	Fine.
8	38	7	0	39	57	30,14	70		S	1	Fair.
	53	2	0	53	60	30,09	62		E	1	Fine.
9	42,5	7	0	43	57	30,07	72		NE	1	Cloudy.
	49	2	0	49	58	30,05	68		E	1	Cloudy.
10	40	7	0	41	57	29,94	71		S	1	Fair.
	51	2	0	50	59	29,85	71		SSW	2	Cloudy.
11	49	7	0	49	56	29,51	78	0,168	SSW	2	Fair.
	56	2	0	55	60	29,57	57		SW	2	Fair.
12	40	7	0	41	57	29,67	71		SW	1	Cloudy.
	48	2	0	46	57	29,50	70		SW	2	Rain.
13	36	7	0	36	55	29,69	72	0,256	SW	1	Fine.
	49	2	0	46	58	29,81	66		W	2	Cloudy.
14	40	7	0	42	55	30,08	71		SW	1	Cloudy.
	51	2	0	51	57	30,10	65		SW	1	Cloudy.
15	45	7	0	49	56	30,00	78	0,125	SSW	2	Cloudy.
	52	2	0	52	58	29,95	75		SSW	2	Rain.
16	48	7	0	48	56	29,93	77	0,093	SSW	1	Rain.
	50	2	0	50	57	29,93	70		SSW	1	Cloudy.

METEOROLOGICAL JOURNAL
for March, 1794.

1794	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hy-gro-me-ter.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
Mar. 17	40	7	0	40	56	30,08	74	0,018	W	2	Fair.
	54	2	0	54	58	30,08	59		SW	1	Fine.
18	42	7	0	47	57	29,78	79	0,095	ESE	1	Rain.
	55	2	0	55	59	29,59	70		SSE	2	Cloudy.
19	41	7	0	45	57	29,66	68	0,048	SSW	2	Fair.
	50	2	0	50	58	29,70	63		SSW	2	Fair.
20	38	7	0	40	55	30,05	72		NE	1	Fine.
	49	2	0	49	57	30,19	67		NE	1	Cloudy.
21	36	7	0	40	54	30,44	68		NE	1	Cloudy.
	49	2	0	46	56	30,46	64		E	1	Cloudy.
22	34	7	0	36	54	30,43	69		ENE	2	Fair.
	49	2	0	49	58,5	30,36	64		E	1	Fair.
23	39	7	0	42	55	30,25	74		NE	1	Cloudy.
	54	2	0	54	58	30,21	64		E	1	Fair.
24	43	7	0	44	56	30,25	77		E	1	Cloudy.
	50	2	0	50	58	30,27	70		E	1	Cloudy.
25	41	7	0	43	56	30,30	78		E	1	Cloudy.
	52	2	0	52	59	30,27	73		E	1	Cloudy.
26	40	7	0	43	56	30,21	78		NE	1	Cloudy.
	56	2	0	56	60	30,13	59		ENE	1	Fine.
27	36	7	0	42	55	30,11	75		NE	1	Cloudy.
	50	2	0	50	59	30,11	69		NE	1	Fair.
28	36	7	0	37	55	30,10	73		NE	1	Cloudy.
	54	2	0	54	59,5	30,08	68		NE	1	Fair.
29	43	7	0	47	57	29.87	75		S	1	Cloudy.
	55	2	0	55	60	29.79	66		SSW	1	Cloudy.
30	45	7	0	45	56	29,77	73	0,087	WSW	2	Cloudy.
	53	2	0	53	59	29.83	59		WNW	2	Fair.
31	41	7	0	47	57	29 78	73		SSE	2	Cloudy.
	53	2	0	53	59	29 68	63		SSW	2	Fair.

METEOROLOGICAL JOURNAL

for April, 1794.

1794	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
Apr. 1	38	7	0	38	57	29,68	78	0,108	SW	2	Fair.
	53	2	0	52	59	29,50	53		W	2	Fair.
2	43	7	0	43	57	29,63	69		SSW	1	Fine.
	51	2	0	51	59	29,60	68		SSW	2	Cloudy.
3	47	7	0	47	57	29,51	77	0,015	S	2	Rain.
	55	2	0	55	59	29,46	72		S	2	Cloudy.
4	38	7	0	38	57	29,05	73	0,280	S	2	Rain. Much wind
	54	2	0	52	59	29,32	62		SSW	2	Cloudy. [last
5	41	7	0	45	57	29,72	72	0,101	W	2	Fine. [night.
	54	2	0	54	61	29,76	52		W	2	Fair.
6	45	7	0	47	58	29,52	71	0,135	WSW	2	Cloudy.
	52	2	0	52	60	29,48	65		SW	2	Cloudy.
7	47	7	0	47	57	29,02	74	0,268	SSW	2	Cloudy.
	56	2	0	55	60	28,98	70		SSW	2	Rain.
8	43	7	0	43	58	29,16	69	0,300	SSW	2	Fair.
	52	2	0	51	58	29,19	63		SSW	2	Cloudy.
9	41	7	0	43	56	29,40	73		NE	1	Fair.
	47	2	0	46	57	29,52	68		NE	2	Cloudy.
10	40	7	0	42	56	29,88	72		NE	2	Cloudy.
	51	2	0	50	57	29,98	68		NE	2	Cloudy.
11	42	7	0	43	56	30,18	69		NW	1	Cloudy.
	51	2	0	51	58	30,16	58		SW	1	Cloudy.
12	43	7	0	43	56	30,10	74	0,110	N	1	Rain.
	51	2	0	51	59	30,15	67		NE	1	Cloudy.
13	43	7	0	44	55	30,11	72		NE	2	Cloudy.
	52	2	0	51,5	57	30,03	66		NE	1	Cloudy.
14	43	7	0	47	56	30,04	70		W	1	Cloudy.
	56	2	0	56	58	30,04	57		WSW	1	Cloudy.
15	46	7	0	47	56	29,85	74	0,057	SSW	1	Cloudy.
	58	2	0	58	60	29,96	52		W	1	Cloudy.
16	43	7	0	45	57	30,20	67		SW	1	Fine.
	58	2	0	58	60	30,29	54		SW	1	Fair.

METEOROLOGICAL JOURNAL

for April, 1794.

1794	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hy-gro-me-ter.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
Apr. 17	43	7	0	48	58	30,44	70		SSW	1	Hazy.
	59	2	0	59	60	30,44	59		E	1	Cloudy.
18	43	7	0	46	57	30,34	65		ENE	1	Fine.
	59	2	0	59	61	30,24	63		ENE	1	Hazy.
19	45	7	0	47	58	30,06	73		SW	1	Cloudy.
	62	2	0	62	61	30,04	57		SW	1	Fine.
20	44	7	0	47	59	30,05	70		WSW	1	Fine.
	63	2	0	63	62	30,06	65		WSW	1	Cloudy.
21	49	7	0	52	60	30,13	64		NE	1	Hazy.
	62	2	0	62	62	30,09	65		NE	1	Cloudy.
22	51	7	0	51	60	30,03	69		E	1	Fair.
	66	2	0	66	64	30,00	52		SSE	1	Hazy.
23	53	7	0	57	61	29,91	63		E	1	Cloudy.
	70	2	0	70	64	29,83	54		SE	2	Cloudy.
24	52	7	0	52	62	30,11	63		W	1	Fine.
	65	2	0	65	64	30,20	49		WNW	1	Fine.
25	49	7	0	53	62	30,37	61		SW	1	Fine.
	67	2	0	67	64	30,33	54		S	1	Fine.
26	52	7	0	52	63	30,28	58		SW	1	Fine.
	71	2	0	71	66	30,23	52		SSW	1	Fine.
27	56	7	0	58	64	30,20	63	0,022	E	1	Cloudy.
	73	2	0	71,5	66	30,09	55		S	2	Fair.
28	50	7	0	52	64	30,16	63		WSW	1	Fine.
	64	2	0	62	64	30,05	64		S	2	Cloudy.
29	52	7	0	53	62	30,09	65		SW	2	Cloudy.
	62	2	0	61	63	30,07	63		SW	2	Cloudy.
30	51	7	0	52	62	29,99	60		SSW	1	Cloudy.
	62	2	0	61	62	29,90	55		SSW	2	Fair.

METEOROLOGICAL JOURNAL

for May, 1794.

1794	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hy-gro-me-ter.	Rain.	Winds.		Weather.
		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
May 1	50	7	0	52	61	29,78	65		S	2	Cloudy.
	61	2	0	61	62	29,72	57		SE	1	Cloudy.
2	49	7	0	51	61	29,91	65		E	2	Cloudy.
	57	2	0	57	62	30,07	54		E	1	Fair.
3	41	7	0	47	60	30,18	65		E	1	Hazy.
	61	2	0	61	62	30,07	47		SE	1	Fine.
4	46	7	0	51	60	30,07	62		E	1	Fine.
	62	2	0	62	62	30,01	56		E	1	Fine.
5	51	7	0	52	61	29,85	63		E	1	Cloudy.
	59	2	0	58	62	29,82	60		NE	1	Cloudy.
6	43	7	0	48	60	30,00	62		SSW	2	Cloudy.
	57	2	0	56	60	29,94	60		WSW	2	Cloudy.
7	51	7	0	54	59	29,93	65		SW	2	Cloudy.
	63	2	0	61	61	29,90	60		WSW	2	Cloudy.
8	51	7	0	51	59	29,63	63		SSW	2	Cloudy.
	56	2	0	51	59	29,48	64		WSW	2	Cloudy.
9	40	7	0	43	57	29,43	65	0,301	S	2	Fine.
	52	2	0	47	57	29,44	65		SSW	2	Rain.
10	41	7	0	44	57	29,45	65	0,302	SE	1	Fine.
	57	2	0	51	58	29,45	63		SSE	2	Fair.
11	43	7	0	47	57	29,61	64	0,092	S	2	Fine.
	59	2	0	58	59	29,61	51		SE	2	Hazy.
12	46	7	0	47	57	29,66	64		SSW	2	Hazy.
	61	2	0	61	60	29,74	51		SSW	1	Fair.
13	47	7	0	47	58	29,90	64		NE	1	Cloudy.
	58	2	0	56	58	29.97	61		NW	1	Cloudy.
14	44	7	0	46	57	30,30	68	0,288	E	1	Fine.
	68	2	0	68	60	30,36	52		WSW	1	Fair.
15	53	7	0	55	58,5	30,45	68		SW	1	Cloudy.
	71	2	0	71	61	30,45	57		W	1	Fine.
16	55	7	0	56	60	30,57	68		NE	1	Fair.
	70	2	0	66	62	30,58	53		E	1	Fair.

METEOROLOGICAL JOURNAL

for May, 1794.

1794	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.	
May 17	52	7	0	56	60	30,52	67		SW	1	Cloudy.
	71	2	0	71	63	30,41	56		SSE	1	Fine.
18	54	7	0	57	62	30,25	67		W	1	Hazy.
	63	2	0	62	62	30,19	60		WNW	1	Cloudy.
19	52	7	0	56	62	30,19	68		NE	1	Cloudy.
	62	2	0	62	62	30,22	56		NE	1	Cloudy.
20	49	7	0	52	60	29,96	67		WNW	1	Cloudy.
	58	2	0	58	59	29,89	57		NW	1	Cloudy.
21	42	7	0	47	60	30,01	63	0,113	NW	1	Fine.
	56	2	0	56	60	29,96	56		NW	1	Cloudy.
22	46	7	0	50	58,5	29,82	67		NW	1	Cloudy.
	59	2	0	54	59	29,88	65		N	1	Rain.
23	41	7	0	45	57	30,06	65		WNW	1	Fine.
	61	2	0	61	59	29,97	57		W	1	Cloudy.
24	47	7	0	52	58	29,92	64	0,032	WNW	1	Cloudy.
	57	2	0	55	58	29,91	62		N	1	Cloudy.
25	42	7	0	47	57,5	29,84	65	0,152	NE	1	Cloudy.
	55	2	0	55	57,5	29,85	54		NE	2	Cloudy.
26	41	7	0	46	56,5	29,86	66		NE	1	Cloudy.
	53	2	0	53	57	29,83	57		NE	1	Cloudy.
27	42	7	0	47	56	29,75	84	0,200	NE	1	Rain.
	50	2	0	49	56,5	29,73	89		NE	1	Rain.
28	46	7	0	48	56	29,74	74	0,735	NW	1	Cloudy.
	55	2	0	54	57	29,74	62		NW	1	Cloudy.
29	48	7	0	52	56	29,90	71		NE	1	Cloudy.
	64	2	0	64	58	30,01	59		NE	1	Cloudy.
30	48	7	0	52	57	30,21	65		WNW	1	Hazy.
	64	2	0	60	59	30,24	58		NW	1	Cloudy.
31	51	7	0	53	58	30,30	64		NE	1	Cloudy.
	63	2	0	61	60	30,30	55		NW	1	Fine.

METEOROLOGICAL JOURNAL

for June, 1794.

1794	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
June 1	46	7	0	48	58	30,33	62		NE	1	Cloudy.
	58	2	0	58	59	30,30	57		NE	1	Fair.
2	46	7	0	51	58	30,25	66		NE	1	Cloudy.
	66	2	0	66	60	30,21	51		E	1	Fine.
3	50	7	0	52	59,5	30,18	68		NE	1	Cloudy.
	58	2	0	56	60	30,15	64		NE	1	Cloudy.
4	48	7	0	51	58,5	30,15	68		NE	1	Cloudy.
	60	2	0	60	60	30,15	59		NE	1	Cloudy.
5	48	7	0	51	58	30,16	68		NE	1	Cloudy.
	54	2	0	54	58	30,13	65		NE	1	Cloudy.
6	47	7	0	50	58	30,02	62		NE	1	Cloudy.
	67	2	0	67	61	29,90	51		W	1	Fine.
7	52	7	0	56	60	29,70	64		NE	1	Cloudy.
	60	2	0	59	60	29,71	60		NE	1	Cloudy.
8	47	7	0	51	59	29,92	58		NE	1	Fair.
	64	2	0	61	60	29,91	51		NE	1	Fair.
9	47	7	0	52	60	29,92	61		NW	1	Cloudy.
	63,5	2	0	63,5	60,5	29,99	54		NW	1	Cloudy.
10	55	7	0	57	60	30,00	59		W	1	Fine.
	67	2	0	66	61	29,99	55		WSW	1	Cloudy.
11	49	7	0	53	60	29,95	63		SW	1	Cloudy.
	66	2	0	65	62	29,95	56		SSW	1	Fair.
12	50	7	0	56	61	30,00	64		SW	1	Fair.
	69	2	0	67,5	62,5	30,01	57		SSW	1	Fine.
13	53	7	0	57	62	30,00	64		SE	1	Fine.
	74	2	0	73	63	29,90	57		ESE	1	Fine.
14	60	7	0	64	65	29,88	70	0,337	E	1	Fair.
	71	2	0	70	67	29,97	61		W	1	Cloudy.
15	54	7	0	58	65	30,16	67		NE	1	Fair.
	67	2	0	66	66	30,16	56		ENE	1	Fair.
16	50	7	0	56	55	30,20	60		E	1	Fine.
	66	2	0	66	66	30,18	51		E	1	Fine.

METEOROLOGICAL JOURNAL

for June, 1794.

1794	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hy-gro-me-ter.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
June 17	47	7	0	55	62	30,20	62		E	1	Fair.
	64,5	2	0	64,5	66	30,19	55		E	1	Fine.
18	50	7	0	53	64	30,13	62		E	1	Cloudy.
	71	2	0	70	65,5	30,07	54		E	1	Fine.
19	53	7	0	55	64	29,90	67		NE	1	Cloudy.
	60	2	0	59	63	29,85	65		NE	1	Cloudy.
20	57	7	0	59	64	29,71	65		SW	1	Cloudy.
	69	2	0	68	65	29,69	55		SW	1	Cloudy.
21	52	7	0	57	64	29,85	66		NE	1	Cloudy.
	69	2	0	67	65	29,92	57		NE	1	Cloudy.
22	56	7	0	58	64	30,07	65		E	1	Fine.
	74	2	0	74	66	30,07	53		E	1	Fine.
23	50	7	0	56	65	30,03	65		E	1	Cloudy.
	75	2	0	75	66	29,96	54		E	1	Fine.
24	59	7	0	63	66	29,91	58		SW	1	Hazy.
	79	2	0	79	68	29,84	50		SSW	1	Fine.
25	57	7	0	61	67	29,84	59	0,048	NE	1	Cloudy.
	69	2	0	69	67	29,84	54		NE	1	Cloudy.
26	59	7	0	62	66	29,90	57		NE	1	Fair.
	74	2	0	72	68	29,93	48		NE	1	Fair.
27	54	7	0	60	66	30,21	63		NE	1	Cloudy.
	72	2	0	70	68	30,26	45		WNW	1	Fine.
28	54	7	0	59	67	30,34	59		SE	1	Fine.
	78	2	0	76	69	30,30	47		SSE	1	Fine.
29	54	7	0	61	67	30,29	58		E	1	Fine.
	69	2	0	68,5	69	30,24	51		E	1	Fine.
30	56	7	0	62	68	30,03	62		E	1	Fine.
	75	2	0	75	70	29,96	54		ESE	1	Fair.

METEOROLOGICAL JOURNAL

for July, 1794.

1794	Six's Therm. least and greatest Heat.	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Str.	Weather.
July 1	57	7	0	61	68	29,97	63		WSW	1	Cloudy.
	75	2	0	75	69	30,00	55		NW	1	Fair.
2	57	7	0	61	68	30,15	61		WSW	1	Fair.
	76	2	0	75	70	30,15	51		NW	1	Fine.
3	63	7	0	67	70	30,12	64		SW	1	Cloudy.
	79	2	0	77	71	30,12	55		WNW	1	Cloudy.
4	58	7	0	62	70	30,26	57		NE	1	Fair.
	76	2	0	76	72	30,26	53		E	1	Hazy.
5	59	7	0	62	70	30,22	59		E	1	Fine.
	72	2	0	72	72	30,17	51		E	1	Fine.
6	56	7	0	64	70	30,00	64		E	1	Fine.
	79	2	0	79	73	29,90	54		E	1	Fine.
7	64	7	0	67	72	29,90	63		NE	1	Hazy.
	83	2	0	83	73,5	29,94	54		SSW	1	Fine.
8	58	7	0	61	71	30,12	60		W	1	Fair.
	81	2	0	80	74	30,12	51		SSW	1	Fine.
9	62	7	0	66	73	30,27	59		NE	1	Hazy.
	79	2	0	79	74	30,28	56		E	1	Hazy.
10	59	7	0	64	72	30,37	60		E	1	Cloudy.
	72	2	0	72	72	30,30	56		E	1	Cloudy.
11	58	7	0	64	72	30,15	59		E	1	Fair.
	80	2	0	79	73	30,07	49		NE	1	Fine.
12	58	7	0	64	72	30,04	57		E	1	Fine.
	75	2	0	73	73	30,01	51		E	1	Fine.
13	58	7	0	63	72	29,94	62		E	1	Fine.
	84	2	0	84	73	29,90	50		E	1	Fine.
14	61	7	0	63	73	30,02	61		WNW	1	Fine.
	77	2	0	77	73	30,07	49		WNW	1	Cloudy.
15	60	7	0	63	72	30,16	60		SW	1	Cloudy.
	76	2	0	75	73	30,12	51		SW	1	Cloudy.
16	61	7	0	65	72	30,05	60		SW	1	Cloudy.
	74	2	0	73	72	30,05	59		S	2	Cloudy.

METEOROLOGICAL JOURNAL

for July, 1794.

1794	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
July 17	60	7	0	62	72	30,06	62		SSW	1	Cloudy.
	78	2	0	78	72	30,06	52		S	1	Hazy.
18	65	7	0	66	72	30,06	58		E	1	Cloudy.
	78	2	0	77	73	30,06	54		E	1	Hazy.
19	67	7	0	69	73	30,00	62		SW	1	Hazy.
	81	2	0	80	74,5	29,96	52		NW	1	Fine.
20	61	7	0	64	73	29,95	58		S	1	Fair.
	79,5	2	0	79	73,5	29,90	52		SW	2	Fair.
21	58	7	0	60	68	29,88	56		SW	2	Fair.
	72	2	0	71	72	29,93	46		W	2	Fair.
22	57	7	0	61	70	29,91	54		S	1	Cloudy.
	75	2	0	74	72	29,80	50		SSE	2	Fair.
23	64	7	0	65	71	29,58	59	0,067	SSE	2	Fair.
	72	2	0	70	70	29,54	53		S	2	Fair.
24	59	7	0	60,5	69,5	29,47	64	0,097	SW	2	Cloudy.
	73	2	0	73	71	29,59	55		SW	2	Fair.
25	54	7	0	57	69,5	29,89	57	0,122	W	1	Fine.
	71	2	0	71	70	29,89	56		SW	1	Cloudy.
26	59	7	0	60	68	29,87	62	0,091	SSE	1	Cloudy.
	72	2	0	72	69,5	29,80	58		SSE	2	Cloudy.
27	58	7	0	60	68	29,92	61		SW	1	Cloudy.
	70,5	2	0	70	69	29,92	53		W	1	Cloudy.
28	59	7	0	61	68	29,91	64	0,138	SW	1	Cloudy.
	72,5	2	0	72,5	69	29,89	57		SSW	1	Cloudy.
29	60	7	0	63	68	29,91	64		SSW	1	Cloudy.
	79	2	0	78	70	29,91	57		SSW	1	Fair.
30	65	7	0	67	70	29,91	64		S	1	Fair.
	81,5	2	0	81	72	29,88	54		SW	1	Fair.
31	63	7	0	65	68	29,88	66		SW	1	Cloudy.
	75	2	0	75	72	29,87	55		WSW	1	Fair.

METEOROLOGICAL JOURNAL

for August, 1794.

1794	Six's Therm. least and greatest Heat.	Time. H.	Time. 'M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hy-gro-me-ter.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
Aug. 1	60	7	0	63	70	29,68	58		SSE	2	Cloudy.
	72	2	0	72	71	29,56	59		SSE	2	Fair.
2	59	7	0	62	70	29,51	62	0,030	SSW	2	Cloudy.
	70	2	0	70	71	29,58	58		W	2	Fair.
3	59	7	0	61	68	29,77	56	0,148	WSW	2	Fair.
	66	2	0	66	68	29,76	56		SSW	2	Cloudy.
4	57	7	0	60	67	29,69	59		WNW	2	Cloudy.
	64	2	0	64	68	29,75	51		NW	2	Cloudy.
5	50	7	0	56	66	29,80	58		NW	2	Cloudy.
	68	2	0	66	66,5	29,78	54		WNW	1	Cloudy.
6	55	7	0	58	66	29,63	66	0,063	E	1	Cloudy.
	69	2	0	68	67	29,63	61		SW	1	Cloudy.
7	57	7	0	61	66	29,61	68	0,117	E	1	Cloudy.
	72	2	0	70	67,5	29,56	59		SSE	1	Cloudy.*
8	55	7	0	57	66	29,79	66	0,410	NW	2	Cloudy.
	63	2	0	62	67	29,89	59		N	2	Cloudy.
9	49	7	0	54	65	30,14	61		N	1	Fair.
	65	2	0	65	66	30,16	55		N	1	Cloudy.
10	53	7	0	55	65	30,16	61		W	1	Cloudy.
	67	2	0	64	65	30,15	57		W	1	Cloudy.
11	57	7	0	59	65	30,05	70		SW	1	Cloudy.
	76	2	0	73	67	30,05	60		WSW	1	Fair.
12	57	7	0	59	66	30,19	65		NNE	1	Fair.
	71	2	0	70	67,5	30,24	52		N	1	Fine.
13	53	7	0	55	67	30,28	64		N	1	Fine.
	73	2	0	73	68	30,24	52		E	1	Hazy.
14	54	7	0	57	67	30,16	65		E	1	Hazy.
	74	2	0	74	69	30,08	52		E	1	Fine.
15	57	7	0	59	68	29,96	63		E	1	Hazy.
	75	2	0	73	69	29,89	53		E	1	Fair.
16	59	7	0	61	68	29,86	61	0,041	S	1	Fair.
	73	2	0	71	69	29,86	56		SW	1	Cloudy.

* A violent storm of rain and hail, with thunder & lightning.

METEOROLOGICAL JOURNAL

for August, 1794.

1794	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
Aug. 17	60	7	0	63	68,5	29,89	61		S	1	Fair.
	78	2	0	77	70	29,90	53		S by W	1	Fine.
18	59	7	0	61	69,5	29,98	65		SW	1	Hazy.
	74	2	0	72	70	29,99	51		W	1	Fine.
19	56	7	0	57	68	29,98	65		WSW	1	Hazy.
	74	2	0	72	69,5	29,96	53		SW	1	Hazy.
20	55	7	0	57	68	30,04	64		NW	1	Cloudy.
	72	2	0	71	69	30,05	53		NNE	1	Cloudy.
21	53	7	0	55	67	30,18	63		N	1	Cloudy.
	70	2	0	69	69	30,14	52		NW	1	Cloudy.
22	51	7	0	53	67	30,18	63		W	1	Hazy.
	70	2	0	68	68	30,14	52		NW	1	Fair.
23	58	7	0	60	67,5	30,02	74	0,088	SW	1	Rain.
	72	2	0	72	69	29,97	60		WSW	1	Cloudy.
24	52	7	0	53	67	29,98	62	0,022	SW	1	Fine.
	73	2	0	73	68	29,96	51		SSW	1	Fine.
25	58	7	0	59	67	29,78	63		SSE	2	Cloudy.
	71	2	0	70	68,5	29,75	60		SSW	2	Fair.
26	53	7	0	53	67	29,88	64	0,243	SSW	1	Hazy.
	71	2	0	68	68	29,86	53		SSW	1	Cloudy.
27	54	7	0	54	66	29,71	66	0,225	SW	2	Cloudy.
	63	2	0	61	66	29,79	61		WSW	2	Cloudy.
28	50	7	0	53	65	29,91	64	0,113	WNW	2	Cloudy.
	67	2	0	66	66	29,98	52		NW	2	Cloudy.
29	48	7	0	52	64,5	30,04	64		SW	1	Hazy.
	68	2	0	68	66	29,99	51		S	1	Hazy.
30	57	7	0	59	65	29,84	66	0,015	ESE	1	Cloudy.
	69	2	0	68	68	29,78	62		S	1	Cloudy.
31	58	7	0	60	65	29,67	73	0,090	S	1	Rain.
	67	2	0	66	66	29,67	67		S	1	Fair.

METEOROLOGICAL JOURNAL
for September, 1794.

1794	Six's Therm. least and greatest Heat.	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Str.	Weather.
Sep. 1	53	7	0	56	65	29,92	69	0,063	SSW	1	Cloudy.
	68	2	0	65	66	29,92	59		SSW	1	Cloudy.
2	57	7	0	57	65	29,80	68		W	1	Fine.
	66	2	0	65	66,5	29,87	62		NW	2	Cloudy.
3	54	7	0	56	65	30,11	66		NE	2	Fine.
	66	2	0	65	66	30,18	63		NE	1	Cloudy.
4	48	7	0	50	64	30,18	68		NE	1	Fine.
	65	2	0	65	65	30,13	54		S	1	Fair.
5	56	7	0	57	64	29,90	73	0,120	E	1	Cloudy.
	62	2	0	58	64,5	29,78	65		ESE	1	Rain.
6	53	7	0	54	64	29,57	75	0,442	NE	1	Rain.
	62	2	0	61	64	29,53	66		NE	1	Fair.
7	53	7	0	54	63	29,53	71	0,015	NE	1	Cloudy.
	64	2	0	63	65	29,56	64		NE	1	Cloudy.
8	53	7	0	54	63,5	29,72	82	0,360	NE	1	Rain.
	60	2	0	57	64	29,72	84		NE	1	Rain.
9	55	7	0	56	63,5	29,72	80	0,236	NE	1	Rain.
	61	2	0	58	64	29,77	78		NE	1	Rain.
10	53	7	0	56	62,5	29,88	84	1,020	NE	1	Cloudy.
	61	2	0	61	63	29,98	68		NE	1	Cloudy.
11	48,5	7	0	51	62	30,10	83	0,146	NE	1	Fine.
	61	2	0	60	63	30,14	58		NE	1	Cloudy.
12	49	7	0	53	62	30,23	71		NE	1	Cloudy.
	60	2	0	60	62	30,25	56		NE	1	Cloudy.
13	52	7	0	52	61,5	30,12	59		NE	1	Cloudy.
	57	2	0	57	61,5	30,03	52		NE	1	Cloudy.
14	50	7	0	51	60,5	29,87	66		NE	1	Cloudy.
	57	2	0	57	61	29,85	63		NE	1	Cloudy.
15	48	7	0	50	60	29,82	71		NE	1	Cloudy.
	59	2	0	59	61	29,83	60		NE	1	Cloudy.
16	52	7	0	56	60	29,89	68		SSE	2	Cloudy.
	66	2	0	66	62	29,91	65		SSW	2	Cloudy.

METEOROLOGICAL JOURNAL

for September, 1794.

1794	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
Sep. 17	58	7	0	59	61,5	29,88	75	0,041	S	2	Cloudy.
	67	2	0	67	64	29,82	69		S	2	Fair.
18	59	7	0	56	62	29,51	73	0,041	S	1	Rain.
	63	2	0	63	64	29,51	57		S	2	Fair.
19	50	7	0	53	63	29,66	69		SE	1	Hazy.
	64	2	0	63	64,5	29,59	57		S	2	Fair.
20	53	7	0	52,5	62	29,27	68	0,170	S	2	Cloudy.
	60	2	0	59	63	29,24	61		S	2	Fair.
21	48	7	0	50	61,5	29,64	68	0,042	SW	2	Fine.
	62	2	0	62	62	29,73	65		W	2	Cloudy.
22	53	7	0	56	62	29,79	72		SE	1	Cloudy.
	66,5	2	0	66,5	63	29,76	65		S	2	Cloudy.
23	60	7	0	61	63	29,54	71		SSW	2	Cloudy.
	65	2	0	65	65	29,42	63		SSW	2	Fair.
24	53	7	0	55	63	29,35	73	0,187	E	2	Cloudy.
	57	2	0	56	63	29,37	68		WNW	1	Rain.
25	45	7	0	47	62	29,58	69	0,094	W	1	Cloudy.
	58	2	0	54	62	29,65	65		NW	1	Rain.
26	42	7	0	44	61	29,86	66	0,035	NW	1	Cloudy.
	52	2	0	52	62	29,93	60		N	1	Cloudy.
27	41	7	0	42	59,5	30,06	69		NW	1	Fair.
	61	2	0	60,5	62	30,08	54		NW	1	Fine.
28	37	7	0	39	58	30,20	64		W	1	Fair.
	52	2	0	52	61	30,22	56		NW	1	Fine.
29	44	7	0	46	59	30,32	69		ESE	1	Cloudy.
	56	2	0	56	60	30,33	65		SE	1	Cloudy.
30	48	7	0	48	59	30,36	69		W	1	Cloudy.
	61	2	0	60	61	30,33	62		N	1	Cloudy.

METEOROLOGICAL JOURNAL

for October, 1794.

1794	Six's Therm. least and greatest Heat.	Time. H.	M.	Therm. without. o	Therm. within. o	Barom. Inches.	Hy-gro-me-ter.	Rain. Inches.	Winds. Points.	Str.	Weather.
Oct. 1	50	7	0	51	59,5	30,30	74	0,035	E	1	Cloudy.
	56	2	0	56	59,5	30,26	68		SW	1	Cloudy.
2	52	7	0	54	60	30,18	78	0,046	SW	1	Cloudy.
	57	2	0	56,5	61,5	30,17	69		SW	1	Cloudy.
3	53	7	0	53	60	30,09	76	0,060	E	1	Cloudy.
	58	2	0	58	62,5	30,00	67		S	1	Cloudy.
4	48	7	0	51	60,5	29,82	71	0,026	SE	1	Cloudy.
	56	2	0	55	62	29,79	70		NW	1	Rain.
5	42	7	0	43	59,5	29,89	74	0,054	W	1	Fair.
	56	2	0	52	60	29,74	69		SSW	1	Rain.
6	48	7	0	49	58	29,15	68	0,272	SW	2	Cloudy.
	54	2	0	54	60	29,29	57		WSW	2	Fair.
7	41	7	0	42	57,5	29,66	68		WSW	1	Fine.
	55	2	0	53,5	59,5	29,65	63		WSW	1	Fair.
8	47	7	0	47	58	29,35	70	0,203	W	1	Fair.
	55	2	0	52,5	61	29,45	57		NW	2	Fair.
9	43	7	0	44	57,5	29,76	66	0,053	WNW	1	Fair.
	54,5	2	0	54,5	59,5	29,83	57		NW	1	Cloudy.
10	49	7	0	54	59	29,71	82	0,020	S	2	Cloudy.
	63	2	0	63	62	29,66	63		S	2	Cloudy.
11	57,5	7	0	58	61	29,43	77	0,045	S	2	Cloudy.
	61	2	0	60	63	29,54	60		SSW	2	Fair.
12	46	7	0	47	60	29,90	68	0,038	SW	1	Fine.
	57,5	2	0	57,5	62	29,93	55		SW	1	Fine.
13	39	7	0	40	59,5	29,93	67		NE	1	Fine.
	55	2	0	54,5	62	29,83	60		NE	1	Fine.
14	50	7	0	53	61	29,48	86	0,110	E	1	Rain.
	60,5	2	0	60	63	29,57	63		SSW	2	Fine.
15	54	7	0	54,5	61	29,86	78	0,177	SSW	1	Fair.
	63	2	0	62	65	30,01	60		SSW	1	Fair.
16	54	7	0	54	62	30,16	80		SE	1	Cloudy.
	62	2	0	61,5	63,5	30,14	63		SE	1	Fair.

METEOROLOGICAL JOURNAL

for October, 1794.

1794	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
Oct. 17	52	7	0	53	62	30,07	78		W	1	Cloudy.
	61	2	0	60	64	29,97	63		S	2	Hazy.
18	44	7	0	45	62	29,85	70	0,183	W	2	Fine.
	57	2	0	53	64	29,86	62		W	1	Fair.
19	42	7	0	43	59	30,08	71	0,081	WNW	1	Fine.
	51	2	0	51	61	30,08	67		W	1	Rain.
20	49	7	0	49	60	30,12	78	0,127	E	1	Cloudy.
	56,5	2	0	56,5	62	30,16	73		S	1	Cloudy.
21	48	7	0	48	60,5	30,34	77	0,072	NE	1	Cloudy.
	53	2	0	53	62	30,34	65		NNE	1	Fair.
22	48	7	0	49	60	30,17	76	-	E	1	Fair.
	55	2	0	54	61	30,04	65		ESE	1	Cloudy.
23	44	7	0	44	60	29,76	73	0,627	WNW	1	Cloudy.
	48	2	0	47	60	29,76	68		N	1	Cloudy.
24	44	7	0	45	58	29,79	74	0,220	N	2	Cloudy.
	49	2	0	48	58	29,83	70		N	2	Cloudy.
25	38	7	0	40	57,5	29,91	70		NE	1	Fair.
	50,5	2	0	50,5	60,5	29,91	67		NE	1	Fair.
26	42	7	0	44	57,5	29,79	75		W	1	Cloudy.
	53	2	0	53	59	29,75	74		SW	1	Cloudy.
27	50	7	0	50	58	29,46	80	0,076	SSW	1	Rain.
	50	2	0	50	59	29,29	76		W	1	Rain.
28	40	7	0	40	57	29,35	73	0,261	W	1	Cloudy.
	45	2	0	44	57	29,34	72		N	1	Cloudy.
29	35	7	0	36	56	29,68	73		SW	1	Fine.
	47	2	0	47	58	29,73	61		WNW	1	Fine.
30	39	7	0	44	55	29,41	78	0,056	SW	1	Rain.
	51	2	0	51	58	29,51	66		NW	1	Fair.
31	44	7	0	45	56,5	29,80	74		W	1	Cloudy.
	53	2	0	53	58	29,83	78		SW	1	Rain.

METEOROLOGICAL JOURNAL

for November, 1794.

1794	Six's Therm. least and greatest Heat.	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Str.	Weather.
Nov. 1	50	7	0	50	57	29,92	76	0,026	WSW	1	Fair.
	57	2	0	56	59,5	29,86	64		WSW	1	Cloudy.
2	46	7	0	48	57	29,71	77	0,056	SW	1	Cloudy.
	52	2	0	50	59	29,57	75		SW	1	Rain.
3	44	7	0	45	57,5	29,20	75	0,360	SW	1	Cloudy.
	46	2	0	46	58	29,24	69		WNW	1	Fair.
4	40	7	0	42	56,5	29,29	73		SW	1	Cloudy.
	53	2	0	48	58	29,11	75		SSE	1	Rain.
5	49	7	0	54	58	29,16	74	0,223	S	3	Cloudy.
	56	2	0	53	59	29,18	77		S	2	Rain.
6	47	7	0	49	58	29,47	76	0,385	S	1	Rain.
	49	2	0	49	60	29,50	70		S	1	Rain.
7	40	7	0	41	58	29,52	76	0,555	SSE	1	Fair.
	50	2	0	50	59	29,47	75		SE	1	Rain.
8	44	7	0	45	58	29,58	75	0,235	SSE	2	Cloudy.
	50	2	0	50	58	29,73	68		S	2	Cloudy.
9	35	7	0	35	56,5	29,98	75		WNW	1	Fair.
	46	2	0	45	58	30,05	72		WNW	1	Fair.
10	36	7	0	37	55,5	30,15	75		N	1	Fine.
	52	2	0	50	57	30,06	68		SSE	1	Cloudy.
11	52	7	0	52	56,5	29,90	80		SSW	1	Cloudy.
	56	2	0	56	58	29,95	76		SSW	1	Cloudy.
12	46	7	0	47	57	30,02	74	0,028	WNW	1	Rain.
	49	2	0	49	58	30,00	72		NNW	1	Cloudy.
13	36	7	0	37	56,5	30,02	69		NW	1	Fine.
	44	2	0	44	57,5	30,03	63		NW	1	Fine.
14	36	7	0	40	56	30,17	70		NW	1	Cloudy.
	46	2	0	46	57	30,19	63		NW	1	Cloudy.
15	42	7	0	44	56	30,18	77		NW	1	Foggy.
	51	2	0	51	58	30,03	76		W	1	Cloudy.
16	44	7	0	48	57	29,92	76		WSW	1	Cloudy.
	52	2	0	52	57	29,95	58		WSW	1	Cloudy.

METEOROLOGICAL JOURNAL
for November, 1794.

1794	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hy-gro-me-ter.	Rain.	Winds.		Weather.
		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
Nov. 17	41	7	0	42	57	30,10	70		NE	1	Cloudy.
	43	2	0	43	57	30,10	66		NE	1	Cloudy.
18	34	7	0	34	54	30,05	63		ESE	1	Cloudy.
	37	2	0	37	53	29,97	62		ESE	1	Cloudy.
19	31	7	0	31,5	51,5	29,73	65		E	2	Fair.
	36	2	0	36	52	29,61	60		E	2	Fair.
20	30,5	7	0	31,5	49	29,36	68		E	2	Fair.
	36	2	0	36	50,5	29,20	68		E	2	Cloudy.
21	37	7	0	46	51	29,22	83	0,200	SE	2	Cloudy.
	51	2	0	51	54	29,38	75		SE	2	Cloudy.
22	43	7	0	43	52	29,68	78	0,050	E	1	Cloudy.
	49	2	0	48,5	55	29,73	78		E	1	Fair.
23	44	7	0	44	53	29,77	80		E	1	Cloudy.
	47	2	0	46	56	29,74	78		E	1	Cloudy.
24	43	7	0	48	55	29,58	89	0,396	E	1	Foggy.
	54	2	0	53	57	29,63	88		SW	1	Fair.
25	46	7	0	48	56	29,74	87	0,115	SW	1	Cloudy.
	54	2	0	51	57,5	29,74	79		S	1	Cloudy.
26	44	7	0	44	56,5	29,60	73	0,030	SW	1	Fair.
	49	2	0	48	58	29,76	61		SW	1	Fine.
27	36	7	0	40	55	29,85	75	0,085	SSW	1	Rain.
	53	2	0	53	57	29,92	63		W	1	Fine.
28	44	7	0	44	55	29,82	72		SSW	2	Cloudy.
	48	2	0	48	56	29,48	80		SSE	2	Rain.
29	43	7	0	43	55	29,60	75	0,265	SW	2	Cloudy.
	46	2	0	46	57	29,85	65		SW	2	Fine.
30	44,5	7	0	47	56	29,84	77	0,331	SSW	2	Fair.
	51	2	0	51	58	29,88	74		S	2	Cloudy.

METEOROLOGICAL JOURNAL
for December, 1794.

1794	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hy-gro-me-ter.	Rain.	Winds.		Weather.
		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
Dec. 1	45	8	0	45	57	29,74	76	0,480	SW	1	Cloudy.
	48	2	0	48	58,5	29,69	73		S	1	Cloudy.
2	39	8	0	47	55	29,93	63		S	2	Cloudy.
	51	2	0	51	57,5	29,85	71		S	2	Cloudy.
3	52	8	0	52	58	29,84	73		S	2	Cloudy.
	54	2	0	52,5	59,5	29,80	67		S	2	Cloudy.
4	46	8	0	47	57,5	29,72	77		S	2	Fair.
	50	2	0	50	60	29,70	73		S	2	Fair.
5	40	8	0	43	57,5	29,67	76		E	1	Foggy.
	48	2	0	48	60	29,70	75		E	1	Fine.
6	44	8	0	45	58,5	29,84	76		E	1	Foggy.
	52	2	0	51	61	29,88	75		S	1	Cloudy.
7	44	8	0	46	58	29,83	78		S	1	Cloudy.
	50	2	0	49	60	29,78	76		S	1	Cloudy.
8	46	8	0	47	59	29,55	77	0,045	SE	1	Rain.
	51	2	0	50	60	29,49	73		SE	1	Fair.
9	39	8	0	42	57	29,65	72		SSE	2	Fair.
	50	2	0	49	59	29,55	75		S	2	Hazy.
10	40	8	0	41	58	29,77	76	0,073	SSW	1	Cloudy.
	42	2	0	42	58	29,92	72		SSW	1	Fair.
11	32	8	0	32	56	30,21	73		W	1	Cloudy.
	42	2	0	42	57	30,22	73		SW	1	Hazy.
12	35	8	0	40	55	30,24	74		SW	1	Cloudy.
	46	2	0	45	57,5	30,24	75		SW	1	Cloudy.
13	39,5	8	0	40	55	30,19	73		S	1	Fair.
	43	2	0	42	57	30,16	69		S	1	Fine.
14	39	8	0	40	55	30,13	73		S	1	Cloudy.
	41	2	0	41	57	30,18	76		S	1	Rain.
15	35,5	8	0	36	55	30,19	77	0,140	E	1	Cloudy.
	36,5	2	0	36	56	30,18	76				Foggy.
16	29	8	0	32	53	30,39	76				Foggy.
	37	2	0	38	55,5	30,44	75		E	1	Fair.

METEOROLOGICAL JOURNAL
for December, 1794.

1794	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
Dec. 17	34	8	0	36	54	30,38	77		S	1	Cloudy.
	41,5	2	0	41,5	55	30,21	75		S b. W	1	Fair.
18	31	8	0	31	53	30,18	75		ESE	1	Fine.
	36	2	0	34,5	54	30,11	73		SSE	1	Fine.
19	30	8	0	30,5	52	29,96	72		ESE	1	Fine.
	37	2	0	37	55	29,94	73		ESE	1	Fine.
20	31	8	0	31	50,5	29,96	71		E	1	Fine.
	34,5	2	0	34,5	53	29,98	70		E	1	Fine.
21	27,5	8	0	28,5	50	29,97	72		E	1	Fine.
	34	2	0	34	52	29,95	71		E	1	Hazy.
22	30	8	0	30	49,5	29,76	74		ENE	1	Cloudy.
	33,5	2	0	33,5	51	29,66	73		ENE	1	Cloudy.
23	33	8	0	37	49	29,72	77		E	1	Cloudy.
	40	2	0	40	52	29,72	75		E	1	Cloudy.
24	29	8	0	29	49	29,82	68		ENE	2	Cloudy.
	30	2	0	29	48	29,84	65		NE	2	Cloudy.
25	25,5	8	0	27	46,5	29,68	73		NE	1	Snow.
	30,5	2	0	30	49	29,60	71		NE	1	Cloudy.
26	27,5	8	0	28	46	29,50	76		NE	1	Snow.
	32	2	0	32	48	29,52	77		NE	1	Snow.
27	26,5	8	0	32	46,5	29,80	79		NE	1	Snow.
	36	2	0	36	49	29,88	79		NE	1	Cloudy.
28	34	8	0	34	47	30,12	78	0,283	NE	1	Rain.
	36,5	2	0	36,5	48	30,18	74		NE	1	Cloudy.
29	33,5	8	0	34	47	30,23	72		NE	1	Cloudy.
	37	2	0	36	49	30,24	69		NE	1	Cloudy.
30	32	8	0	32	47,5	30,18	74		WNW	1	Cloudy.
	35	2	0	35	50	30,14	73		WNW	1	Cloudy.
31	26	8	0	26	47	30,08	72		NW	1	Cloudy.
	32	2	0	32	48,5	30,08	75		NE	1	Cloudy.

1794.	Six's Therm. without. Greatest height.	Six's Therm. without. Least height.	Six's Therm. without. Mean height.	Thermometer without. Greatest height.	Thermometer without. Least height.	Thermometer without. Mean height.	Thermometer within. Greatest height.	Thermometer within. Least height.	Thermometer within. Mean height.	Barometer. Greatest height.	Barometer. Least height.	Barometer. Mean height.	Hygrometer. Greatest height.	Hygrometer. Least height.	Hygrometer. Mean height.	Rain.
	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Inches.	Inches.	Inches.	Deg.	Deg.	Deg.	Inches.
January	51	22	35,2	50	22,5	35,6	54	43	48,6	30,56	28,75	30,03	88	58	74,3	0,403
February	56	35	46,7	56	36	47,0	62	51	56,8	30,29	29,40	29,85	80	58	71,8	0,655
March	56	34	46,0	56	36	46,9	60	54	57,1	30,46	29,50	29,98	79	56	69,6	1,077
April	73	38	52,1	71,5	38	52,8	66	55	59,7	30,44	28,98	29,90	77	49	64,4	1,396
May	71	40	53,3	71	43	54,5	63	56	59,2	30,58	29,43	29,96	89	47	62,4	2,215
June	79	46	60,1	79	48	61,5	70	55	63,2	30,34	29,70	30,03	70	45	59	0,385
July	84	54	68,2	84	57	69,4	74,5	68	71,2	30,37	29,47	29,99	66	46	56,9	0,515
August	78	48	62,8	77	52	63,7	71	64,5	67,4	30,28	29,51	29,91	74	51	59,8	1,605
September	68	37	56,1	67	39	56,4	66,5	59	62,6	30,36	29,24	29,85	84	52	66,8	3,012
October	63	35	50,6	63	36	50,8	65	55	60,0	30,34	29,34	29,81	86	55	70,0	2,842
November	57	30,5	45,2	56	31,5	45,7	60	49	56,2	30,19	29,11	29,73	89	58	72,9	3,340
December	54	25,5	38,1	52,5	27	38,7	61	46	53,8	30,44	29,49	29,94	79	63	73,7	1,021
Whole year			51,2			51,9			59,6			29,91			66,8	18,466