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MDCCCII.

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.

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

THE PRESIDENT and COUNCIL of the ROYAL SOCIETY adjudged, for the year 1801, the Medal on Sir GODFREY COPLEY's Donation, to Mr. ASTLEY COOPER, for his Papers On the Effects which take place from the Destruction of the Membrana Tympani of the Ear; with an Account of an Operation for the removal of a particular species of Deafness.

ERRATA.

Page 133, line 2 and 3, *for* 38,3, *read* 383.
—— 134, — penult. *for* hyperoxygenized, *read* oxygenized.

PHILOSOPHICAL

TRANSACTIONS.

I. *The Croonian Lecture. On the Power of the Eye to adjust itself to different Distances, when deprived of the Crystalline Lens. By* Everard Home, *Esq. F. R. S.*

Read November 5, 1801.

It is intended, on the present occasion, to state some facts and observations, in support of an opinion advanced in a former lecture, that the adjustment of the eye to see objects at different distances, does not depend upon any internal changes in the crystalline lens.

The first of the experiments which will be stated, was made with the assistance of the late Mr. Ramsden; and, had not the death of that valuable member of this Society deprived me of his further aid, the following observations would undoubtedly have been more deserving the attention of my learned audience.

It is impossible for me to mention Mr. Ramsden, from whom I have received so much assistance in every pursuit connected with optics and mathematics, in which I have been engaged, without availing myself of this opportunity of paying that tribute of gratitude to his memory, which feelings of delicacy prevented me from offering to him while alive. It is unnecessary here to mention his genius, his merits, or his exertions for the promotion of science; these are equally well known to every member present, as to myself. It is only my individual obligations, in the prosecution of inquiries connected with the objects of this learned Society, that are meant to be taken notice of.

To his friendly and zealous assistance I am indebted for the information which was necessary to enable me to prosecute investigations upon the subject of vision; and, without such assistance, I should have shrunk from the inquiry. It is also to his early friendship, and his readiness to communicate to me his knowledge, that I look back, as among the sources of my early exertions, and love of philosophical pursuits.

In the year 1794, I laid before this learned Society some experiments, suggested and made by Mr. RAMSDEN, upon the comparative powers of adjustment of the eye, when in a perfect state, and when deprived of the crystalline lens. From the result of these experiments it appeared, that the removal of the lens did not deprive the eye of the power of seeing distinctly at different distances. As the person upon whom the experiments were tried did not see very distinctly, without a substitute for the lens, in making them, a double convex glass, of $2\frac{1}{4}$ inches focus, was placed before his eye; and, to render the image distinct, by correcting the spherical aberrations, the aperture was diminished to $\frac{3}{20}$ths of an inch; a less degree of diminution not answering that purpose.

The subject of these experiments was BENJAMIN CLERK, twenty-one years of age; one of his eyes was in a very perfect state, and the other without defect, except what arose from the removal of the lens: and the results appeared to be satisfactory in deciding, that the eye, when deprived of the crystalline lens, retains a power of adjustment.

Opportunities of instituting experiments of this kind very rarely occur; the patients who have had their lenses extracted, either not seeing sufficiently well, or being too much advanced in life to be fit subjects for that purpose; but, in the year 1798, the following case came under my care, which enabled me to make some further observations, in confirmation of the former experiments.

HENRY MILES, a carpenter, at Westborough Green in Sussex, fifty years of age, applied, in the month of August, 1798, at St. George's Hospital, to be admitted as a patient, on account of blindness, from having a cataract in each eye; and was received under my care. Both the cataracts were extracted; and the eyes recovered from the effects of the operation, without suffering from inflammation. The right eye had the power of seeing objects with unusual distinctness; but the left was less perfect, the iris having been slightly torn, by the lens being too big to pass through the aperture, without injuring the membrane.

As soon as this man's eyes had recovered, I requested Mr. RAMSDEN to repeat some of the former experiments, on his right eye; which he readily agreed to do. Before the experiments were made, upon trying what was his power of vision with the naked eye, we were agreeably surprised to find that he saw so distinctly, as to admit of our ascertaining, without the aid of glasses, what were the ranges of his eye's adjustment.

A piece of pasteboard, with a letter of a moderate size, as an object upon it, was put into his hands; as he could not read, the page of a book might have confused him: he was directed to vary the distance of the pasteboard from his eye, till he had ascertained the nearest and most distant situations, in which the object appeared distinct; these distances, by measurement, were 7 inches, and 18 inches. In repeating this experiment several different times, he brought the object very correctly to the same situations.

This result convinced Mr. RAMSDEN, that the eye possessed the power of varying its adjustment; and he did not think any more complex experiments would be nearly so satisfactory; consequently, no others were made, and the man was allowed to go into the country.

It was intended to make him a present of a pair of spectacles, allowing him to choose those best adapted to his eye; but his sight was so very good, that we entirely forgot it, till some time after he was gone.

These experiments confirmed the former ones so very strongly, and from their simplicity were so much less liable to error, that Mr. RAMSDEN and myself considered the object of our inquiry completely attained; the reason for not, at the time, laying them before this learned Society was, that they established no new fact, and the former ones did not appear to require their support.

This inquiry, always regarded as highly important by physiologists, has continued to engage their attention; and, in the BAKERIAN Lecture for last year, Dr. YOUNG has advanced some experiments to prove, that the adjustment of the eye to different distances, depends upon the crystalline lens: he considers the results of the experiments made by Mr. RAMSDEN, upon BENJAMIN CLERK'S eyes, as inconclusive; and the phenomena met with, as arising from the smallness of the aperture, and not from any power of adjustment in the eye. Dr. YOUNG, therefore, with a view to obviate all possibility of deception in future, constructed an optometer, upon the principle of that of Dr. PORTERFIELD. In this instrument, when applied to presbyopic eyes, the eye, by looking along a line through a small convex lens, before which is placed a card with two narrow slits in it, near enough to each other to be within the limits of the pupil, will see the line as two lines, crossing each other at the point of perfect vision; and every eye that has the power of adjustment, will make the lines cross in different places, when adjusted to different distances.

With this instrument, Dr. YOUNG made experiments upon several eyes which had been deprived of the crystalline lens; and with all of them found, that the crossing of the lines was seen only at one point; he therefore concludes, that the power of adjustment was lost.

These experiments of Dr. YOUNG led me to reconsider the subject; and it was matter of regret that BENJAMIN CLERK was not in this country, as making a trial with the optometer on his eye, would have determined, in the most satisfactory manner, whether there had been a fallacy in the former experiments.

This not being in my power, I made inquiry after HENRY MILES, upon whom the second experiments were tried; and I had the pleasure to hear, that he was in good health, and that his eyes continued to have very distinct vision, so much so, that he never had occasion to make use of any glasses, from the time the operation had been performed.

With the view of making some experiments on this man's eyes, with Dr. YOUNG's optometer, I procured that instrument from

Mr. CARY, the optician, made exactly in the same manner as that which had been executed under Dr. YOUNG's direction. I first, however, tried the experiments upon my own eye; but had the mortification to find myself unable to make the lines cross in two different situations. This led me to try the eyes of several of my friends; who were equally unable to make the lines cross any where, except at one point. Young people, indeed all those under thirty years of age, were capable of varying the place of intersection; but none who were above forty, could produce any change in it.

As I could not doubt of my own eye having the power of varying its adjustment, I was led to believe that the instrument required some address in the management, which I had not acquired; and therefore despaired of making HENRY MILES sufficiently master of it, to do justice to my views.

To obviate these difficulties, I adapted the optometer, without the lens, to presbyopic eyes, by making a line 4 feet long, upon strong paper, divided into inches, and having the same slits to look through as in the other. This instrument, and Dr. YOUNG's, I put into the hands of my friend Sir HENRY ENGLEFIELD, with a request that he would examine them, and, when he had become perfectly master of them, and of the best mode of using them, that he would assist me in making experiments with them; for, as he was more in the habit of changing the focus of his eye, in using optical instruments, he would more readily detect the circumstance which prevented me from succeeding in the experiment.

After several trials with this optometer, and seeing its defects, Sir HENRY ENGLEFIELD improved it, by having the paper pasted upon a strong board, 4 feet long, which rendered the surface free from the slightest inequalities; and, instead of a line marked with ink, a thread of black silk was stretched along the middle of the board. With this instrument, he found that his eye could make the lines cross at two different points, at several inches distance from each other. The readiest mode of making the experiment succeed, was first fixing his eye upon some near object, held above and a little on one side of the silk thread, and, when the focus of his eye was adapted to that distance, then to look at the thread; afterwards to look at some distant object, and, when that had become very distinct, again to look at the thread. Upon trying the instrument with my own eye, in this way, I found the crossing of the lines changed its situation, with every change of adjustment; and, after being accustomed to make this experiment, I was enabled to produce a similar change in the optometer with the lens, but by no means in so satisfactory a manner, nor did it last more than an instant; my eye probably not being so well fitted as many others, for experiments of this kind.

The optometer without the lens was hence admitted to be the most easily managed, by the eye of a person unaccustomed to such experiments, and therefore it was determined to make use of it in the trials upon HENRY MILES's eye; which we were enabled to do, as his vision was sufficiently distinct without the aid of glasses, and as, from never having used them, he saw much better with his naked eye.

The following experiments were made with the optometer without the lens, on the 27th of August, 1801.

The first trials were upon Sir HENRY ENGLEFIELD's eye; which, being most familiar with the use of the instrument, became a standard with which the others might be compared.

Sir HENRY ENGLEFIELD's eye made the lines to intersect each other at $12\frac{1}{4}$ inches, as the near distance; and at $28\frac{1}{2}$ inches, as the furthest distance. The experiment was repeated several different times, and the results were very nearly the same.

My own eye made the lines intersect at $12\frac{1}{2}$ inches, as the near distance; and at $29\frac{1}{2}$ inches, as the furthest distance.

A man servant of Sir Henry Englefield's, twenty-five years of age, made the lines intersect at 12 inches, and at $31\frac{1}{2}$ inches.

Henry Miles, fifty years of age, whose eye had been deprived of the crystalline lens for three years, made the lines intersect at $8\frac{3}{10}$ inches, as the near distance; and at $13\frac{3}{10}$, as the furthest distance.

This experiment was repeated two different times in the forenoon, with the same result, and again in the afternoon, without there being any considerable variation; but, upon trying it again, after the eye had been fatigued, he was unable to make the lines cross nearer than $11\frac{2}{8}$ inches, although he could make them cross at $13\frac{3}{10}$ inches; so that adjusting the eye to a near distance, was more difficult after it had been much used, than before.

Henry Miles was unable, in the optometer with the lens, to produce any change in the crossing of the lines, nor did he see them cross with sufficient distinctness to make us consider it a fair experiment.

The following experiment was made upon Miles's eye, at the suggestion of Sir Henry Englefield, with a view to ascertain in another though less decisive way, whether any change took place in it, when directed from a near object to a more distant one.

A piece of pasteboard, in which a black circle, about $\frac{1}{4}$ of an inch in diameter, with a dot in the centre, had been described near to its edge, was placed perpendicularly to the horizon, at 5 inches distance from the eye; another piece of pasteboard, with a circle and dot in it, was placed at the distance of 18 inches; the farthest circle was made a little larger than the other, that it might appear equally distinct at the greater distance. When the eye was directed towards these two objects, they appeared upon the same level; and the circumference of the circles, had they been projected on the same perpendicular plane, would have been nearly in contact.

Miles was placed opposite these objects, with his head made steady, and prevented from moving: he was then told to look at one, till it became very distinct; and, when he had done so, this was removed, and he was directed to look at the other, which did not immediately appear to him with the same distinctness. This was equally the case, whether he looked from the near one to the distant one, or the reverse: the eye did not see the object to which it was so suddenly directed, with the same defined outline as that from which it had been withdrawn.

This man sees best in a strong light; and it was in that light all the experiments were made: he can see very well in any degree of daylight; but his eyes are much fatigued by candle-light. Upon examining the eye attentively, the pupil was rather larger than in perfect eyes; the iris was in a very perfect state; and the cicatrix of the wound, in the inferior part of the cornea, was scarcely visible.

The sight being so good, without the aid of glasses, is not common; and, had not the lenses been extracted in a public hospital, before a number of spectators, some doubts might be entertained whether they had been removed.

From the experiments which have been stated, it appeared to Sir HENRY ENGLEFIELD, that MILES's eye was not deprived of its power of adjustment; and, by whatever circumstances my own judgment might be deceived, or rendered partial, there was nothing by which his could be biassed, as he could have no object in view, but the promotion of science. His knowledge of optics, and his habit of making experiments, are the best pledges of these having been as accurately performed as the nature of the subject admits of; for, certainly, the sources of fallacy, in optical experiments, are numerous. Those that have been related, to be made with perfect accuracy, should be tried upon the eye of a person skilled in optics, and accustomed to such experiments; and whose eye had been deprived of the crystalline lens, without having received the slightest degree of injury in any of its other parts.

The experiments were instituted in the Isle of Wight, which prevented me from requesting several of my friends to be present at them, whose knowledge of the subject would have made me desirous of their assistance.

HALLER mentions the case of a nobleman, from whose eye the crystalline lens had been extracted, who used glasses, and could see with them objects at different distances. As this was an observation made upon a particular friend of his own, and as he refers to PEMBERTON, who mentions a case of depressed crystalline lens, in which no such effect took place, it is natural to suppose, that he had given considerable attention to the subject; and that, although the experiments he instituted are not mentioned, the opinion was not advanced, without what appeared to him sufficient authority.*

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* Et lente ob cataractam extracta vel deposita, oculum tamen ad varias distantias videre, ut coram in nobili viro video, absque ullo experimento, quo eam facultatem recuperaverit. Et si enim tunc, ob diminutas vires, quæ radios uniunt, æger lente vitrea opus habet, eadem lens in omnia distantia sufficit.

HALLER. *Elementa Physiologiæ.* Tom. V. Lib. xvi. §. 25. p. 514.

II. *The Bakerian Lecture. On the Theory of Light and Colours. By* Thomas Young, *M. D. F. R. S. Professor of Natural Philosophy in the Royal Institution.*

Read November 12, 1801.

ALTHOUGH the invention of plausible hypotheses, independent of any connection with experimental observations, can be of very little use in the promotion of natural knowledge; yet the discovery of simple and uniform principles, by which a great number of apparently heterogeneous phenomena are reduced to coherent and universal laws, must ever be allowed to be of considerable importance towards the improvement of the human intellect.

The object of the present dissertation is not so much to propose any opinions which are absolutely new, as to refer some theories, which have been already advanced, to their original inventors, to support them by additional evidence, and to apply them to a great number of diversified facts, which have hitherto been buried in obscurity. Nor is it absolutely necessary in this instance to produce a single new experiment; for of experiments there is already an ample store, which are so much the more unexceptionable, as they must have been conducted without the least partiality for the system by which they will be explained; yet some facts, hitherto unobserved, will be brought forwards, in order to show the perfect agreement of that system with the multifarious phenomena of nature.

The optical observations of Newton are yet unrivalled; and, excepting some casual inaccuracies, they only rise in our estimation, as we compare them with later attempts to improve on them. A further consideration of the colours of thin plates, as they are described in the second book of Newton's optics, has converted that prepossession which I before entertained for the undulatory system of light, into a very strong conviction of its truth and sufficiency; a conviction which has been since most strikingly confirmed, by an analysis of the colours of striated substances. The phenomena of thin plates are indeed so singular, that their general complexion is not without great difficulty reconcileable to any theory, however complicated, that has hitherto been applied to them; and some of the principal circumstances have never been explained by the most gratuitous assumptions; but it will appear, that the minutest particulars of these phenomena, are not only perfectly consistent with the theory which will now be detailed, but that they are all the necessary consequences of that theory, without any auxiliary suppositions; and this by inferences so simple, that they become particular corollaries, which scarcely require a distinct enumeration.

A more extensive examination of Newton's various writings has shown me, that he was in reality the first that suggested such a theory as I shall endeavour to maintain; that his own opinions varied less from this theory than is now almost universally supposed; and that a variety of arguments have been advanced, as if to confute him, which may be found nearly in a similar form in his own works; and this by no less a mathematician than Leonard Euler, whose system of light, as far as it is worthy of notice, either was, or might have been, wholly borrowed from NEWTON, HOOKE, HUYGENS, and MALEBRANCHE.

Those who are attached, as they may be with the greatest justice, to every doctrine which is stamped with the NEWTONIAN approbation, will probably be disposed to bestow on these considerations so much the more of their attention, as they appear to coincide more nearly with NEWTON'S own opinions. For this reason, after having briefly stated each particular position of my theory, I shall collect, from NEWTON'S various writings, such passages as seem to be the most favourable to its admission; and, although I shall quote some papers which may be thought to have been partly retracted at the publication of the optics, yet I shall borrow nothing from them that can be supposed to militate against his maturer judgment.

HYPOTHESIS I.

A luminiferous Ether pervades the Universe, rare and elastic in a high degree.

Passages from NEWTON.

" The hypothesis certainly has a much greater affinity with " his own," that is, Dr. HOOKE'S, " hypothesis, than he seems " to be aware of; the vibrations of the ether being as useful and " necessary in this, as in his." (Phil. Trans. Vol. VII. p. 5087. Abr. Vol. I. p. 145. Nov. 1672.)

" To proceed to the hypothesis: first, it is to be supposed " therein, that there is an ethereal medium, much of the same " constitution with air, but far rarer, subtler, and more strongly " elastic.—It is not to be supposed, that this medium is one " uniform matter, but compounded, partly of the main phlegmatic body of ether, partly of other various ethereal spirits,

" much after the manner that air is compounded of the phleg- " matic body of air, intermixed with various vapours and " exhalations: for the electric and magnetic effluvia, and gravi- " tating principle, seem to argue such variety." (BIRCH: Hist. of R. S. Vol. III. p. 249. Dec. 1675.)

" Is not the heat (of the warm room) conveyed through the " vacuum by the vibrations of a much subtiler medium than air? " —And is not this medium the same with that medium by which " light is refracted and reflected, and by whose vibrations light " communicates heat to bodies, and is put into fits of easy re- " flection, and easy transmission? And do not the vibrations of " this medium in hot bodies, contribute to the intenseness and " duration of their heat? And do not hot bodies communicate " their heat to contiguous cold ones, by the vibrations of this me- " dium propagated from them into the cold ones? And is not this " medium exceedingly more rare and subtile than the air, and " exceedingly more elastic and active? And doth it not readily " pervade all bodies? And is it not, by its elastic force, expanded " through all the heavens?—May not planets and comets, and " all gross bodies, perform their motions in this ethereal me- " dium?—And may not its resistance be so small, as to be " inconsiderable? For instance, if this ether (for so I will call " it) should be supposed 700,000 times more elastic than our " air, and above 700,000 times more rare, its resistance would " be about 600,000000 times less than that of water. And " so small a resistance would scarce make any sensible altera- " tion in the motions of the planets, in ten thousand years. " If any one would ask how a medium can be so rare, let him " tell me—how an electric body can by friction emit an exha- " lation so rare and subtile, and yet so potent?—And how the

" effluvia of a magnet can pass through a plate of glass, without resistance, and yet turn a magnetic needle beyond the " glass?" (Optics, Qu. 18, 22.)

HYPOTHESIS II.

Undulations are excited in this Ether whenever a Body becomes luminous.

Scholium. I use the word undulation, in preference to vibration, because vibration is generally understood as implying a motion which is continued alternately backwards and forwards, by a combination of the momentum of the body with an accelerating force, and which is naturally more or less permanent; but an undulation is supposed to consist in a vibratory motion, transmitted successively through different parts of a medium, without any tendency in each particle to continue its motion, except in consequence of the transmission of succeeding undulations, from a distinct vibrating body; as, in the air, the vibrations of a chord produce the undulations constituting sound.

Passages from Newton.

" Were I to assume an hypothesis, it should be this, if pro-
" pounded more generally, so as not to determine what light is,
" further than that it is something or other capable of exciting
" vibrations in the ether; for thus it will become so general and
" comprehensive of other hypotheses, as to leave little room for
" new ones to be invented." (Birch. Vol. III. p. 249. Dec. 1675.)

" In the second place, it is to be supposed, that the ether is a
" vibrating medium like air, only the vibrations far more swift
" and minute; those of air, made by a man's ordinary voice,
" succeeding one another at more than half a foot, or a foot

" distance; but those of ether at a less distance than the hundred thousandth part of an inch. And, as in air the vibrations are some larger than others, but yet all equally swift, " (for in a ring of bells the sound of every tone is heard at two " or three miles distance, in the same order that the bells are " struck,) so, I suppose, the ethereal vibrations differ in bigness, but not in swiftness. Now, these vibrations, beside their " use in reflection and refraction, may be supposed the chief " means by which the parts of fermenting or putrifying substances, fluid liquors, or melted, burning, or other hot bodies, " continue in motion." (Birch Vol. III. p. 251. Dec. 1675.)

" When a ray of light falls upon the surface of any pellucid " body, and is there refracted or reflected, may not waves of " vibrations, or tremors, be thereby excited in the refracting or " reflecting medium?—And are not these vibrations propagated " from the point of incidence to great distances? And do they " not overtake the rays of light, and by overtaking them successively, do not they put them into the fits of easy reflection " and easy transmission described above?" (Optics. Qu. 17.)

" Light is in fits of easy reflection and easy transmission, " before its incidence on transparent bodies. And probably it is " put into such fits at its first emission from luminous bodies, " and continues in them during all its progress." (Optics. Second Book. Part III. Prop. 13.)

HYPOTHESIS III.

The Sensation of different Colours depends on the different frequency of Vibrations, excited by Light in the Retina.

Passages from NEWTON.

"The objector's hypothesis, as to the fundamental part of it, "is not against me. That fundamental supposition is, that the "parts of bodies, when briskly agitated, do excite vibrations in "the ether, which are propagated every way from those bodies "in straight lines, and cause a sensation of light by beating "and dashing against the bottom of the eye, something after "the manner that vibrations in the air cause a sensation of "sound by beating against the organs of hearing. Now, the "most free and natural application of this hypothesis to the "solution of phenomena, I take to be this: that the agitated "parts of bodies, according to their several sizes, figures, and "motions, do excite vibrations in the ether of various depths "or bignesses, which, being promiscuously propagated through "that medium to our eyes, effect in us a sensation of light of a "white colour; but if by any means those of unequal bignesses "be separated from one another, the largest beget a sensation "of a red colour, the least or shortest of a deep violet, and "the intermediate ones of intermediate colours; much after "the manner that bodies, according to their several sizes, "shapes, and motions, excite vibrations in the air of various "bignesses, which, according to those bignesses, make several "tones in sound: that the largest vibrations are best able to "overcome the resistance of a refracting superficies, and so "break through it with least refraction; whence the vibrations

" of several bignesses, that is, the rays of several colours, which " are blended together in light, must be parted from one an-" other by refraction, and so cause the phenomena of prisms, " and other refracting substances; and that it depends on the " thickness of a thin transparent plate or bubble, whether a " vibration shall be reflected at its further superficies, or trans-" mitted; so that, according to the number of vibrations, inter-" ceding the two superficies, they may be reflected or transmitted " for many successive thicknesses. And, since the vibrations " which make blue and violet, are supposed shorter than those " which make red and yellow, they must be reflected at a less " thickness of the plate: which is sufficient to explicate all the " ordinary phenomena of those plates or bubbles, and also of " all natural bodies, whose parts are like so many fragments of " such plates. These seem to be the most plain, genuine, and " necessary conditions of this hypothesis. And they agree so " justly with my theory, that if the animadversor think fit to " apply them, he need not, on that account, apprehend a divorce " from it. But yet, how he will defend it from other difficulties, " I know not." (Phil. Trans. Vol. VII. p. 5088. Abr. Vol. I. p. 145. Nov. 1672.)

" To explain colours, I suppose, that as bodies of various " sizes, densities, or sensations, do by percussion or other " action excite sounds of various tones, and consequently vi-" brations in the air of different bigness; so the rays of light, " by impinging on the stiff refracting superficies, excite vibra-" tions in the ether,—of various bigness; the biggest, strongest, " or most potent rays, the largest vibrations; and others shorter, " according to their bigness, strength, or power: and therefore " the ends of the capillamenta of the optic nerve, which pave

" or face the retina, being such refracting superficies, when the " rays impinge upon them, they must there excite these vibra- " tions, which vibrations (like those of sound in a trunk or " trumpet) will run along the aqueous pores or crystalline pith " of the capillamenta, through the optic nerves, into the senso- " rium;—and there, I suppose, affect the sense with various " colours, according to their bigness and mixture; the biggest " with the strongest colours, reds and yellows; the least with " the weakest, blues and violets; the middle with green; and a " confusion of all with white, much after the manner that, in " the sense of hearing, nature makes use of aerial vibrations of " several bignesses, to generate sounds of divers tones; for the " analogy of nature is to be observed." (BIRCH Vol. III. p. 262. Dec. 1675.)

" Considering the lastingness of the motions excited in the " bottom of the eye by light, are they not of a vibrating nature? " —Do not the most refrangible rays excite the shortest vibra- " tions,—the least refrangible the largest? May not the harmony " and discord of colours arise from the proportions of the vibra- " tions propagated through the fibres of the optic nerve into " the brain, as the harmony and discord of sounds arise from " the proportions of the vibrations of the air?" (Optics, Qu. 16, 13, 14.)

Scholium. Since, for the reason here assigned by NEWTON, it is probable that the motion of the retina is rather of a vibratory than of an undulatory nature, the frequency of the vibrations must be dependent on the constitution of this substance. Now, as it is almost impossible to conceive each sensitive point of the retina to contain an infinite number of particles, each capable of vibrating in perfect unison with every possible undulation, it

33537

becomes necessary to suppose the number limited, for instance, to the three principal colours, red, yellow, and blue, of which the undulations are related in magnitude nearly as the numbers 8, 7, and 6; and that each of the particles is capable of being put in motion less or more forcibly, by undulations differing less or more from a perfect unison; for instance, the undulations of green light being nearly in the ratio of 6½, will affect equally the particles in unison with yellow and blue, and produce the same effect as a light composed of those two species: and each sensitive filament of the nerve may consist of three portions, one for each principal colour. Allowing this statement, it appears that any attempt to produce a musical effect from colours, must be unsuccessful, or at least that nothing more than a very simple melody could be imitated by them; for the period, which in fact constitutes the harmony of any concord, being a multiple of the periods of the single undulations, would in this case be wholly without the limits of sympathy of the retina, and would lose its effect; in the same manner as the harmony of a third or a fourth is destroyed, by depressing it to the lowest notes of the audible scale. In hearing, there seems to be no permanent vibration of any part of the organ.

HYPOTHESIS IV.

All material Bodies have an Attraction for the ethereal Medium, by means of which it is accumulated within their Substance, and for a small Distance around them, in a State of greater Density, but not of greater Elasticity.

It has been shewn, that the three former hypotheses, which may be called essential, are literally parts of the more complicated NEWTONIAN system. This fourth hypothesis differs perhaps in some degree from any that have been proposed by former authors, and is diametrically opposite to that of NEWTON; but, both being in themselves equally probable, the opposition is merely accidental; and it is only to be inquired which is the best capable of explaining the phenomena. Other suppositions might perhaps be substituted for this, and therefore I do not consider it as fundamental, yet it appears to be the simplest and best of any that have occurred to me.

PROPOSITION I.

All Impulses are propagated in a homogeneous elastic Medium with an equable Velocity.

Every experiment relative to sound coincides with the observation already quoted from NEWTON, that all undulations are propagated through the air with equal velocity; and this is further confirmed by calculations. (LAGRANGE. Misc. Taur. Vol. I. p. 91. Also, much more concisely, in my Syllabus of a course of Lectures on Natural and Experimental Philosophy, about to be published. Article 289.) If the impulse be so great as materially to disturb the density of the medium, it will be no longer homogeneous; but, as far as concerns our senses, the quantity of motion may be considered as infinitely small. It is surprising that EULER, although aware of the matter of fact, should still have maintained, that the more frequent undulations are more rapidly propagated. (Theor. mus. and Conject. phys.) It is possible, that the actual velocity of the particles of the luminiferous ether may bear a much less proportion to the velocity of the undulations than in sound; for light may be excited by the motion of a body moving at the rate of only one mile in the time that light moves a hundred millions.

Scholium 1. It has been demonstrated, that in different mediums the velocity varies in the subduplicate ratio of the force directly, and of the density inversely. (Misc. Taur. Vol. I. p. 91. YOUNG's Syllabus. Art. 294.)

Scholium 2. It is obvious, from the phenomena of elastic bodies and of sounds, that the undulations may cross each other without interruption. But there is no necessity that the various colours of white light should intermix their undulations; for, supposing the vibrations of the retina to continue but a five hundredth of a second after their excitement, a million undulations of each of a million colours may arrive in distinct succession within this interval of time, and produce the same sensible effect, as if all the colours arrived precisely at the same instant.

PROPOSITION II.

An Undulation conceived to originate from the Vibration of a single Particle, must expand through a homogeneous Medium in a spherical Form, but with different quantities of Motion in different Parts.

For, since every impulse, considered as positive or negative, is propagated with a constant velocity, each part of the undulation must in equal times have past through equal distances from the vibrating point. And, supposing the vibrating particle, in the course of its motion, to proceed forwards to a small distance in a given direction, the principal strength of the undulation will naturally be straight before it; behind it, the motion will be equal, in a contrary direction; and, at right angles to the line of vibration, the undulation will be evanescent.

Now, in order that such an undulation may continue its progress to any considerable distance, there must be in each part of it, a tendency to preserve its own motion in a right line from the centre; for, if the excess of force at any part were communicated to the neighbouring particles, there can be no reason why it should not very soon be equalised throughout, or, in other words, become wholly extinct, since the motions in contrary directions would naturally destroy each other. The origin of sound from the vibration of a chord is evidently of this nature; on the contrary, in a circular wave of water, every part is at the same instant either elevated or depressed. It may be difficult to show mathematically, the mode in which this inequality of force is preserved; but the inference from the matter of fact, appears to be unavoidable; and, while the science of hydrodynamics is so imperfect that we cannot even solve the simple problem of the time required to empty a vessel by a given aperture, it cannot be expected that we should be able to account perfectly for so complicated a series of phenomena, as those of elastic fluids. The theory of HUYGENS indeed explains the circumstance in a manner tolerably satisfactory: he supposes every particle of the medium to propagate a distinct undulation in all directions; and that the general effect is only perceptible where a portion of each undulation conspires in direction at the same instant; and it is easy to show that such a general undulation would in all cases proceed rectilinearly, with proportionate force; but, upon this supposition, it seems to follow, that a greater quantity of force must be lost by the divergence of the partial undulations, than appears to be consistent with the propagation of the effect to any considerable distance. Yet it is obvious, that some such limitation of the motion must naturally be expected to take place; for, if the intensity of the motion of any particular part, instead of continuing to be propagated straight forwards, were supposed to affect the intensity of a neighbouring part of the undulation, an impulse must then have travelled from an internal to an external circle in an oblique direction, in the same time as in the direction of the radius, and consequently with a greater velocity; against the first proposition. In the case of water, the velocity is by no means so rigidly limited as in that of an elastic medium. Yet it is not necessary to suppose, nor is it indeed probable, that there is absolutely not the least lateral communication of the force of the undulation, but that, in highly elastic mediums, this communication is almost insensible. In the air, if a chord be perfectly insulated, so as to propagate exactly such vibrations as have been described, they will in fact be much less forcible than if the chord be placed in the neighbourhood of a sounding board, and probably in some measure because of this lateral communication of motions of an opposite tendency. And the different intensity of different parts of the same circular undulation may be observed, by holding a common tuning fork at arm's length, while sounding, and turning it, from a plane directed to the ear, into a position perpendicular to that plane.

PROPOSITION III.

A Portion of a spherical Undulation, admitted through an Aperture into a quiescent Medium, will proceed to be further propagated rectilinearly in concentric Superficies, terminated laterally by weak and irregular Portions of newly diverging Undulations.

At the instant of admission, the circumference of each of the undulations may be supposed to generate a partial undulation, filling up the nascent angle between the radii and the surface terminating the medium; but no sensible addition will be made to its strength by a divergence of motion from any other parts of the undulation, for want of a coincidence in time, as has already been explained with respect to the various force of a spherical undulation. If indeed the aperture bear but a small proportion to the breadth of an undulation, the newly generated undulation may nearly absorb the whole force of the portion admitted; and this is the case considered by NEWTON in the Principia. But no experiment can be made under these circumstances with light, on account of the minuteness of its undulations, and the interference of inflection; and yet some faint radiations do actually diverge beyond any probable limits of inflection, rendering the margin of the aperture distinctly visible in all directions; these are attributed by NEWTON to some unknown cause, distinct from inflection; (Optics, Third Book, Obs. 5.) and they fully answer the description of this proposition.

Let the concentric lines in Fig. 1. (Plate I.) represent the contemporaneous situation of similar parts of a number of successive undulations diverging from the point A; they will also represent the successive situations of each individual undulation: let the force of each undulation be represented by the breadth of the line, and let the cone of light ABC be admitted through the aperture BC; then the principal undulations will proceed in a rectilinear direction towards GH, and the faint radiations on each side will diverge from B and C as centres, without receiving any additional force from any intermediate point D of the undulation, on account of the inequality of the lines DE and DF. But, if we allow some little lateral divergence from the extremities of the undulations, it must diminish their force, without adding materially to that of the dissipated light; and their termination, instead of the right line BG, will assume the form CH; since the loss of force must be more considerable near to C than at greater distances. This line corresponds with the boundary of the shadow in NEWTON's first observation, Fig. 1; and it is much more probable that such a dissipation of light was the cause of the increase of the shadow in that observation, than that it was owing to the action of the inflecting atmosphere, which must have extended a thirtieth of an inch each way in order to produce it; especially when it is considered that the shadow was not diminished by surrounding the hair with a denser medium than air, which must in all probability have weakened and contracted its inflecting atmosphere. In other circumstances, the lateral divergence might appear to increase, instead of diminishing, the breadth of the beam.

As the subject of this proposition has always been esteemed the most difficult part of the undulatory system, it will be proper to examine here the objections which NEWTON has grounded upon it.

"To me, the fundamental supposition itself seems impossible; "namely, that the waves or vibrations of any fluid can, like the "rays of light, be propagated in straight lines, without a con-"tinual and very extravagant spreading and bending every "way into the quiescent medium, where they are terminated "by it. I mistake, if there be not both experiment and demon-"stration to the contrary." (Phil. Trans. VII. 5089, Abr. I. 146. Nov. 1672.)

"Motus omnis per fluidum propagatus divergit a recto tra-"mite in spatia immota."

"Quoniam medium ibi," in the middle of an undulation admitted, "densius est, quam in spatiis hinc inde, dilatabit sese "tam versus spatia utrinque sita, quam versus pulsuum rariora "intervalla; eoque pacto—pulsus eadem *fere* celeritate sese in "medii partes quiescentes hinc inde relaxare debent;—ideoque "spatium totum occupabunt.—Hoc experimur in sonis." (Princip. Lib. II. Prop. 42.

"Are not all hypotheses erroneous, in which light is supposed "to consist in pression or motion, propagated through a fluid "medium?—If it consisted in pression or motion, propagated "either in an instant, or in time, it would bend into the shadow. "For pression or motion cannot be propagated in a fluid in "right lines beyond an obstacle which stops part of the motion, "but will bend and spread every way into the quiescent medium "which lies beyond the obstacle.—The waves on the surface of "stagnating water, passing by the sides of a broad obstacle "which stops part of them, bend afterwards, and dilate themselves gradually into the quiet water behind the obstacle. "The waves, pulses, or vibrations of the air, wherein sounds "consist, bend manifestly, though not so much as the waves "of water. For a bell or a cannon may be heard beyond a "hill, which intercepts the sight of the sounding body; and "sounds are propagated as readily through crooked pipes as "straight ones. But light is never known to follow crooked "passages, nor to bend into the shadow. For the fixed stars, "by the interposition of any of the planets, cease to be seen. "And so do the parts of the sun, by the interposition of the "moon, Mercury, or Venus. The rays which pass very near "to the edges of any body, are bent a little by the action of the "body;—but this bending is not towards but from the shadow, rectilinear propagation of undulations, NEWTON has made no reply; perhaps because of his own misconception of the nature of the motions of elastic mediums, as dependent on a peculiar law of vibration, which has been corrected by later mathematicians. (Phil. Trans. for 1800, p. 116.) On the whole, it is presumed, that this proposition may be safely admitted, as perfectly consistent with analogy and with experiment.

PROPOSITION IV.

When an Undulation arrives at a Surface which is the Limit of Mediums of different Densities, a partial Reflection takes place, proportionate in Force to the Difference of the Densities.

This may be illustrated, if not demonstrated, by the analogy of elastic bodies of different sizes. "If a smaller elastic body "strikes against a larger one, it is well known that the smaller "is reflected more or less powerfully, according to the diffe-"rence of their magnitudes: thus, there is always a reflection "when the rays of light pass from a rarer to a denser stratum "of ether; and frequently an echo when a sound strikes "against a cloud. A greater body striking a smaller one, pro-"pels it, without losing all its motion: thus, the particles of a "denser stratum of ether, do not impart the whole of their "motion to a rarer, but, in their effort to proceed, they are "recalled by the attraction of the refracting substance with "equal force; and thus a reflection is always secondarily pro-"duced, when the rays of light pass from a denser to a rarer "stratum." (Phil. Trans. for 1800. p. 127.) But it is not absolutely necessary to suppose an attraction in the latter case, since the effort to proceed would be propagated backwards without it, and the undulation would be reversed, a rarefaction

" and is performed only in the passage of the ray by the body, " and at a very small distance from it. So soon as the ray is " past the body, it goes right on." (Optics, Qu. 28.)

Now the proposition quoted from the Principia does not directly contradict this proposition; for it does not assert that such a motion must diverge equally in all directions; neither can it with truth be maintained, that the parts of an elastic medium communicating any motion, must propagate that motion equally in all directions. (Phil. Trans. for 1800. p. 109—112.) All that can be inferred by reasoning is, that the marginal parts of the undulation must be somewhat weakened, and that there must be a faint divergence in every direction; but whether either of these effects might be of sufficient magnitude to be sensible, could not have been inferred from argument, if the affirmative had not been rendered probable by experiment.

As to the analogy with other fluids, the most natural inference from it is this: " The waves of the air, wherein sounds consist, " bend manifestly, though not so much as the waves of water;" water being an inelastic, and air a moderately elastic medium; but ether being most highly elastic, its waves bend very far less than those of the air, and therefore almost imperceptibly. Sounds are propagated through crooked passages, because their sides are capable of reflecting sound, just as light would be propagated through a bent tube, if perfectly polished within.

The light of a star is by far too weak to produce, by its faint divergence, any visible illumination of the margin of a planet eclipsing it; and the interception of the sun's light by the moon, is as foreign to the question, as the statement of inflection is inaccurate.

To the argument adduced by HUYGENS, in favour of the returning in place of a condensation; and this will perhaps be found most consistent with the phenomena.

PROPOSITION V.

When an Undulation is transmitted through a Surface terminating different Mediums, it proceeds in such a Direction, that the Sines of the Angles of Incidence and Refraction are in the constant Ratio of the Velocity of Propagation in the two Mediums.

(BARROW, Lect. Opt. II. p. 4. HUYGENS, *de la Lum.* cap. 3. EULER, *Conj. Phys.* Phil. Trans. for 1800, p. 128. YOUNG'S Syllabus. Art. 382.)

Corollary 1. The same demonstrations prove the equality of the angles of reflection and incidence.

Corollary 2. It appears from experiments on the refraction of condensed air, that the ratio of the difference of the sines varies simply as the density. Hence it follows, by Schol. I. Prop. I. that the excess of the density of the ethereal medium is in the duplicate ratio of the density of the air; each particle cooperating with its neighbours in attracting a greater portion of it.

PROPOSITION VI.

When an Undulation falls on the Surface of a rarer Medium, so obliquely that it cannot be regularly refracted, it is totally reflected, at an Angle equal to that of its Incidence.

(Phil. Trans. for 1800, p. 128.)

Corollary. This phenomenon tends to prove the gradual increase and diminution of density at the surface terminating two mediums, as supposed in HYPOTHESIS IV; although HUYGENS has attempted to explain it somewhat differently.

PROPOSITION VII.

If equidistant Undulations be supposed to pass through a Medium, of which the Parts are susceptible of permanent Vibrations somewhat slower than the Undulations, their Velocity will be somewhat lessened by this vibratory Tendency; and, in the same Medium, the more, as the Undulations are more frequent.

For, as often as the state of the undulation requires a change in the actual motion of the particle which transmits it, that change will be retarded by the propensity of the particle to continue its motion somewhat longer: and this retardation will be more frequent, and more considerable, as the difference between the periods of the undulation and of the natural vibration is greater.

Corollary. It was long an established opinion, that heat consists in vibrations of the particles of bodies, and is capable of being transmitted by undulations through an apparent vacuum. (NEWT. Opt. Qu. 18.) This opinion has been of late very much abandoned. Count RUMFORD, Professor PICTET, and Mr. DAVY, are almost the only authors who have appeared to favour it; but it seems to have been rejected without any good grounds, and will probably very soon recover its popularity.

Let us suppose that these vibrations are less frequent than those of light; all bodies therefore are liable to permanent vibrations slower than those of light; and indeed almost all are liable to luminous vibrations, either when in a state of ignition, or in the circumstances of solar phosphori; but much less easily, and in a much less degree, than to the vibrations of heat. It will follow from these suppositions, that the more frequent luminous undulations will be more retarded than the less frequent; and consequently, that blue light will be more refrangible than red, and radiant heat least of all; a consequence which coincides exactly with the highly interesting experiments of Dr. HERSCHEL. (Phil. Trans. for 1800. p. 284.) It may also be easily conceived, that the actual existence of a state of slower vibration may tend still more to retard the more frequent undulations, and that the refractive power of solid bodies may be sensibly increased by an increase of temperature, as it actually appears to have been in EULER's experiments. (Acad. de Berlin. 1762. p. 328.)

Scholium. If, notwithstanding, this proposition should appear to be insufficiently demonstrated, it must be allowed to be at least equally explanatory of the phenomena with any thing that can be advanced on the other side, from the doctrine of projectiles; since a supposed accelerating force must act in some other proportion than that of the bulk of the particles; and, if we call this an elective attraction, it is only veiling under a chemical term, our incapacity of assigning a mechanical cause. Mr. SHORT, when he found by observation the equality of the velocity of light of all colours, felt the objection so forcibly, that he immediately drew an inference from it in favour of the undulatory system. It is assumed in the proposition, that when light is dispersed by refraction, the corpuscles of the refracting substance are in a state of actual alternate motion, and contribute to its transmission; but it must be confessed, that we cannot at present form a very decided and accurate conception of the forces concerned in maintaining these corpuscular vibrations.

PROPOSITION VIII.

When two Undulations, from different Origins, coincide either perfectly or very nearly in Direction, their joint effect is a Combination of the Motions belonging to each.

Since every particle of the medium is affected by each undulation, wherever the directions coincide, the undulations can proceed no otherwise than by uniting their motions, so that the joint motion may be the sum or difference of the separate motions, accordingly as similar or dissimilar parts of the undulations are coincident.

I have, on a former occasion, insisted at large on the application of this principle to harmonics; (Phil. Trans. for 1800. p. 130.) and it will appear to be of still more extensive utility in explaining the phenomena of colours. The undulations which are now to be compared are those of equal frequency. When the two series coincide exactly in point of time, it is obvious that the united velocity of the particular motions must be greatest, and, in effect at least, double the separate velocities; and also, that it must be smallest, and if the undulations are of equal strength, totally destroyed, when the time of the greatest direct motion belonging to one undulation coincides with that of the greatest retrograde motion of the other. In intermediate states, the joint undulation will be of intermediate strength; but by what laws this intermediate strength must vary, cannot be determined without further data. It is well known that a similar cause produces in sound, that effect which is called a beat; two series of undulations of nearly equal magnitude cooperating and destroying each other alternately, as they coincide more or less perfectly in the times of performing their respective motions.

COROLLARY I. *Of the Colours of striated Surfaces.*

BOYLE appears to have been the first that observed the colours of scratches on polished surfaces. NEWTON has not noticed them. MAZEAS and Mr. BROUGHAM have made some experiments on the subject, yet without deriving any satisfactory conclusion. But all the varieties of these colours are very easily deduced from this proposition.

Let there be in a given plane two reflecting points very near each other, and let the plane be so situated that the reflected image of a luminous object seen in it may appear to coincide with the points; then it is obvious that the length of the incident and reflected ray, taken together, is equal with respect to both points, considering them as capable of reflecting in all directions. Let one of the points be now depressed below the given plane; then the whole path of the light reflected from it, will be lengthened by a line which is to the depression of the point as twice the cosine of incidence to the radius. Fig. 2.

If, therefore, equal undulations of given dimensions be reflected from two points, situated near enough to appear to the eye but as one, wherever this line is equal to half the breadth of a whole undulation, the reflection from the depressed point will so interfere with the reflection from the fixed point, that the progressive motion of the one will coincide with the retrograde motion of the other, and they will both be destroyed; but, when this line is equal to the whole breadth of an undulation, the effect will be doubled; and when to a breadth and a half, again destroyed; and thus for a considerable number of alternations; and, if the reflected undulations be of different kinds, they will be variously affected, according to their proportions to the various length of the line which is the difference between the lengths of their two paths, and which may be denominated the interval of retardation.

In order that the effect may be the more perceptible, a number of pairs of points must be united into two parallel lines; and, if several such pairs of lines be placed near each other, they will facilitate the observation. If one of the lines be made to revolve round the other as an axis, the depression below the given plane will be as the sine of the inclination; and, while the eye and luminous object remain fixed, the difference of the length of the paths will vary as this sine.

The best subjects for the experiment are Mr. COVENTRY'S exquisite micrometers; such of them as consist of parallel lines drawn on glass, at the distance of one five hundredth of an inch, are the most convenient. Each of these lines appears under a microscope to consist of two or more finer lines, exactly parallel, and at the distance of somewhat more than a twentieth of that of the adjacent lines. I placed one of these so as to reflect the sun's light at an angle of 45°, and fixed it in such a manner, that while it revolved round one of the lines as an axis, I could measure its angular motion; and I found, that the brightest red colour occurred at the inclinations $10\frac{1}{4}°$, $20\frac{3}{4}°$, 32°, and 45°; of which the sines are as the numbers 1, 2, 3, and 4. At all other angles also, when the sun's light was reflected from the surface, the colour vanished with the inclination, and was equal at equal inclinations on either side.

This experiment affords a very strong confirmation of the theory. It is impossible to deduce any explanation of it from any hypothesis hitherto advanced; and I believe it would be difficult to invent any other that would account for it. There is a striking analogy between this separation of colours, and the production of a musical note by successive echoes from equidistant iron palisades; which I have found to correspond pretty accurately with the known velocity of sound, and the distances of the surfaces.

It is not improbable that the colours of the integuments of some insects, and of some other natural bodies, exhibiting in different lights the most beautiful versatility, may be found to be of this description, and not to be derived from thin plates. In some cases, a single scratch or furrow may produce similar effects, by the reflection of its opposite edges.

COROLLARY II. *Of the Colours of thin Plates.*

When a beam of light falls on two parallel refracting surfaces, the partial reflections coincide perfectly in direction; and, in this case, the interval of retardation, taken between the surfaces, is to their distance as twice the cosine of the angle of refraction to the radius. For, in Fig. 3, drawing AB and CD perpendicular to the rays, the times of passing through BC and AD will be equal, and DE will be half the interval of retardation; but DE is to CE as the sine of DCE to the radius. Hence, that DE may be constant, or that the same colour may be reflected, the thickness CE must vary as the secant of the angle of refraction CED: which agrees exactly with NEWTON's experiments; for the correction is perfectly inconsiderable.

Let the medium between the surfaces be rarer than the surrounding mediums; then the impulse reflected at the second surface, meeting a subsequent undulation at the first, will render the particles of the rarer medium capable of wholly stopping the motion of the denser, and destroying the reflection, (prop. iv.) while they themselves will be more strongly propelled than if they had been at rest; and the transmitted light will be increased. So that the colours by reflection will be destroyed, and those by transmission rendered more vivid, when the double thicknesses, or intervals of retardation, are any multiples of the whole breadths of the undulations; and, at intermediate thicknesses the effects will be reversed; according to the Newtonian observations.

If the same proportions be found to hold good with respect to thin plates of a denser medium, which is indeed not improbable, it will be necessary to adopt the corrected demonstration of prop. iv. but, at any rate, if a thin plate be interposed between a rarer and a denser medium, the colours by reflection and transmission may be expected to change places.

From Newton's measures of the thicknesses reflecting the different colours, the breadth and duration of their respective undulations may be very accurately determined; although it is not improbable, that when the glasses approach very near, the atmosphere of ether may produce some little irregularity. The whole visible spectrum appears to be comprised within the ratio of three to five, or a major sixth in music; and the undulations of red, yellow, and blue, to be related in magnitude as the numbers 8, 7, and 6; so that the interval from red to blue is a fourth. The absolute frequency expressed in numbers is too great to be distinctly conceived, but it may be better imagined by a comparison with sound. If a chord sounding the tenor $\bar{c}$, could be continually bisected 40 times, and should then vibrate, it would afford a yellow-green light: this being denoted by $\overset{41}{c}$, the extreme red would be $\overset{40}{a}$, and the blue $\overset{41}{d}$.

The absolute length and frequency of each vibration is expressed in the table; supposing light to travel in 8$\frac{1}{8}$ minutes 500,000,000000 feet.

Colours.	Length of an Undulation in parts of an Inch, in Air.	Number of Undulations in an Inch.	Number of Undulations in a Second.
Extreme -	.0000266	37640	463 millions of millions
Red - -	.0000256	39180	482
Intermediate	.0000246	40720	501
Orange - -	.0000240	41610	512
Intermediate	.0000235	42510	523
Yellow -	.0000227	44000	542
Intermediate	.0000219	45600	561 (= 2^{48} nearly)
Green - -	.0000211	47460	584
Intermediate	.0000203	49320	607
Blue - -	.0000196	51110	629
Intermediate	.0000189	52910	652
Indigo - -	.0000185	54070	665
Intermediate	.0000181	55240	680
Violet - -	.0000174	57490	707
Extreme - -	.0000167	59750	735

Scholium. It was not till I had satisfied myself respecting all these phenomena, that I found in HOOKE's Micrographia, a passage which might have led me earlier to a similar conclusion. "It is most evident that the reflection from the under or further side of the body, is the principal cause of the production "of these colours. — Let the ray fall obliquely on the thin "plate, part therefore is reflected back by the first superficies, "—part refracted to the second surface,—whence it is reflected "and refracted again.—So that, after two refractions and one

" reflection, there is propagated a kind of fainter ray—," and, " by reason of the time spent in passing and repassing,—this " fainter pulse comes behind the" former reflected " pulse; so " that hereby, (the surfaces being so near together that the eye " cannot discriminate them from one,) this confused or duplicated " pulse, whose strongest part precedes, and whose weakest fol- " lows, does produce on the retina, the sensation of a yellow. " If these surfaces are further removed asunder, the weaker " pulse may become coincident with the" reflection of the " second," or next following pulse, from the first surface, " and " lagg behind that also, and be coincident with the third, " fourth, fifth, sixth, seventh, or eighth—; so that, if there be " a thin transparent body, that from the greatest thinness requi- " site to produce colours, does by degrees grow to the greatest " thickness,—the colours shall be so often repeated, as the " weaker pulse does lose paces with its primary or first pulse, " and is coincident with a" subsequent " pulse. And this, as " it is coincident, or follows from the first hypothesis I took of " colours, so upon experiment have I found it in multitudes of " instances that seem to prove it." (P. 65—67.) This was printed about seven years before any of NEWTON's experiments were made. We are informed by NEWTON, that HOOKE was afterwards disposed to adopt his " suggestion" of the nature of colours; and yet it does not appear that HOOKE ever applied that improvement to his explanation of these phenomena, or inquired into the necessary consequence of a change of obliquity, upon his original supposition, otherwise he could not but have discovered a striking coincidence with the measures laid down by NEWTON from experiment. All former attempts to explain the colours of thin plates, have either proceeded on suppositions which, like NEWTON's, would lead us to expect the greatest irregularities in the direction of the refracted rays; or, like Mr. MICHELL's, would require such effects from the change of the angle of incidence, as are contrary to the effects observed; or they are equally deficient with respect to both these circumstances, and are inconsistent with the most moderate attention to the principal phenomena.

COROLLARY III. *Of the Colours of thick Plates.*

When a beam of light passes through a refracting surface, especially if imperfectly polished, a portion of it is irregularly scattered, and makes the surface visible in all directions, but most conspicuously in directions not far distant from that of the light itself: and, if a reflecting surface be placed parallel to the refracting surface, this scattered light, as well as the principal beam, will be reflected, and there will also be a new dissipation of light, at the return of the beam through the refracting surface. These two portions of scattered light will coincide in direction; and, if the surfaces be of such a form as to collect the similar effects, will exhibit rings of colours. The interval of retardation is here, the difference between the paths of the principal beam and of the scattered light between the two surfaces; of course, wherever the inclination of the scattered light is equal to that of the beam, although in different planes, the interval will vanish, and all the undulations will conspire. At other inclinations, the interval will be the difference of the secants from the secant of the inclination or angle of refraction of the principal beam. From these causes, all the colours of concave mirrors observed by NEWTON and others are necessary consequences: and it appears that their production, though somewhat similar, is by no means, as NEWTON imagined, identical with the production of those of thin plates.

COROLLARY IV. *Of Blackness.*

In the three preceding corollaries, we have considered the refracting and reflecting substances as limited by a mathematical surface; but this is perhaps never physically true. The ethereal atmospheres may extend on each side the surface as far as the breadth of one or more undulations; and, if they be supposed to vary equally in density at every part, the partial reflections from each of the infinite number of surfaces, where the density changes, will very much interfere with each other, and destroy a considerable portion of the reflected light, so that the substance may become positively black; and this effect may take place in a greater or less degree, as the density of the ethereal atmosphere varies more or less equably; and, in some cases, particular undulations being more affected than others, a tinge of colour may be produced. Accordingly, M. BOUGUER has observed a considerable loss of light, and in some instances a tinge of colour, in total reflections at the surface of a rarer medium.

COROLLARY V. *Of Colours by Inflection.*

Whatever may be the cause of the inflection of light passing through a small aperture, the light nearest its centre must be the least diverted, and the nearest to its sides the most: another portion of light falling very obliquely on the margin of the aperture, will be copiously reflected in various directions; some of which will either perfectly or very nearly coincide in direction with the unreflected light, and, having taken a circuitous route, will so interfere with it, as to cause an appearance of colours. The length of the two tracks will differ the less, as the direction of the reflected light has been less changed by its reflection, that is, in the light passing nearest to the margin ; so that the blues will appear in the light nearest the shadow. The effect will be increased and modified, when the reflected light falls within the influence of the opposite edge, so as to interfere with the light simply inflected by that also.

But, in order to examine the consequences more minutely, it will be convenient to suppose the inflection caused by an ethereal atmosphere, of a density varying as a given power of the distance from a centre, as in the eighth proposition of the last BAKERIAN Lecture. (Phil. Trans. for 1801, p. 83.) Putting $r=3$, and $x=\frac{1}{2}$, I have constructed a diagram, (Fig. 4,) which shows, by the two pairs of curves, the relative position of the reflected and unreflected portions of any one undulation at two successive times, and also, by shaded lines drawn across, the parts where the intervals of retardation are in arithmetical progression, and where similar colours will be exhibited at different distances from the inflecting substance. The result fully agrees with the observations of NEWTON's third book, and with those of later writers. But I do not consider it as quite certain, until further experiments have been made on the inflecting power of different substances, that Dr. HOOKE's explanation of inflection, by the tendency of light to diverge, may not have some pretensions to truth. I am sorry to be obliged to recall here the assent which, at first sight, I was induced to give to a supposed improvement of a late author. (Phil. Trans. for 1800, p. 128.)

Scholium. In the construction of the diagram, it becomes necessary to find the time spent by each ray in its passage, Since the velocity was denoted by $x^{-\frac{1}{r}}$, on the supposition of a projectile, it will be as $x^{\frac{1}{r}}$ on the contrary supposition, (Phil. Trans. for 1801, p. 27. *Schol.* 2. Prop. I.) and the fluxion of the distance described being $\frac{\dot{x}}{\sqrt{1-yy}}$, that of the time will be $\frac{x^{-\frac{1}{r}}\dot{x}}{\sqrt{1-yy}}$. or $\frac{rs}{1-r} \cdot \frac{\dot{y}}{yy \, . \sqrt{1-yy}}$, of which the fluent is $\frac{r}{1-r} \cdot \frac{s}{y} \cdot \sqrt{1-yy}$. Therefore, with the radius $x^{1-\frac{1}{r}}$, describe a circle concentric with the surfaces of the inflecting atmosphere, then the angle described by the ray during its passage through the atmosphere, will always be to the angle subtended by the line cut off by this circle from the incident ray produced, in the ratio of r to $r-1$; and the time spent in this passage, will be in the same ratio to the time that would have been spent in describing this intercepted portion with the initial velocity. For y, being equal to $sx^{\frac{1}{r}-1}$, is the sine of the inclination of the incident ray to the radius, where it meets this circle; therefore, by the proposition quoted, the angle described is in a given ratio to the angle at the centre, which is the difference of the inclinations. Making $x^{1-\frac{1}{r}}$ or $\frac{s}{y}$ radius, the sine, instead of y, becomes s, and the cosine $\sqrt{\frac{ss}{yy}-ss}$, or $\frac{s}{y}\sqrt{1-yy}$, and, when $y = ss$, $\sqrt{1-ss}$; therefore the line intercepted is to the difference of the fluents as r to $r-1$. (See also YOUNG's Syllabus, Art. 372.)

PROPOSITION IX.

Radiant Light consists in Undulations of the luminiferous Ether.

This proposition is the general conclusion from all the preceding; and it is conceived that they conspire to prove it in as satisfactory a manner as can possibly be expected from the nature of the subject. It is clearly granted by NEWTON, that there are undulations, yet he denies that they constitute light; but it is shown in the three first Corollaries of the last Proposition, that all cases of the increase or diminution of light are referable to an increase or diminution of such undulations, and that all the affections to which the undulations would be liable, are distinctly visible in the phenomena of light; it may therefore be very logically inferred, that the undulations are light.

A few detached remarks will serve to obviate some objections which may be raised against this theory.

1. NEWTON has advanced the singular refraction of the Iceland crystal, as an argument that the particles of light must be projected corpuscles; since he thinks it probable that the different sides of these particles must be differently attracted by the crystal, and since HUYGENS has confessed his inability to account in a satisfactory manner for all the phenomena. But, contrarily to what might have been expected from NEWTON'S usual accuracy and candour, he has laid down a new law for the refraction, without giving a reason for rejecting that of HUYGENS, which Mr. HAUY has found to be more accurate than NEWTON'S; and, without attempting to deduce from his own system any explanation of the more universal and striking effects of doubling spars, he has omitted to observe that HUYGENS'S most elegant and ingenious theory perfectly accords with these general effects, in all particulars, and of course derives from them additional pretensions to truth: this he omits, in order to point out a difficulty, for which only a verbal solution can be found in his own theory, and which will probably long remain unexplained by any other.

2. Mr. MICHELL has made some experiments, which appear to show that the rays of light have an actual momentum, by

means of which a motion is produced when they fall on a thin plate of copper delicately suspended. (PRIESTLEY'S Optics.) But, taking for granted the exact perpendicularity of the plate, and the absence of any ascending current of air, yet since, in every such experiment, a greater quantity of heat must be communicated to the air at the surface on which the light falls than at the opposite surface, the excess of expansion must necessarily produce an excess of pressure on the first surface, and a very perceptible recession of the plate in the direction of the light. Mr. BENNET has repeated the experiment, with a much more sensible apparatus, and also in the absence of air; and very justly infers from its total failure, an argument in favour of the undulatory system of light. (Phil. Trans. for 1792, p. 87.) For, granting the utmost imaginable subtility of the corpuscles of light, their effects might naturally be expected to bear some proportion to the effects of the much less rapid motions of the electrical fluid, which are so very easily perceptible, even in their weakest states.

3. There are some phenomena of the light of solar phosphori, which at first sight might seem to favour the corpuscular system; for instance, its remaining many months as if in a latent state, and its subsequent re-emission by the action of heat. But, on further consideration, there is no difficulty in supposing the particles of the phosphori which have been made to vibrate by the action of light, to have this action abruptly suspended by the intervention of cold, whether as contracting the bulk of the substance or otherwise; and again, after the restraint is removed, to proceed in their motion, as a spring would do which had been held fast for a time in an intermediate stage of its vibration; nor is it impossible that heat itself may, in some circumstances, become in a similar manner latent. (NICHOLSON'S

Journal. Vol. II. p. 399.) But the affections of heat may perhaps hereafter be rendered more intelligible to us; at present, it seems highly probable that light differs from heat only in the frequency of its undulations or vibrations; those undulations which are within certain limits, with respect to frequency, being capable of affecting the optic nerve, and constituting light; and those which are slower, and probably stronger, constituting heat only; that light and heat occur to us, each in two predicaments, the vibratory or permanent, and the undulatory or transient state; vibratory light being the minute motion of ignited bodies, or of solar phosphori, and undulatory or radiant light the motion of the ethereal medium excited by these vibrations; vibratory heat being a motion to which all material substances are liable, and which is more or less permanent; and undulatory heat that motion of the same ethereal medium, which has been shown by Mr. KING, (Morsels of Criticism. 1786. p. 99,) and M. PICTET, (*Essais de Physique.* 1790,) to be as capable of reflection as light, and by Dr. HERSCHEL to be capable of separate refraction. (Phil. Trans. for 1800. p. 284.) How much more readily heat is communicated by the free access of colder substances, than either by radiation or by transmission through a quiescent medium, has been shown by the valuable experiments of Count RUMFORD. It is easy to conceive that some substances, permeable to light, may be unfit for the transmission of heat, in the same manner as particular substances may transmit some kinds of light, while they are opaque with respect to others.

On the whole it appears, that the few optical phenomena which admit of explanation by the corpuscular system, are equally consistent with this theory; that many others, which have long been known, but never understood, become by these means perfectly intelligible; and that several new facts are found to be thus only reducible to a perfect analogy with other facts, and to the simple principles of the undulatory system. It is presumed, that henceforth the second and third books of NEWTON's Optics will be considered as more fully understood than the first has hitherto been; but, if it should appear to impartial judges, that additional evidence is wanting for the establishment of the theory, it will be easy to enter more minutely into the details of various experiments, and to show the insuperable difficulties attending the NEWTONIAN doctrines, which, without necessity, it would be tedious and invidious to enumerate. The merits of their author in natural philosophy, are great beyond all contest or comparison; his optical discovery of the composition of white light, would alone have immortalised his name; and the very arguments which tend to overthrow his system, give the strongest proofs of the admirable accuracy of his experiments.

Sufficient and decisive as these arguments appear, it cannot be superfluous to seek for further confirmation; which may with considerable confidence be expected, from an experiment very ingeniously suggested by Professor ROBISON, on the refraction of the light returning to us from the opposite margins of Saturn's ring; for, on the corpuscular theory, the ring must be considerably distorted when viewed through an achromatic prism: a similar distortion ought also to be observed in the disc of Jupiter; but, if it be found that an equal deviation is produced in the whole light reflected from these planets, there can scarcely be any remaining hope to explain the affections of light, by a comparison with the motions of projectiles.

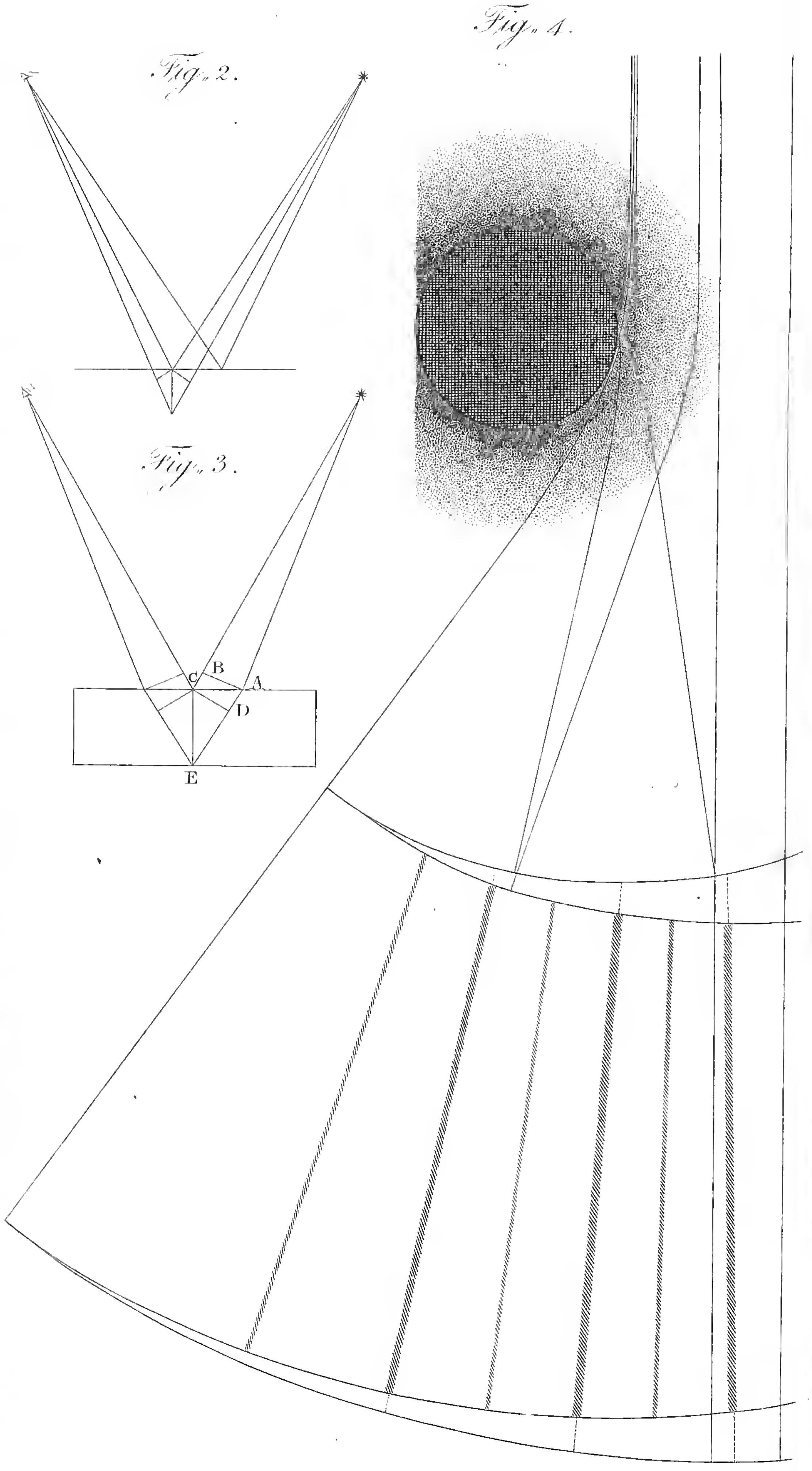

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Fig. 1.

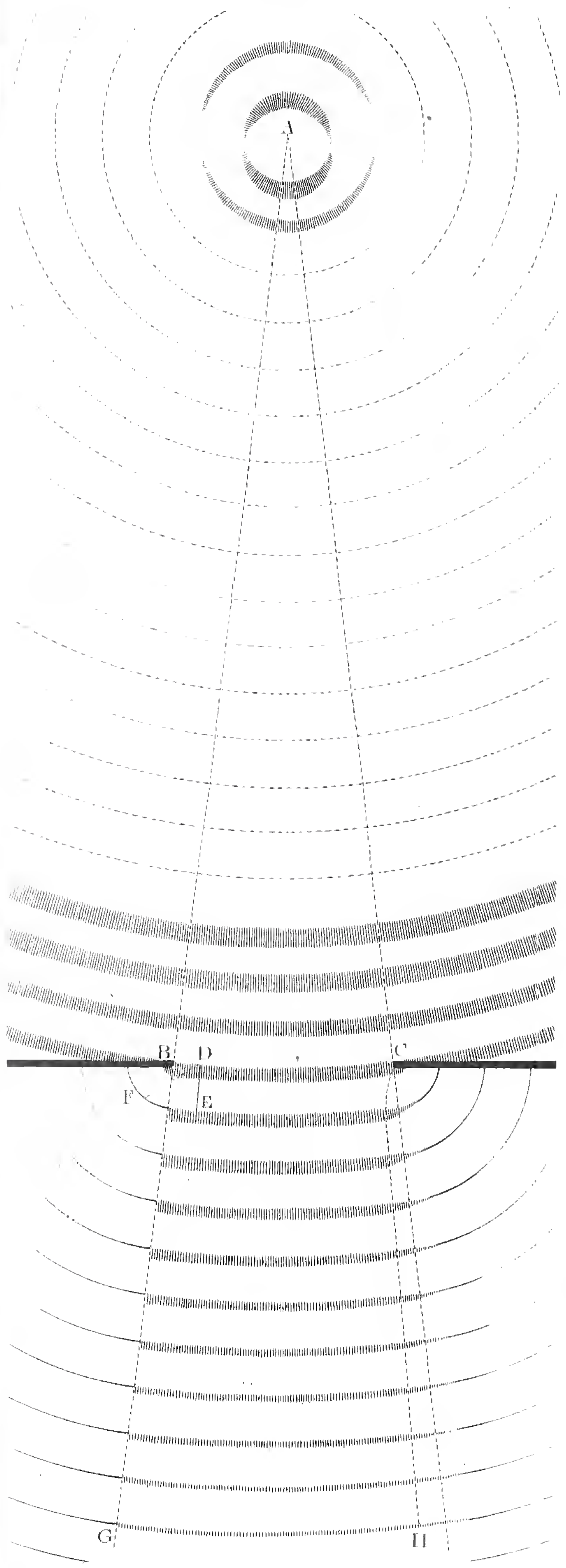


Fig. 2.

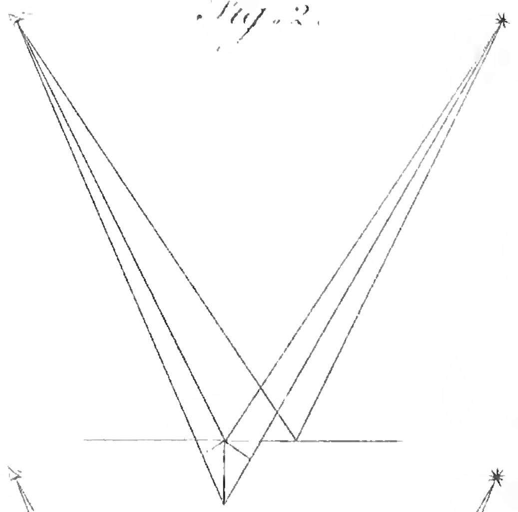


Fig. 3.

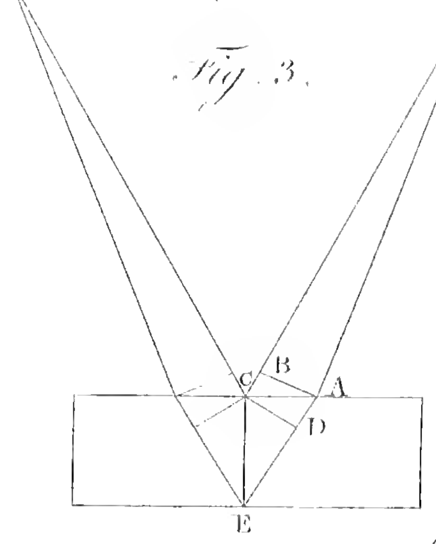
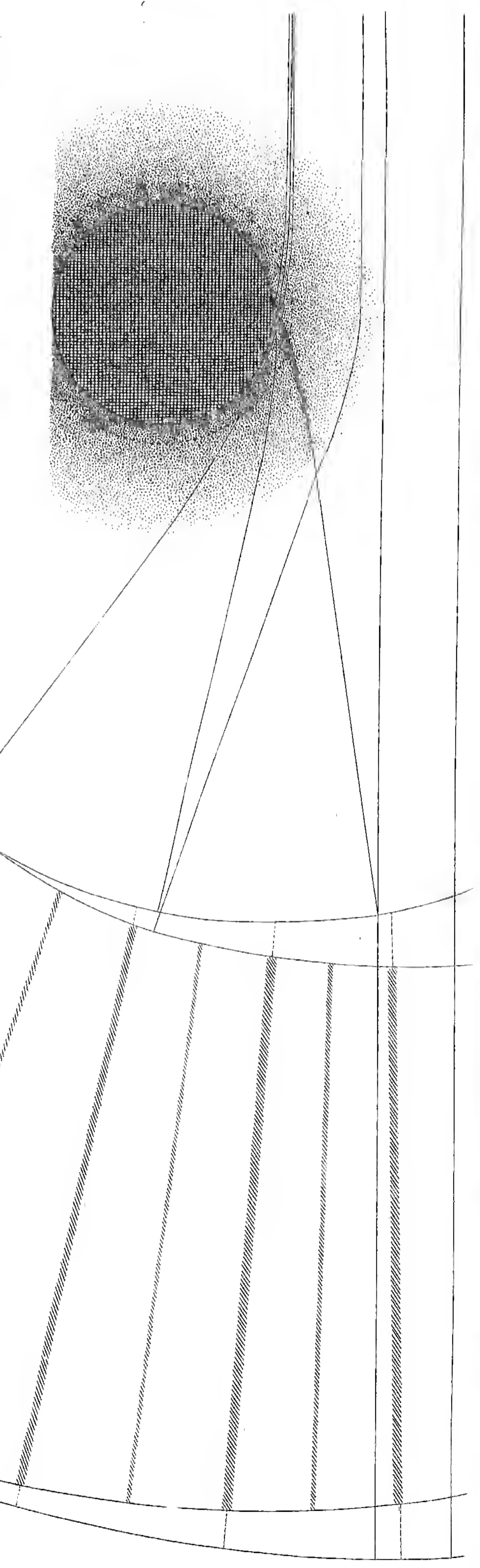


Fig. 4.



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III. *An Analysis of a mineral Substance from* North America, *containing a Metal hitherto unknown. By* Charles Hatchett, *Esq. F. R. S.*

Read November 26, 1801.

In the course of the last summer, when I was examining and arranging some minerals in the British Museum, I observed a small specimen of a dark-coloured heavy substance, which attracted my attention, on account of some resemblance which it had with the Siberian chromate of iron, on which at that time I was making experiments.

Upon referring to Sir Hans Sloane's catalogue, I found that this specimen was only described as "a very heavy black stone, "with golden streaks," which proved to be yellow mica; and it appeared, that it had been sent, with various specimens of iron ores, to Sir Hans Sloane, by Mr. Winthrop, of Massachusets. The name of the mine, or place where it was found, is also noted in the catalogue; the writing however is scarcely legible: it appears to be an Indian name, (Nautneauge;) but I am informed by several American gentlemen, that many of the Indian names (by which certain small districts, hills, &c. were forty or fifty years ago distinguished,) are now totally forgotten, and European names have been adopted in the room of them. This may have been the case in the present instance; but, as the other specimens sent by Mr. Winthrop were from the mines of Massachusets, there is every reason to believe that the mineral substance in question came from one of them, although it may not now be easy to identify the particular mine.

§ I. DESCRIPTION OF THE ORE.

The external colour is dark brownish gray.

The internal colour is the same, inclining to iron gray.

The longitudinal fracture is imperfectly lamellated; and the cross fracture shews a fine grain.

The lustre is vitreous, slightly inclining in some parts to metallic lustre.

It is moderately hard, and is very brittle.

The colour of the streak or powder is dark chocolate brown.

The particles are not attracted by the magnet.

The specific gravity, at temp. 65°, is 5918.*

Experiment I.

Some of the ore, reduced to fine powder, was digested in boiling muriatic acid for about one hour.

The acid appeared to have acted but slightly upon the powder; as the former remained colourless, and the latter did not seem to be diminished. A portion, however, chiefly of iron, was found to be dissolved; for ammonia formed a yellow flocculent precipitate; prussiate of potash produced one which was blue;

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* The following results of some experiments which I have purposely made, will shew how much the specific gravity of this ore is different from that of Wolfram, and Siberian chromate of iron.

Pure Wolfram, free from extraneous substances, at temp. 65° - -	6955.
Siberian chromate of iron, containing some of the green oxide -	3728.
Pure Siberian chromate of iron - - -	4355.

The Siberian chromate of iron, like all other mineral substances which are not crystallized, and which consequently are not always homogeneous, must evidently be liable to considerable variations in specific gravity.

and tincture of galls, when the excess of acid had been previously saturated by an alkali, formed a precipitate of a rich purplish brown colour.

Experiment II.

Another portion of the powder was, in like manner, digested with nitric acid; but, excepting some slight traces of iron, this acid afforded nothing worthy of notice; the action of it upon the ore, was indeed scarcely perceptible.

Experiment III.

Some of the pulverized ore was digested with concentrated sulphuric acid, in a strongly-heated sand-bath, until nearly the whole of the acid was evaporated; the edges of the mass then appeared bluish, and became white, when boiling distilled water was added.

This acid certainly acted much more powerfully than those which have been mentioned; but still only a small part of the ore was dissolved. It must however be observed, that a very copious blue precipitate was obtained by prussiate of potash; a plentiful purplish brown precipitate was also produced by tincture of galls, after the excess of acid had been saturated by an alkali; and, lastly, when the yellow ferruginous precipitate formed by ammonia was dissolved in diluted nitric acid, some white flocculi remained, which were completely insoluble in the acid, even when it was added so as to be in considerable excess.

From these experiments it was evident, that the ore could not readily be decomposed by the direct application of the mineral acids; and I therefore had recourse to the following method, which has frequently been employed with success in similar cases.

ANALYSIS.

A.

A mixture of 200 grains of the powdered ore with five times the weight of carbonate of potash, was exposed to a strong red heat, in a silver crucible. As soon as the matter began to flow, a very perceptible effervescence took place; and, when this had subsided, the whole was poured into a proper vessel.

The mass, when cold, was grayish-brown.

Boiling distilled water was poured upon it; and the brown residuum, which was considerable, was well edulcorated upon a filter.

The filtrated liquor had a slight yellowish tinge, and, being supersaturated with nitric acid, afforded a copious white flocculent precipitate, which speedily subsided; but, although a very considerable additional quantity of nitric acid was poured upon the precipitate, it was not re-dissolved.

The residuum of the ore was dark brown, and was again melted with potash, and treated as before; but scarcely any effect was thus produced; the alkali was therefore washed off, and the powder was digested with muriatic acid, which soon assumed the deep yellow colour usually communicated to it by iron. After half an hour, the acid was decanted, and the residuum was washed with distilled water.

This powder was now of a much paler colour; and, being mixed with potash, it was melted and treated as before. A considerable precipitate was again obtained by the addition of nitric acid; and the residuum, after being digested with muriatic acid, was again fused with potash, by which means the whole was completely decomposed, after about five repetitions of each operation.

B.

The muriatic solution was diluted, and, being saturated with ammonia, afforded a plentiful ochraceous precipitate; which again was dissolved in cold dilute nitric acid, and afforded a small quantity of a white insoluble substance, similar to that which was obtained from the alkaline solution. From this nitric solution, I then obtained, by means of ammonia, a precipitate of oxide of iron, which, being properly dried, weighed 40 grains.

C.

The different alkaline solutions which had been made subsequent to that which has been first mentioned, were mixed together, and, being supersaturated with nitric acid, afforded the same white insoluble precipitate; the total quantity of which, obtained from 200 grains of the ore, amounted to about 155 grains.

The liquor from which this precipitate had been separated by nitric acid, was then saturated with ammonia, and, being boiled, afforded about 2 grains of oxide of iron.

I obtained, therefore, from 200 grains of the ore,

	Grains.	Grains.
Oxide of iron - - -	42	= 197.
And of the white precipitated substance	155	

But, as I could not repeat the analysis without destroying the remaining part of the only specimen at present known of this ore, I do not wish the above stated proportions to be regarded as rigidly exact; it will be sufficient, therefore, to say at present, that the ore is composed of about three parts of the white matter, and rather less than one of iron.

§ II. PROPERTIES OF THE WHITE PRECIPITATE.

A.

It is of a pure white, and is not extremely heavy.

It has scarcely any perceptible flavour, nor does it appear to be soluble in boiling water; when, however, some of the powder is placed upon litmus paper moistened with distilled water, the paper in a few minutes evidently becomes red.

B.

1. When examined by the blow-pipe, it is not fusible per se in a spoon of platina, nor upon charcoal, but only becomes of a less brilliant white.

2. Borax does not appear to act upon it; for the white particles are only dispersed throughout the globule.

3. It produces an effervescence when fused with carbonate of soda, and forms a colourless salt; but, if too much of it be added, then the mass, when cold, appears like a white opaque enamel.

4. When carbonate of potash is employed, the effects are similar in every respect to those of soda; and it may here be remarked, that the saline combinations thus formed with soda, or potash, are soluble in water; and that these solutions have the same properties as that which was formed when the ore was decomposed by an alkali. The portion of the white precipitate which may be in excess, subsides unaltered, when the globules are dissolved in water.

5. Phosphate of ammonia produces a very marked effect; for, when melted in a platina spoon, if some of the white substance be added, a considerable effervescence takes place, and the two substances rapidly unite. The globule, when cold, is

deep blue, with a tinge of purple, but, when held between the eye and the light, it appears of a greenish gray colour.

C.

It is perfectly insoluble, and remains unchanged in colour, and in every other respect, when digested in boiling concentrated nitric acid.

D.

It is dissolved by boiling sulphuric acid, and forms a transparent colourless solution, which is however only permanent while the acid remains in a concentrated state; for, if a large quantity of water be added to the solution, or if the latter be poured into a vessel of distilled water, the whole in a few minutes assumes a milky appearance, and a white precipitate is gradually deposited, which cracks as it becomes dry upon the filter, and, from white, changes to a lavender-blue colour, and again, when completely dry, to a brownish gray. It is then insoluble in water, has not any flavour, is semi-transparent, and breaks with a glossy vitreous fracture.

This substance is much heavier than the original white precipitate; and in a very slight degree may be dissolved by boiling muriatic acid, or by boiling lixivium of potash.

Upon examining these solutions, I found that both contained the original white substance, together with some sulphuric acid; so that the precipitate obtained from the sulphuric solution by the addition of water, is a sulphate of the white matter.*

The whole is not however precipitated by water; for a part

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* This sulphate is also precipitated when the sulphuric solution has been long exposed in an open vessel to the air; and, according as this may be moist or dry, the effect is produced sooner or later.

remains in solution, which may be separated from the sulphuric acid by either of the fixed alkalis, or by ammonia.

The sulphuric solution is not rendered turbid by the addition of water, until some minutes at least have elapsed; when, therefore, some prussiate of potash was added immediately after the water, the colour of the liquor became olive green, and a copious precipitate, of a beautiful olive colour, was gradually deposited.

Tincture of galls, after a few minutes, caused the liquor to become turbid, and a very high orange-coloured precipitate was obtained.

A few drops of phosphoric acid were added to a part of the concentrated sulphuric solution; and, after about 12 hours, the whole became a white opaque stiff jelly, which was insoluble in water.

Potash, soda, and ammonia, whether pure or in the state of carbonates, separate the substance in question from the sulphuric solution, in the form of a white flocculent precipitate; and, when these alkalis are added to a considerable excess, they do not redissolve the precipitate, unless they are heated; then, indeed, the fixed alkalis act upon it, and form combinations which have already been mentioned, but which we shall soon have occasion more particularly to notice.

E.

1. The white precipitate, when recently separated from potash, is soluble in boiling muriatic acid; and this solution may be considerably diluted with water, without any change being produced.

2. A part was evaporated to dryness, and left a pale yellow substance, which was not soluble in water, and was dissolved

with great difficulty, when it was again digested with muriatic acid.

3. Prussiate of potash changed the colour of the muriatic solution to an olive-green; the liquor then gradually became turbid, and an olive-coloured precipitate was obtained, similar to that which has been lately mentioned. But,

4. If some nitric acid was previously added to the muriatic solution, then the prussiate changed the liquor to a grass-green, but did not produce any precipitate.

5. Tincture of galls, in a few minutes, formed an orange-coloured precipitate, like that which has been mentioned; but, if the acid was in too great an excess, it was necessary to add a small quantity of lixivium of potash or soda, before the precipitate could be obtained.

6. A small quantity of phosphoric acid, being added to the muriatic solution, in a few hours formed a white flocculent precipitate.

7. Potash, soda, and ammonia, also produced white flocculent precipitates, which were not redissolved by an excess of the alkalis, unless the liquors were heated; and, in that case, part was dissolved by the fixed alkalis, but not by ammonia.

8. The muriatic solution did not yield any precipitate, when the muriates of lime, magnesia, and strontian, were added; but muriate of barytes formed a slight cloud.

9. When a piece of zinc was immersed in the muriatic solution, a white flocculent precipitate was obtained.*

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* This appears to indicate the obstinacy with which this substance retains a certain portion of oxygen; for we here see that zinc does not precipitate it in the metallic state, but only reduces it to an insoluble oxide.

F.

The acetous acid has not any apparent effect on the white precipitate, when long digested with it.

G.

The fixed alkalis readily combine with this substance, both in the dry and in the humid way.

We have already seen, that the former method was employed with success in the analysis of the ore; and the experiments made with the blow-pipe may be regarded as an additional confirmation. In each of these cases, the white precipitate combined with the alkali, as soon as the heat was sufficient to cause the latter to flow; and, when a carbonate was employed, a portion of carbonic acid was expelled.

The carbonic acid was in like manner disengaged, when the white precipitate was boiled with lixivium of carbonate of potash, or of soda; and the solutions thus prepared, resembled in every respect those which were formed by dissolving in water the salts which had been produced in the dry way.

It will be proper here to give a more particular account of these combinations.

1. Some of the white precipitate was digested, during nearly one hour, with boiling lixivium of pure or caustic potash: about one-fourth of the powder was dissolved; and the remainder, which appeared little if at all altered, subsided to the bottom of the vessel.

The clear solution, which contained a great excess of alkali, was decanted; and, by gentle evaporation, yielded a white glittering salt, in scales, very much resembling the concrete boracic acid.

The salt was placed upon a filter, so that the lixivium might be separated. It was then washed with a small quantity of cold distilled water; and, being dried, remained as above described, although constantly exposed to the open air.

This salt had an acrid disagreeable flavour, and contained a small excess of alkali. It did not dissolve very readily in cold water; but, when dissolved, the solution was perfect and permanent.

Some nitric acid was added to part of the solution, and immediately rendered it white and turbid. In a short time, a white precipitate was collected, similar to that which had been employed to neutralise the potash; and the clear supernatant liquor, being evaporated, only afforded nitre.

Prussiate of potash was added to another portion; but did not produce any effect, until some muriatic acid was dropped into the liquor, which then immediately assumed a tinge of olive green, and slowly deposited a precipitate of the same colour.

Tincture of galls did not affect the solution at first; but, when a few drops of muriatic acid had been added, it gradually lost its transparency, and yielded an orange-coloured precipitate.

2. As so large a part of the white precipitate had remained undissolved in the foregoing experiment, it was digested again with another portion of the same lixivium, but without any effect. I therefore washed off the alkali, and boiled some nitric acid with the powder, until the acid was completely evaporated. After this, the powder was exposed to a strong heat in a sand-bath. It was then again digested with the lixivium, and a part was dissolved as before; but still the residuum required to be treated with nitric acid, before the alkaline liquor could again act upon it; so that it was necessary to repeat these alternate

operations several times, before the whole of the powder could be united with the alkali.

3. When the white precipitate was digested with solution of carbonate of potash, or of soda, it was dissolved, much in the same manner as above related; and the properties of the solutions, when examined by reagents, were also similar, excepting that the orange-coloured precipitates produced by tincture of galls were of a paler colour.

Tungstate of potash, molybdate of potash, and cobaltate of ammonia, being severally added to the solution of the white substance in potash, produced white flocculent precipitates.

Hydro-sulphuret of ammonia produced a reddish chocolate-coloured precipitate.

4. As the ore was decomposed by being fused with potash, the following experiment affords a curious instance (among the many already known) of the change in the order of affinities produced by a difference of temperature.

Some of the solution of the white precipitate in potash, was poured into the alkaline solution of iron, which was formerly known by the name of Stahl's *Tinctura Alkalina Martis*. Potash was in excess in both of these solutions; but nevertheless a cloud was immediately produced, and a brown ferruginous precipitate was deposited.

Part of this precipitate was dissolved in muriatic acid; and the solution, being examined in the usual way, yielded a blue precipitate when prussiate of potash was added, and a purplish brown precipitate with tincture of galls.

The other part of the precipitate was digested with dilute nitric acid; which dissolved the ferruginous part, but left untouched a white flocculent matter, perfectly resembling the substance which has been so often mentioned. The precipitate therefore produced by the mixture of the two alkaline solutions, was a combination of the white matter with oxide of iron, very similar to the original ore.

H.

The white precipitate, when distilled with four parts of sulphur, remained pulverulent, and, from white, was only changed to a pale ash colour.

Nitric acid was digested on the powder, and, being heated, afforded some nitrous gas ; after this, the powder became white, and in every respect recovered its original properties.

I.

Before I conclude this section, I must observe, that when the olive-green precipitates, obtained by prussiate of potash, were digested in an alkaline lixivium, they were decomposed; for the alkali combined with the prussic acid, and with a small part of the white matter ; but the greater part of the latter remained undissolved, in the same white flocculent state which was noticed when the alkaline combinations were mentioned.

The orange-coloured precipitates, formed by tincture of galls, were also decomposed when digested in boiling nitric acid ; and the white matter was recovered in its original state.

§ III. REMARKS.

The preceding experiments shew, that the ore which has been analysed, consists of iron combined with an unknown substance, and that the latter constitutes more than three-fourths of the whole. This substance is proved to be of a metallic nature, by the coloured precipitates which it forms with prussiate of potash, and with tincture of galls; by the effects which zinc produces, when immersed in the acid solutions; and by the colour which it communicates to phosphate of ammonia, or rather to concrete phosphoric acid, when melted with it.

Moreover, from the experiments made with the blow-pipe, it seems to be one of those metallic substances which retain oxygen with great obstinacy, and are therefore of difficult reduction.

It is an acidifiable metal; for the oxide reddens litmus paper, expels carbonic acid, and forms combinations with the fixed alkalis. But it is very different from the acidifiable metals which have of late been discovered; for,

1. It remains white when digested with nitric acid.

2. It is soluble in the sulphuric and muriatic acids, and forms colourless solutions, from which it may be precipitated, in the state of a white flocculent oxide, by zinc, by the fixed alkalis, and by ammonia. Water also precipitates it from the sulphuric solution, in the state of a sulphate.

3. Prussiate of potash produces a copious and beautiful olive-green precipitate.

4. Tincture of galls forms orange or deep yellow precipitates.

5. Unlike the other metallic acids, it refuses to unite with ammonia.

6. When mixed and distilled with sulphur, it does not combine with it so as to form a metallic sulphuret.

7. It does not tinge any of the fluxes, except phosphoric acid, with which, even in the humid way, it appears to have a very great affinity.

8. When combined with potash and dissolved in water, it forms precipitates, upon being added to solutions of tungstate of potash, molybdate of potash, cobaltate of ammonia, and the alkaline solution of iron.

These properties completely distinguish it from the other acidifiable metals, *viz.* arsenic, tungsten, molybdena, and chromium; as to the other metals lately discovered, such as uranium, titanium, and tellurium, they are still farther removed from it.

The colours of the precipitates produced by prussiate of potash and tincture of galls, approach the nearest to those afforded by titanium. But the prussiate of the latter is much browner; and the gallate is not of an orange colour, but of a brownish red, inclining to the colour of blood. Besides, even if these precipitates were more like each other, still the obstinacy with which titanium refuses to unite with the fixed alkalis, and the insolubility of it in acids when heated, sufficiently denote the different nature of these two substances.

The iron in the ore which has been examined, is apparently in the same state as it is in wolfram, *viz.* brown oxide; and this oxide is mineralised by the metallic acid which has been described, in the same manner as the oxides of iron and manganese are mineralised by the tungstic acid or rather oxide. For, from several experiments made upon a large scale, I have reason to believe that in wolfram, the tungsten has not attained the maximum of oxidation. Several facts in the course of the experiments lately described, seem to prove, that this new metal differs from tungsten and the other acidifiable metals, by a more limited extent of oxidation; for, unlike these, it seems to be incapable of retaining oxygen sufficient to enable the total quantity to combine with the fixed alkalis. In § II. G. 2, this is very evident; for, from the experiment there described it appears, that when the metallic acid or oxide was digested with lixivium of potash, only a part was dissolved; and that the remainder was insoluble in the same lixivium, till it had received an additional portion of oxygen, by being treated with nitric acid; also that several of these alternate operations were required, before any given quantity of the metallic oxide could be completely combined with the alkali. Now there is much reason to believe, that in this case, when the metallic oxide or acid was digested with potash, the portion which was dissolved, received an accession of oxygen at the expense of the other part, which of course was thus reduced to the state of an insoluble oxide, and therefore required to be again oxidated by nitric acid, before it could combine with the alkaline solution; but still it appeared, that an adequate proportion of oxygen could never be superinduced, so as to render the oxide totally and immediately soluble in the alkalis by one operation, or even by two.

We may, therefore, regard this as an instance of the effects resulting from disposing affinity, and as very similar to those observed in respect to copper, which have been noticed by my ingenious friend Mr. CHENEVIX, in his valuable analysis of the arseniates of copper and of iron.*

My researches into the properties of this metal, have of course been much limited by the smallness of the quantity which I had to operate upon; but I flatter myself that more of the ore may soon be procured from the Massachuset mines, particularly as a gentleman now in England, (Mr. SMITH, Secretary to the American Philosophical Society,) has obligingly offered his assistance on this occasion. We shall then be able more fully to investigate the nature of this substance; and shall be more capable of judging how far it may be applicable to useful purposes. At present, all that can be said is, that the olive green prussiate and the orange-coloured gallate are fine colours;

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* Phil. Trans. for 1801, p. 233.

and, as they do not appear to fade when exposed to light and air, they might probably be employed with advantage as pigments.

I am much inclined to believe, that the time is perhaps not very distant, when some of the newly-discovered metals, and other substances, which are now considered as simple, primitive, and distinct bodies, will be found to be compounds. Yet I only entertain and state this opinion as a probability; for, until an advanced state of chemical knowledge shall enable us to compose, or at least to decompose, these bodies, each must be classed and denominated as a substance *sui generis*. Considering, therefore, that the metal which has been examined is so very different from those hitherto discovered, it appeared proper that it should be distinguished by a peculiar name; and, having consulted with several of the eminent and ingenious chemists of this country, I have been induced to give it the name of Columbium.

POSTSCRIPT.

It appears proper to mention some unsuccessful attempts, which I have lately made to reduce the white oxide.

Fifty grains were put into a crucible coated with charcoal; and, being covered with the same, the crucible was closely luted, and was exposed to a strong heat, in a small wind-furnace, during about one hour and an half. When the crucible was broken, the oxide was found in a pulverulent state; and, from white, was become perfectly black.

In order to form a phosphuret, some phosphoric acid was poured upon a portion of the white oxide; and, being evaporated to dryness, the whole was put into a crucible coated with charcoal, as above described. The crucible was then placed in a forge belonging to Mr. CHENEVIX; and a strong heat was kept up for half an hour.

The inclosed matter was spongy, and of a dark brown; it in some measure resembled phosphuret of titanium.

After this, we wished to try the effect of a still greater heat; but in this experiment the crucible was melted.

The above experiments shew, that the white oxide, like several other metallic substances, may be deoxidated to a certain degree, without much difficulty, but that the complete reduction of it is still far from being easily effected.

IV. *A Description of the Anatomy of the* Ornithorhynchus paradoxus. *By* Everard Home, *Esq. F. R. S.*

Read December 17, 1801.

THE subjects from which the following description is taken, were sent from New South Wales, to Sir JOSEPH BANKS, who very obligingly submitted them to my examination.

These were two specimens preserved in spirit; one male, the other female. The male was rather larger than the female, and in every respect a much stronger animal; they had both arrived at their full growth, or nearly so, as the epiphyses were completely united to the bodies of the bones, which is not the case in growing animals.

The natural history of this animal is at present very little known. Governor HUNTER, who has lately returned from New South Wales, where he had opportunities of seeing them alive, has favoured me with the following particulars respecting them.

The Ornithorhynchus is only found in the fresh-water lakes, of which there are many in the interior parts of the country, some three quarters of a mile long, and several hundred yards broad. This animal does not swim upon the surface of the water, but comes up occasionally to breathe, which it does in the same manner as the turtle. The natives sit upon the banks, with small wooden spears, and watch them every time they come to the surface, till they get a proper opportunity of striking them. This they do with much dexterity; and frequently succeed in catching them in this way.

Governor HUNTER saw a native watch one for above an hour before he attempted to spear it, which he did through the neck and fore leg: when on shore, it used its claws with so much force, that they were obliged to confine it between two pieces of board, while they were cutting off the barbs of the spear, to disengage it. When let loose, it ran upon the ground with as much activity as a land tortoise; which is faster than the structure of its fore feet would have led us to believe. It inhabits the banks of the lakes, and is supposed to feed in the muddy places which surround them; but the particular kind of food on which it subsists, is not known.

Description of the external Appearances.

The male is 17½ inches in length, from the point of the bill to the extremity of the tail. The bill is 2¼ inches long; and the tail, measuring from the anus, 4½ inches.

The body of the animal is compressed, and nearly of the same general thickness throughout, except at the shoulders, where it is rather smaller. The circumference of the body is 11 inches. There is no fat deposited between the skin and the muscles.

The female measures in length 16¼ inches, and in circumference 11 inches. The size of the body is rendered proportionally larger than that of the male, by a quantity of fat lying every where under the skin.

The male is of a very dark brown colour, on the back, legs, bill, and tail; the under surface of the neck and belly is of a silver gray. In the female, the colour of the belly is lighter.

The hair is made up of two kinds; a very fine thick fur, $\frac{1}{2}$ of an inch long, and a very uncommon kind of hair, $\frac{3}{4}$ of an inch long; the portion next the root has the common appearance of hair, but, for $\frac{1}{4}$ of an inch towards the point, it becomes flat, giving it some faint resemblance to very fine feathers: this portion has a gloss upon it; and, when the hair is dry, the different reflections from the edges and surfaces of these longer hairs, give the whole a very uncommon appearance. The fur and hair upon the belly, is longer than that upon the back.

Externally there is no appearance of the organs of generation, in either sex; the orifice of the anus being a common opening to the rectum and prepuce in the male, and to the rectum and vagina in the female.

There is no appearance, that could be detected, of nipples; although the skin on the belly of the female was examined with the utmost accuracy for that purpose.

The head is rather compressed. The bill, which projects beyond the mouth, in its appearance resembles that of the duck; but is in its structure more like that of the spoonbill, the middle part being composed of bone, as in that bird; it has a very strong cuticular covering.

In the upper portion of the bill, the lip extends for half an inch anteriorly, and laterally, beyond the bony part, and is thick and fleshy. The upper surface of the bill is uniformly smooth, and does not terminate where the hair begins, but is continued on for $\frac{3}{4}$ of an inch, forming a cuticular flap, which lies loose upon the hair. In the dried specimens that have been brought to Europe, the flap has been contracted in drying, and stands up perpendicularly; this, however, is now ascertained not to be its natural situation.

The under surface of the upper half of the bill is also smooth; but has two hard ridges of a horny nature, an inch long and $\frac{1}{10}$ of an inch broad, situated longitudinally, one on each side of the middle line of the bill.

The lower portion of the bill is much smaller than the upper; and, when opposed to it, the lip of the upper extends beyond it for the whole of its breadth. The edges of the lip of this lower portion have deep serræ, in a transverse direction, like those in the duck's bill, but they are entirely confined to the fleshy lip; and, immediately within these serrated edges are grooves, lined with a horny substance, which receive, in the closed state of the bill, the ridges of the upper portion above described. There is also a cuticular flap extended upon the hair, as in the upper portion of the bill.

The nostrils are two orifices, very close to each other, near the end of the bill; the upper lip projecting $\frac{3}{4}$ of an inch beyond them.

The eyes are very small; they are situated more upon the upper part of the head than is usual, and are directly behind the loose edge of the cuticular flap belonging to the bill. The eyelids are circular orifices, concealed in the hair; and in the male are with difficulty discovered, but in the female there is a tuft of lighter hair, which marks their situation.

The external ears are two oval slits, directly behind the eyes, and much larger than the orifices of the eyelids.

The teeth, if they can be so called, are all grinders; they are four in number, situated in the posterior part of the mouth, one on each side of the upper and under jaw, and have broad flattened crowns. In the smaller specimens before examined, each of these large teeth appeared to be made up of two smaller ones, distinct from each other. The animal, therefore, most probably sheds its teeth as it increases in size. They differ from common teeth very materially, having neither enamel nor bone, but being composed of a horny substance only embedded in the gum, to which they are connected by an irregular surface, in the place of fangs. When cut through, which is readily done by a knife, the internal structure is fibrous, like nail; the direction of the fibres is from the crown downwards.

This structure is similar to that of the horny crust which lines the gizzard in birds.

Between the cheek and the jaw, on each side of the mouth, there is a pouch, as in the monkey tribe, lined with a cuticle. When laid open, it is 1½ inch long, and the same in breadth. In the female, it contained a concreted substance, the size of a very small nut, one in each pouch: this, when examined in the microscope, was made up of very small portions of broken crystals.

Besides these grinding teeth, there are two small pointed horny teeth upon the projecting part of the posterior portion of the tongue, the points of which are directed forwards, seemingly to prevent the food from being pushed into the fauces during the process of mastication. This circumstance, of small teeth on the tongue, is, I believe, peculiar to this animal, not being met with in other quadrupeds. In the tongue of the flamingo there is a row of short teeth on each side, but in no other bird that I have seen. The teeth are represented in the annexed drawing.

The fore legs are short, and the feet webbed; the length of the leg and foot, to the end of the web, is about three inches. On each foot there are five toes, united together by the web, which is very broad, and is continued beyond the points of the toes, for nearly an inch. On each toe there is a rounded straight nail, which lies loose upon the membrane forming the web. The palms of the feet are covered with a strong cuticle; and there is a small prominence at the heel.

The hind legs are nearly of the same length as the fore legs, but stronger. Each leg has five toes, with curved claws; these are webbed, but the web does not extend beyond the points of the toes. The four outer toes are at equal distances from each other; but the inner one is at a much greater distance from the one next it. The under surface of the foot is defended by a strong cuticular covering.

In the male, just at the setting on of the heel, there is a strong crooked spur, $\frac{1}{2}$ an inch long, with a sharp point, which has a joint between it and the foot, and is capable of motion in two directions. When the point of it is brought close to the leg, the spur is almost completely concealed among the hair; when directed outwards, it projects considerably, and is very conspicuous. It is probably by means of these spurs or hooks, that the female is kept from withdrawing herself in the act of copulation; since they are very conveniently placed for laying hold of her body on that particular occasion. The female has no spur of this kind.

The tail, in its general shape, is very similar to that of the beaver. The hair upon its upper surface is long and strong; it has a coarse appearance. The under surface, if superficially examined, appears to have no hair; but, when more closely inspected, is found to be covered with short straggling hairs.

Description of the internal Parts.

The panniculus carnosus, which lies immediately under the skin, and extends over the greatest part of the body, is exceedingly strong.

The tongue is two inches long; it lies in the hollow between the two jaws, but does not project any way into the bill, being confined to its situation, except a very small portion at the tip. It is smallest at the point, and becomes larger towards the root; the posterior portion becomes very large, and rises considerably higher than the rest, forming a projection, on the anterior part of which are the two small teeth already mentioned. The tongue is covered with short cuticular papillæ, the points of which are directed backwards.

The velum pendulum of the palate is very broad. The glottis is uncommonly narrow; and the epiglottis proportionally small. The rings of the trachea are broad for their size; they do not meet behind, but nearly so. The tongue and epiglottis are represented in Plate II. Fig. 2.

In the structure of the bones of the chest, there are some peculiarities which deserve notice.

The ribs are sixteen in number: the six superior are united to the sternum, which is narrow and very moveable; the other ten terminate anteriorly in broad, flattened, oval, bony plates, which overlap each other in the contracted state of the chest, and are united together by a very elastic ligamentous substance, which admits of their being pulled to some distance; so that the capacity of the chest can undergo a very unusual degree of change.

The ribs are not connected to the sternum by their cartilages, as in other quadrupeds, but by bone; the cartilaginous portion being only about an inch long, and situated at some distance from the sternum, between two portions of rib, forming a kind of joint at that part. There is no ensiform cartilage.

On the upper end of the sternum is a bone an inch long, which at its upper part has two processes that answer the purpose of clavicles, and unite with the upper part of the scapulæ, keeping them at a proper distance. The scapulæ have a very unusual shape: the posterior part is more like the imperfect scapula in the bird; and the flat part is situated with one edge under the bone, immediately above the sternum. The other edge forms the glenoid cavity, for the articulation of the os humeri; so that the fore legs have their connection with the trunk more forward than in other quadrupeds; and the scapula itself is much more firmly confined to its situation.

This bone above the sternum, with the anterior part of the two scapulæ, forms a bony covering of some strength, under which pass the great blood-vessels of the neck, secured from compression.

The appearance of the ribs, sternum, and other bones, is represented in Plate III.

The heart is situated in the middle line of the chest, its apex pointing to the sternum, and is inclosed in a strong pericardium: it is made up of two auricles and two ventricles. The foramen ovale between the auricles was closed, nor was there any communication between the ventricles. The right auricle is very large, and has two ascending venæ cavæ; that to the left winding round the basis of the heart, and forming the subclavian and jugular vein of that side, after giving off the vena azygos. This is similar to the kangaroo, beaver, otter, and many other animals. The aorta and other arteries are small.

The lungs are large in size, corresponding to the capacity of the chest. On the right side there are two lobes; there is a small azygos lobe under the heart; and in the left side only one. Instead of a portion of the lungs being above the heart, as in other animals, the heart may be said to be above the lungs; for they only embrace its sides, and do not surround its upper surface, but extend downwards, into the more moveable part of the cavity of the chest.

The diaphragm is very broad, and every where towards the circumference is muscular, having only a small central portion, which is tendinous, immediately under the heart.

The œsophagus is extremely small, more particularly at its origin behind the larynx, where the fauces terminate in it.

The stomach is a membranous bag, of an oval form, into which the œsophagus can hardly be said to enter, being rather continued along one end of the oval, till it forms the duodenum; so that the stomach appears to be a lateral dilatation of a canal, which is the œsophagus where the dilatation is formed, and becomes the duodenum immediately afterwards, at which part the coats are thickened, forming the valve of the pylorus.

The stomach is smaller than in most other animals; in this respect it is like the true stomach of birds. In the collapsed state it is only 1½ inch long, and ¾ of an inch broad. This is exactly double the size of one of the pouches in the cheek.

The duodenum makes a turn in the right side of the abdomen; then crosses the spine, and becomes a loose intestine. The small intestines are strung upon a loose, broad, transparent mesentery. The origin of the colon is only to be distinguished by a small lateral appendage, 1½ inch long, and ¼ of an inch in diameter, going off from the side of the intestine, which is not altered in its size at this part. This process corresponds to the cæcum: it is unlike the cæcum in quadrupeds, but resembles that in birds, only is much smaller, and in general they have two; but the bittern and heron have only one. From this part, the colon passes up the left side, fixed to its situation by being attached to the omentum; then goes across the body, and becomes rectum, which gradually increases in size, and is very capacious before it terminates at the anus.

The small intestines are four feet four inches long. The colon and rectum are one foot four inches long.

The rectum opens externally at the root of the tail, 1½ inch below the pelvis. On each side of the anus is a large solid body, about the size of the testicle, which proves to be a gland, whose ducts open by several orifices into the rectum. In the female, the same glands are met with, but of a much smaller size.

The mesentery is free from fat; nor are there any fatty appendages, or longitudinal bands, on the colon. The mesenteric glands are of the size of millet-seeds; they are numerous, and scattered over the mesentery. The lacteals are small.

The internal surface of the stomach is uniformly smooth. The duodenum has valvulæ conniventes, which are transverse: these are not met with in the jejunum and ilium; but in them the internal membrane is studded over with glands. There is no appearance whatever of valve at the beginning of the colon; but there are ten dotted lines, which run in a longitudinal direction, at equal distances from one another, and have their origin at the orifice of the cæcum: these dots, upon a close inspection, prove to be the projecting orifices of ducts belonging to the glands of the intestine. The cavity of the small cæcum is very cellular, as is shown in Plate II. Fig. 3.

The omentum is a thin transparent membrane, without any fat in it, originating from the side of the stomach next the duodenum, and also from that intestine anteriorly: on the left side it hangs loose, and the spleen is connected to it; but, on the right, after it reaches the colon, it surrounds that gut, and returns to the spine; so that although the colon is confined by the omentum, there is no part of that membranous bag projecting beyond it.

The liver is composed of four lobes, besides the small lobe or lobulus Spigelii. The gall-bladder is in the usual situation, and of the common size. The cystic and hepatic ducts unite into one, and are joined by the pancreatic duct before their termination in the duodenum, which is about an inch from the pylorus.

The pancreas is spread upon the great and little omentum, as in the sea-otter, and is made up of small parts, in a very similar manner.

The spleen consists of two very long slender bodies, united together at one end for the length of half an inch: one of these portions is six inches, the other four inches long.

The kidnies are conglobate, and lie in the usual situation. The capsulæ renales are rather small. The ureters are pellucid and small.

The urinary bladder is not situated in the pelvis, but just above it, in the cavity of the abdomen, and is attached to the peritonæum lining the abdominal muscles.

The skull is rather flattened upon the upper surface: its cavity is capacious; and there is a bony process projecting from the cranium, in the place of the falx of the dura mater. This, I believe, is not the case in any other quadruped: it is met with in some birds in a less degree, as in the parrot and the spoonbill; which last bird, in the structure of its beak, bears some analogy to this animal. The tentorium is entirely membranous.

The brain was not in a state to admit of its structure being accurately examined; but it appears to be made up of the same parts as those of quadrupeds in general.

The olfactory nerves are small, and so are the optic nerves; but the fifth pair, which supplies the muscles of the face, are uncommonly large. We should be led, from this circumstance, to believe that the sensibility of the different parts of the bill is very great, and therefore that it answers the purpose of a hand, and is capable of nice discrimination in its feeling.*

The eye is very small, and is nearly spherical: the globe is about $\frac{1}{4}$ of an inch in diameter; the cornea $\frac{3}{16}$ of an inch in diameter. There is a membrana nictitans; and the eyelid is very loose upon the eyeball; it is probably capable of great dilatation and contraction.

The organ of smell, in its construction resembles that of other quadrupeds, and may be said to consist of two turbinated bones in each nostril; that next the bill is the largest, and has the long axis in the direction of the nostril; its external surface is very irregular. The posterior one is shorter, projects further into the nostril, and is situated transversely, with respect to the nostril. As the external openings of the nose are at the end of the bill, there is a canal of an unusual length for the air to pass through, before it is applied to the immediate organ, unless there is an extension of the branches of the olfactory nerve upon the linings of the cavity, so as to make it a part of it. The external

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* The same observations were made by Professor BLUMENBACH, of Gottingen, who first dissected these nerves.

opening of the ear is at a great distance from the organ; and there is a cartilaginous canal, the size of a crow-quill, winding round the side of the head, upon the outside of the temporal muscle, leading to the orifice in the temporal bone.

The membrana tympani is larger than in other quadrupeds of the same size: it is of an oval form; and the central part is drawn in, making its external surface concave. It has only two bones; one passing directly from the membrane towards the foramen ovale, upon which there is a second bone, imperfectly resembling the stapes, having a flat surface of a circular form upon the orifice, and a small neck, by which it is united to the other bone.

This structure of the bones is less perfect or complex than in other quadrupeds; so that the organ altogether bears a greater resemblance to that of the bird.

The organs of generation in this animal have several peculiarities of a very extraordinary nature.

The male organs do not appear externally; so that the distinguishing mark of the sex is the spur on the hind leg.

The testicles are situated in the cavity of the abdomen, immediately below the kidneys: they are large for the size of the animal. The epididymis is connected to the body of the testicle by a broad membrane, which admits of its lying very loose.

The penis in this animal does not, as in other quadrupeds, give passage to the urine. It is entirely appropriated to the purpose of conveying the semen; and a distinct canal conducts the urine into the rectum, by an opening about an inch from the external orifice of the intestine. The gut, at this part, is defended from the acrimony of the urine, by the mucus secreted by two glands already described, which probably for this reason are very large in the male, but small in the female. The opening of the meatus urinarius, and the orifices of the glands, are represented in Plate IV.

The penis is short and small in its relaxed state; and its body does not appear capable of being very much enlarged when erected. The prepuce is a fold of the internal membrane of the verge of the anus, as in the bird; and the penis, when retracted, is entirely concealed.

The glans penis is double; one glans having its extremity directed to the right, the other to the left; and, as they supply two distinct cavities with semen, they may be considered as two penises. This is an approach to the bird, many of which have two. Each glans has, at its extremity, pointed conical papillæ, surrounding a central depression. In one glans, the papillæ are five in number, in the other four. When the urethra is laid open from the bladder into the rectum, about half an inch from its termination it communicates with the proper urethra of the penis, which afterwards divides into two, one going to each glans, in the centre of which is a cavity communicating directly with the papillæ, the points of which are pervious, forming the orifices by which the semen is evacuated.

The vasa deferentia open into the membranous part of the urethra, before it comes to the root of the penis.

Not being aware of so extraordinary a structure, and the parts not being in a perfect state of preservation, they were too much injured by dissection before it was discovered, to admit of their being prepared by injection. The appearance of these parts is delineated in Plate IV.

There was no appearance of vesiculæ seminales.

The female organs open into the rectum, as in the bird. Just within the anus there is a valvular projection, between the rectum and vagina, which appears to be the proper termination of the rectum. This also is similar to the bird.

There was no appearance of clitoris, that could be observed.

The vagina is 1½ inch long: its internal membrane is rugous; the rugæ being in a longitudinal direction. At the end of the vagina, instead of an os tincæ, as in other quadrupeds, is the meatus urinarius; on each side of which is an opening leading into a cavity, resembling the horn of the uterus in the quadruped, only thinner in its coats. Each of these cavities terminates in a fallopian tube, which opens into the capsule of an ovarium.

The ovaria are very small: they were not in a very perfect state of preservation, but bore a general resemblance to those of other quadrupeds.

This structure of the female organs is unlike any thing hitherto met with in quadrupeds; since, in all of them that I have examined, there is the body of the uterus, from which the horns go off, as appendages. The opossum differs from all other animals in the structure of these parts, but has a perfectly formed uterus; nor can I suppose it wanting in any of the class Mammalia.

This animal having no nipples, and no regularly formed uterus, led me to examine the female organs in birds, to see if there was any analogy between the oviducts in any of that class, and the two membranous uteri of this animal; but none could be observed; nor would it be easy to explain how an egg could lie in the vagina, to receive its shell, as the urine from the bladder must pass directly over it. Finding they had no resemblance to the oviducts in birds, I was led to compare them with the uteri of those lizards which form an egg, that is afterwards deposited in a cavity corresponding to the uterus of other animals, where it is hatched; which lizards may therefore be called ovi-viviparous; and I find a very close resemblance between them. In these lizards there are two uteri, that open into one common canal or vagina, which is extremely short; and the meatus urinarius is situated between these openings. The coats of these uteri are thinner than those of the uteri of quadrupeds of the same size.

In the ovi-viviparous dog-fish, the internal organs of the female have a very similar structure. There is therefore every reason to believe, that this animal also is ovi-viviparous in its mode of generation.

EXPLANATION OF THE DRAWINGS.

See PLATES II. III. and IV.

PLATE II.

Fig. 1. Represents the hind leg of the male, in order to shew the situation and appearance of the spur.

Fig. 2. Represents the tongue, in its natural situation; and shows its relative position to the grinding teeth, and the lower portion of the bill; also the two pointed teeth upon the tongue itself.

On the outside of the jaw, on each side, are the pouches for the food.

The glottis, epiglottis, and œsophagus, are represented of the natural size.

Fig. 3. The loculated cæcum, with a portion of the ilium and colon.

PLATE III.

Represents the bones of the chest and pelvis, in their relative situation, to show the uncommon shape of the scapulæ, which are not connected with the chest, but with a bone placed above the sternum, the upper part of which answers the purpose of clavicles; the anterior part of each scapula passes under this bone laterally, forming with it a bony case for this part of the neck.

Another peculiarity is, the cartilages of the ribs not being placed next the sternum, but between two portions of the rib. The false ribs have their cartilages terminated by thin bony scales, which slide on one another in the motions of the chest.

The pelvis is unusually small, and has the two moveable bones, attached to the os pubis, which are met with in the kangaroo.

a a a. The bone which corresponds to the clavicles in other animals.

b b b. The left scapula.

c c c. The bony scales along the margin of the chest.

d d d. The cartilages of the true ribs.

e e. The moveable bones of the pelvis.

PLATE IV.

Fig. 1. Represents the penis in a relaxed state, but drawn out to its full extent, with its relative situation to the rectum and testicles, which are contained in the cavity of the abdomen.

a a. The bodies of the testicles.

b b. The epididymis.

c. The urinary bladder.

d d. The rectum.

e e. Two glands, whose ducts enter the rectum by a number of small orifices.

f. The body of the penis, whose external covering is a continuation of the lining of the lower part of the rectum.

gg. The double glans: at the point of the right one are five conical papillæ, and at the point of the left only four, which are open at their extremities; through these orifices the semen passes.

h. The opening of the urethra into the rectum.

Fig. 2. A view of the uteri and vagina.

aa. The vestibulum, common to the rectum and vagina.

bb. The cut edges of the rectum; the gut being dissected off to expose the vagina.

c. The vagina.

d. The meatus urinarius.

e. The bladder.

ff. The orifices leading to the uteri.

gg. The two uteri.

hh. The fallopian tubes.

ii. The ovaria, enclosed in the capsules.

Philos. Trans. MDCCCII. Plate II. p. 84.

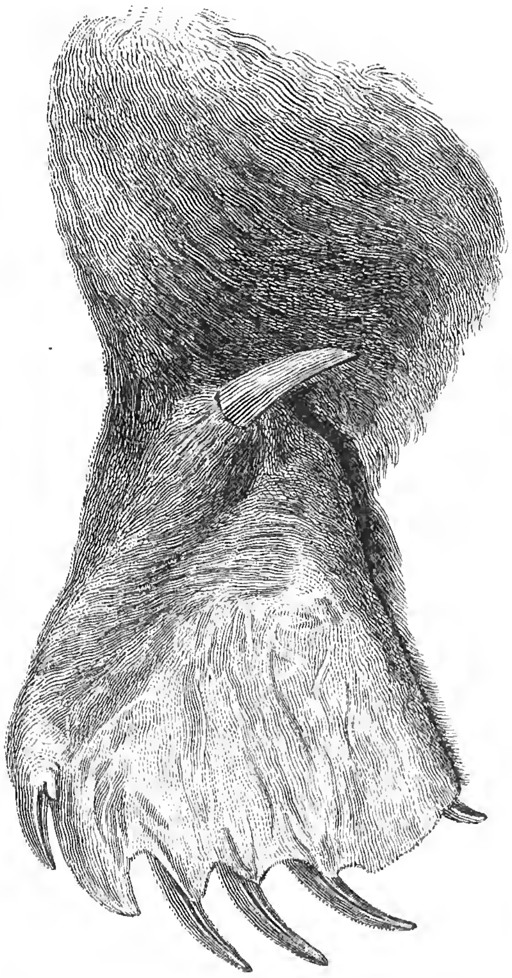

Fig. 3.

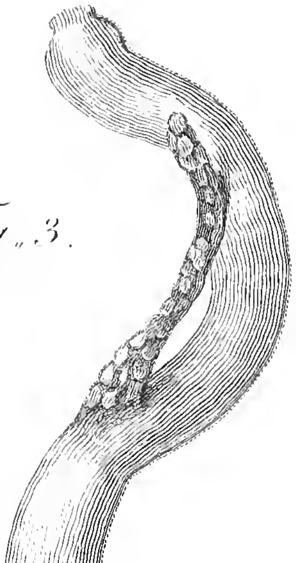

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Fig. 1.

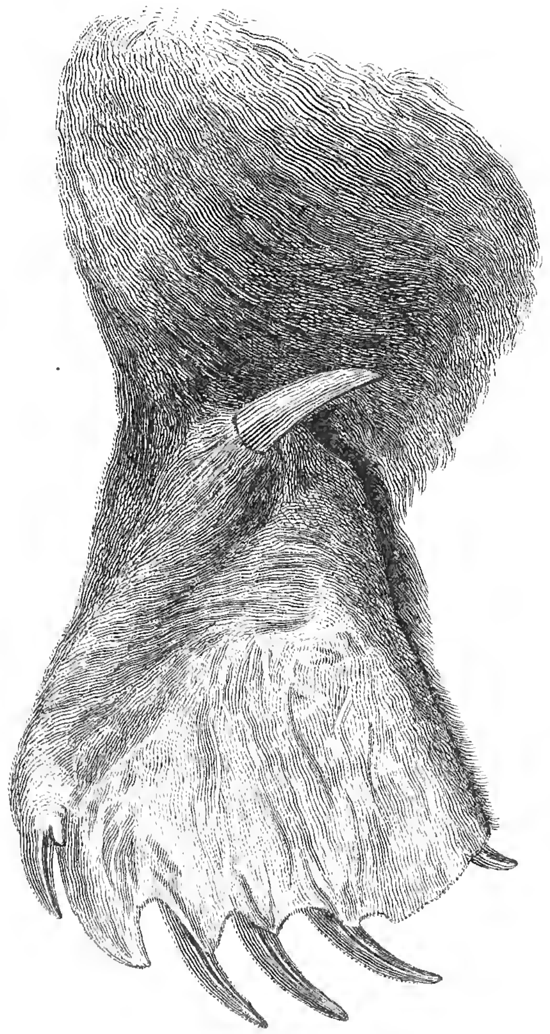

Fig. 2.

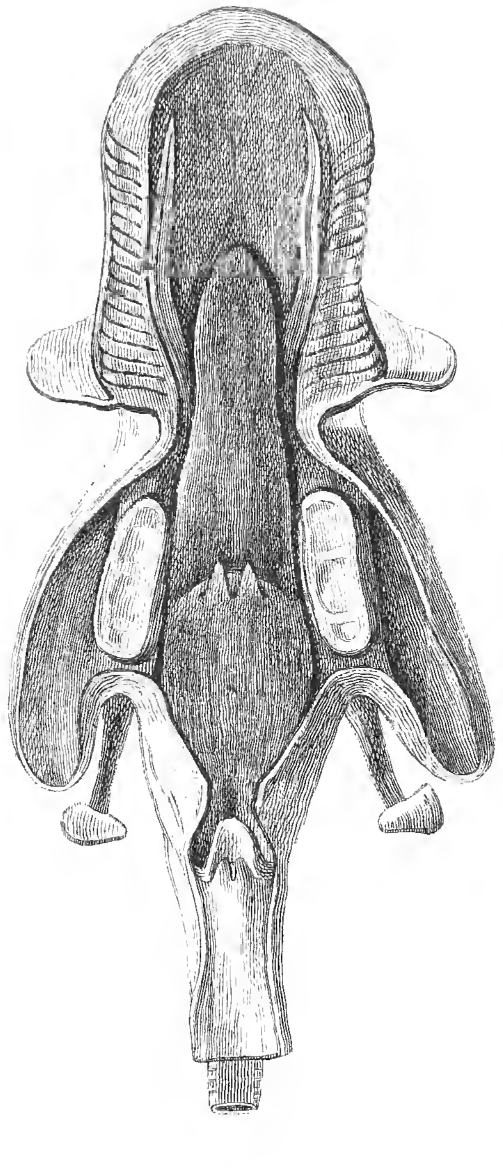

Fig. 3.

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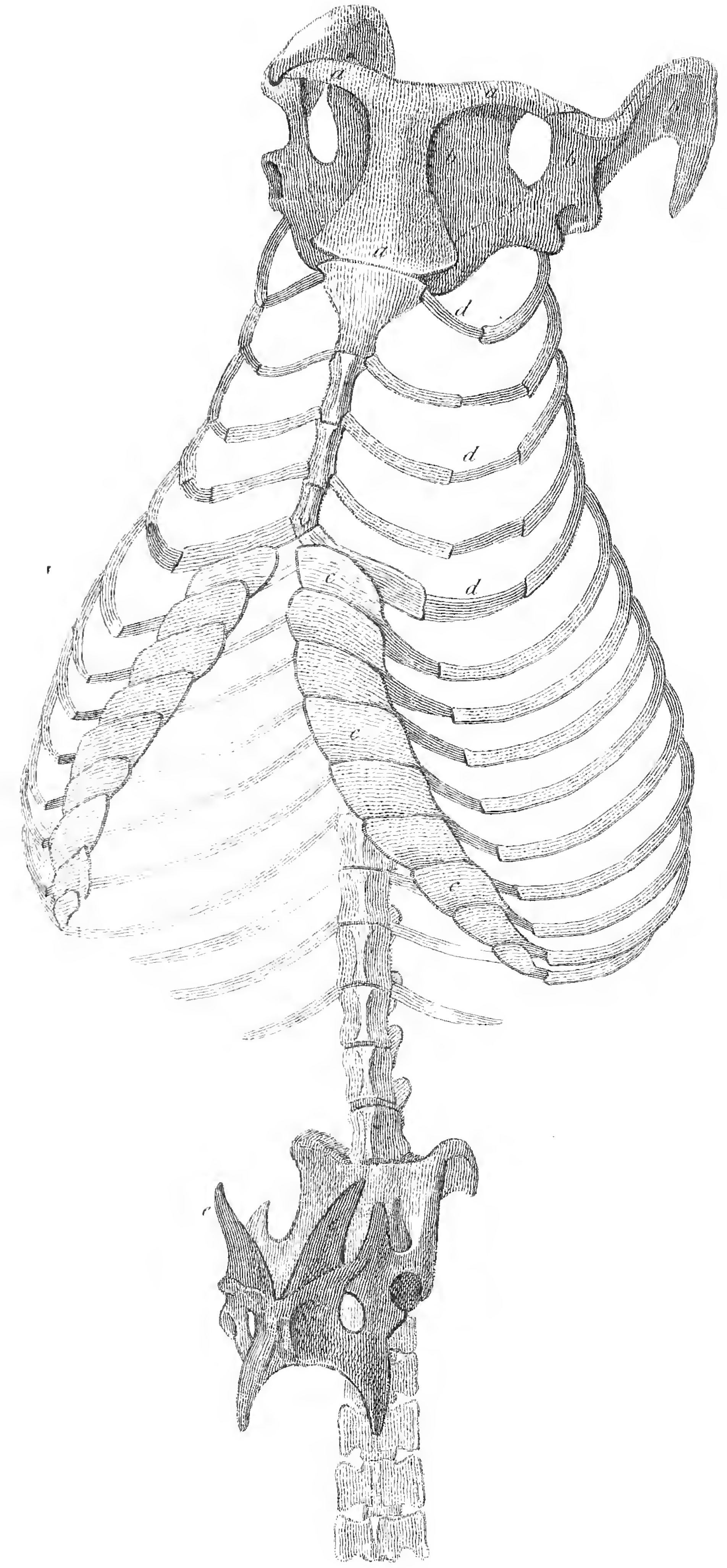

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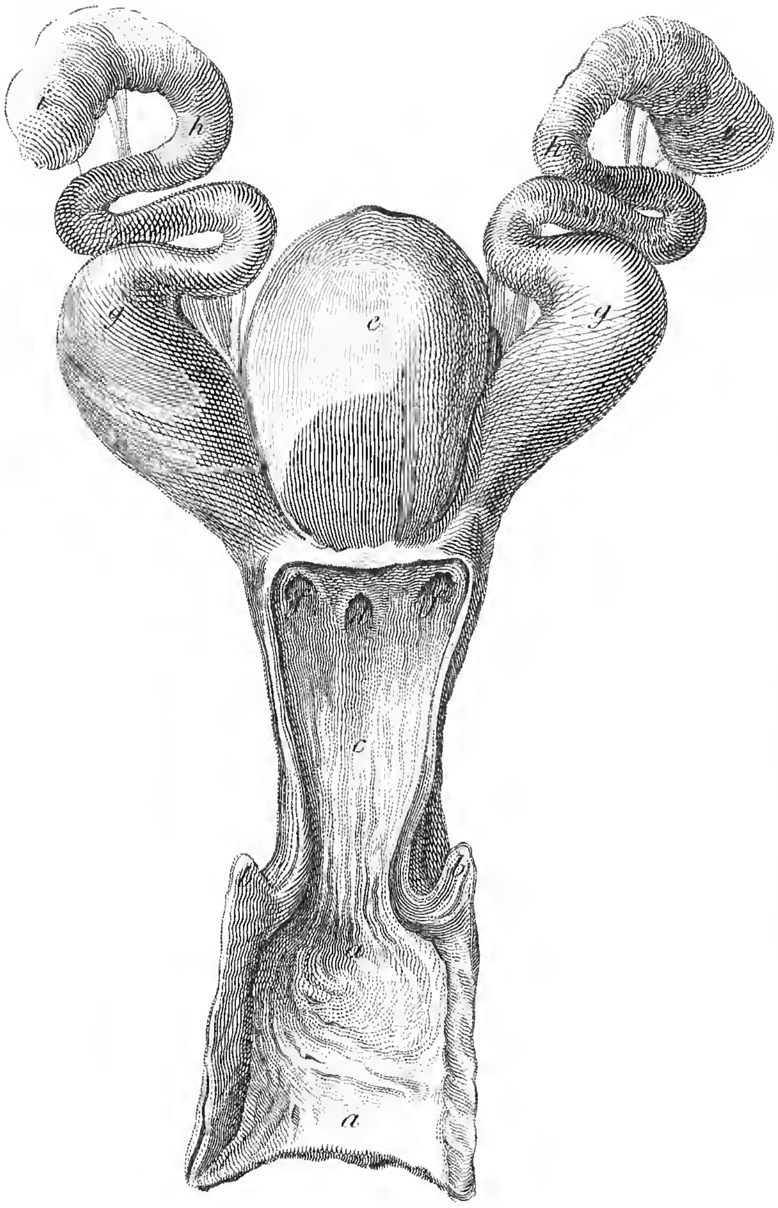
Fig. 2.



Basire, sc.

Fig. 1.

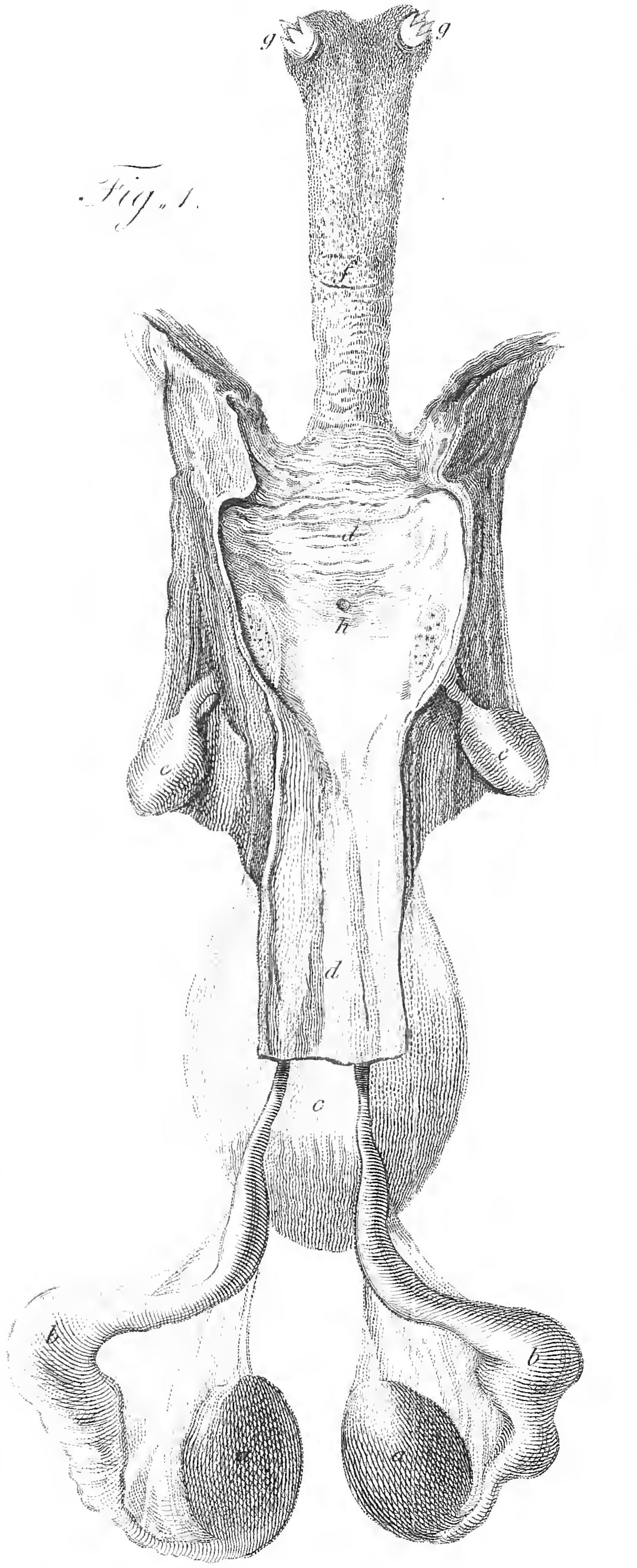
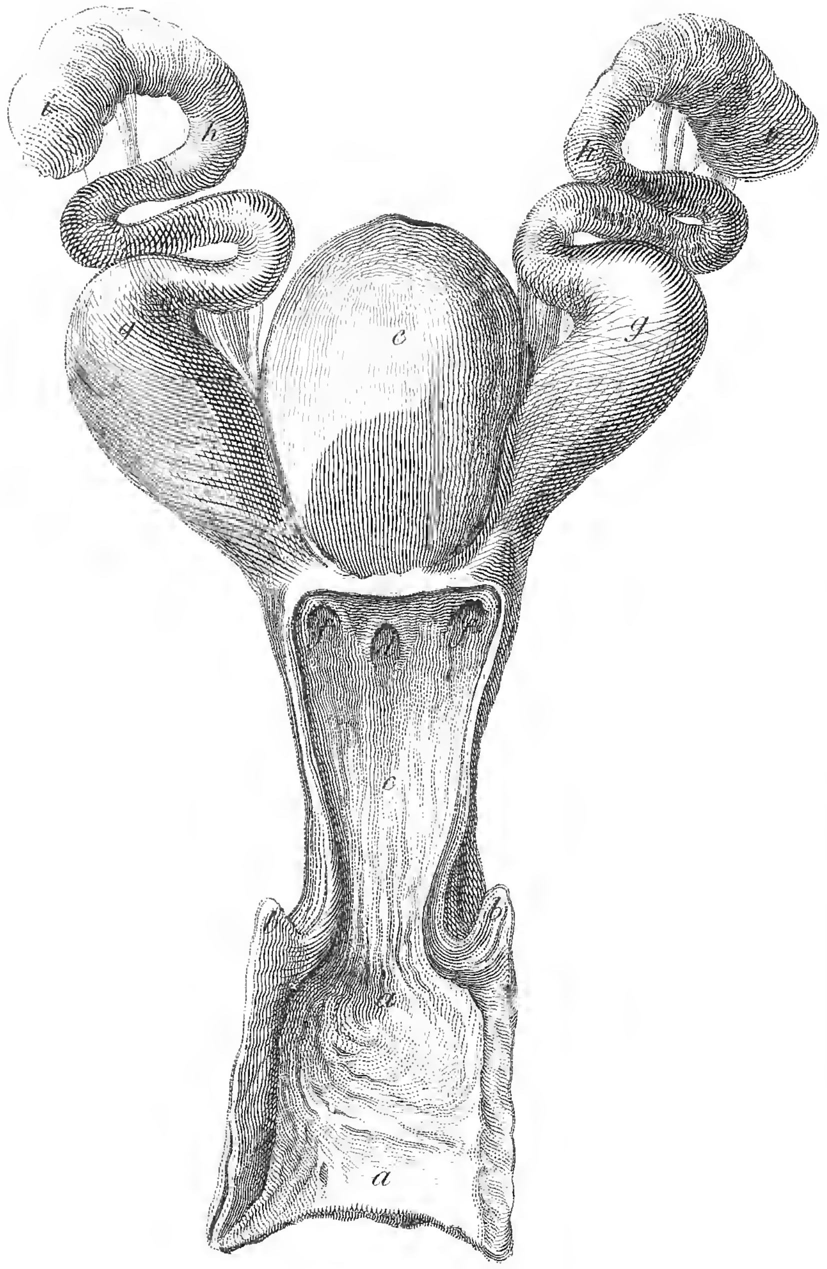


Fig. 2.



Will.m Clift del.

Basire sc.

V. *On the Independence of the analytical and geometrical Methods of Investigation; and on the Advantages to be derived from their Separation. By* Robert Woodhouse, *A. M. Fellow of Caius College,* Cambridge. *Communicated by* Joseph Planta, *Esq. Sec. R. S.*

Read January 14, 1802.

One of the objects of the paper which last year I had the honour of presenting to the Royal Society, was to shew the insufficiency in mathematical reasoning, of a principle of analogy, by which the properties demonstrated for one figure were to be transferred to another, to which the former was supposed to bear a resemblance; and the argument for the insufficiency of the principle was this, that the analogy between the two figures was neither antecedent to calculation, nor independent of it, and consequently could not regulate it; that analogy was the object of investigation, not the guide; the result of demonstration, not its directing principle.

Having shewn that analogy could not establish the truth of certain mathematical conclusions, I next endeavoured to shew why such conclusions had been rightly inferred; not by proposing any new excogitated principle, nor by pointing out an hitherto unobserved intellectual process; but I conceived they might be obtained by operations conducted in a manner similar to that by which all reasoning with general terms is conducted, and that the relations between the symbols or general terms were to be established by giving the true meaning to the connecting signs, which indicate not so much the arithmetical computation of quantities, as certain algebraical operations. It was further observed, that, from certain established formulas, abridged symbols or general terms might be formed, which consequently must have their signification dependent on such formulas; and that, although the parts of certain abridged expressions could not separately be arithmetically computed, yet the expressions themselves might be legitimately employed in all algebraic operations.

The chief object of my paper was to shew, that operations with imaginary quantities, as they are called, were strictly and logically conducted, that is, conducted after the same manner as operations with quantities that can be arithmetically computed: the question, whether calculation with imaginary symbols is commodious or not, was then slightly discussed. I have since attentively considered it, and, what usually happens in such cases, my inquiries have been extended beyond their original object; for, actual research has convinced me of what there were antecedent reasons for suspecting, that not only in the theory of angular functions, demonstration is most easy and direct by giving to quantities their true and natural* representation; but, that the introduction of expressions and formulas not analytical, into analytical investigation, has caused much ambiguity, confused notion, and paradox; that it has made

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* $(2^{-1}) . \left\{ \varepsilon^{x\sqrt{-1}} + \varepsilon^{-x\sqrt{-1}} \right\}$, $(2\sqrt{-1})^{-1} . \left\{ \varepsilon^{x\sqrt{-1}} - \varepsilon^{-x\sqrt{-1}} \right\}$ &c. I call the *natural* representations of the cosines, sines, &c. of an arc x; because, admitting the algebraical notation, they, by strict inference, adequately, unambiguously, and solely, represent the cosines, sines, &c.

demonstration prolix, by rendering it less direct, and has made it deficient in precision and exactness, by diverting the mind from the true source and derivation of analytical expression.

The expressions and formulas alluded to are geometrical, that is, taken from the language of geometry, and established by its rules: they are to be found mixed with analytical* expressions and reasonings, in all works on abstract science; and, as they are certainly foreign and circumlocutory, if it can be shewn that they are not essentially necessary, there will exist an argument for their exclusion, especially if it appears that in analytical investigation they are productive of the evils above mentioned.

That, in algebraical calculation, geometrical expressions and formulas are not essentially necessary, perhaps this short and easy consideration may convince us; since algebra is an universal language, it ought surely to be competent to express the conditions belonging to any subject of inquiry; and, if adequate expressions be obtained, then there is no doubt that with such, reasoning or deduction may be carried on.

All expressions and formulas, such as, sin. x, cos. x, hyp. log. x, sin. $n x = 2$ cos. x. sin. $(n-1)\ x -$ sin. $(n-2)\ x$,

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* The terms analysis, analytical, algebra, algebraical, have been so often distinguished, and so often confounded, that I shall not take the trouble again to distinguish them. I use the words analytical, algebraical, indifferently, in contradistinction to geometrical. The first relates to an arbitrary system of characters; the latter to a system of signs, that are supposed to bear a resemblance to the things signified, and in which system, lines and diagrams are used as the representatives of quantity: and I am principally induced to use the words indifferently, because, if analytical were properly defined, another word with a sufficient extent of meaning could not be found; for, by an improper limitation, the word algebraical has not an extensive signification, being frequently used in contradistinction to transcendental, exponential, &c.

$\int x^{\cdot} (1 - x^2)^{\frac{1}{2}} =$ circular arc, $\int x^{\cdot} \sqrt{\frac{1 + n^2 x^2}{1 - x^2}} =$ elliptical arc, &c. are geometrical, or involve geometrical language: they suppose the existence of a particular system of signs, and method of deduction; and relate to certain theorems, established conformably to such system and method.

I. Sin. x, cos. x, tang. x, &c. These expressions are borrowed from geometry; but, analytically, denote certain functions of x. Typographically considered, these expressions are more commodious than $(2\sqrt{-1})^{-1} \left\{ \varepsilon^{x\sqrt{-1}} - \varepsilon^{-x\sqrt{-1}} \right\}$, $(2)^{-1} \left\{ \varepsilon^{x\sqrt{-1}} + \varepsilon^{-x\sqrt{-1}} \right\}$, &c. but this is the sole advantage; for, all analytical operations with these latter signs are much easier, and more expeditious, than with the former; since they are carried on after a manner analogous to that by which operations with similar expressions are. But, if the geometrical expressions be retained, then, in order to calculate with them, recourse must be had to the geometrical method, proceeding by the similarity of triangles, the doctrines of proportions, and of prime and ultimate ratios; so that, in the same investigation, two methods of deduction, between which there is no similarity, must be employed.

II. The value of $\int x^{\cdot} (1 + x)^{-1}$, is said to be a portion of the area of an hyperbola intercepted between two ordinates to its assymptotes; but this is a foreign and circumlocutory mode of expression; since, to find the value of the area, $x^{\cdot} . (1 + x)^{-1}$ must be expanded, and the integrals of the several terms taken; and this same operation must have taken place, in order to approximate to the value of $\int x^{\cdot} (1 + x)^{-1}$, if no such curve as the hyperbola had ever been invented.

III. $\int x^{\cdot} \left\{ 1 - x^2 \right\}^{-\frac{1}{2}}$ is said to equal the arc of the circle rads. 1, sin. x; but nothing is gained by this; since, in order to find the arc of a circle, $x\cdot\,(1-x^2)^{-\frac{1}{2}}$ is expanded, and the integrals of the several parts taken and added together. To shew (if it is necessary to add any thing more on so clear a point) that $\int x\cdot\,\left\{1-x^2\right\}^{-\frac{1}{2}} =$ arc circle, is merely a mode of expression borrowed from geometry; suppose the investigation of the properties of motion to have been prior to the investigation of the properties of extension, for, that the science of geometry was first invented is properly an accidental circumstance, then, such an expression as $\int x\cdot\,\left\{1-x^2\right\}^{-\frac{1}{2}}$ might have occurred, and its value must have been exhibited as it *really* is now, that is, by expanding it, and integrating the several terms.

IV. It is an objection certainly against these modes of expression, that they are foreign, and tend to produce confused and erroneous notions; for the student may be led by them to believe, that the determination of the values of certain analytical expressions, essentially require the existence of certain curves, and the investigation of their properties. But there is a more valid objection against them, which is, that they divert the mind from the true derivation of such expressions as $\frac{x\cdot}{x}$, $x\cdot\,(1-x^2)^{-\frac{1}{2}}$, &c. and consequently tend to produce ambiguity and indirect methods; for although, in order to obtain approximately the numerical value of $\int\frac{x\cdot}{x}$, $\int x\cdot\,(1-x^2)^{-\frac{1}{2}}$, &c. it is convenient to expand the expressions, and to take the integrals of the resulting terms, yet, if the symbol $\int$ denotes a reverse operation, $\int\frac{x\cdot}{x}$, $\int x\cdot\,(1-x^2)^{-\frac{1}{2}}$ are not properly and by strict inference equal to $(x-1) - \frac{1}{2}\left\{x-1\right)^2 + \frac{1}{3}\left\{x-1\right)^3 -$, &c. and $x + \frac{x^3}{2.3} + \frac{3x^5}{5.8} +$, &c. But, in order to explain clearly what I mean, it is necessary to state what I understand by the integral or fluent of an expression.

V. Let φx denote a function of x; if x be increased by o, then φx becomes $\varphi(x+o)$, and $\varphi(x+o)$, developed according to the powers of o, becomes $\varphi x + \mathrm{P}o + \frac{\mathrm{Q}}{1.2}o^2 + \frac{\mathrm{R}}{1.2.3}o^3$, &c. where P is derived from φx, Q from P, R from Q, &c. by the same law; so that the manner of deriving P being known, Q, R, &c. are known. The entire difference or increment of φx is $\varphi(x+o) - \varphi x$; the differential or fluxion of φx is only a part of the difference or $\mathrm{P}.o$. If, instead of o, dx, or $x^{\cdot}$, be put, it is $\mathrm{P}.dx$ or $\mathrm{P}x^{\cdot}$; the integral or fluent of $\mathrm{P}x^{\cdot}$ is that function from which $\mathrm{P}x^{\cdot}$ is derived; and, in order to remount to it, we must observe the manner or the operation by which it was deduced; and, by reversing such operation, the integral or fluent is obtained. Now, in taking the fluxion of certain functions of x, it appears there are conditions to which the indices of x without and under the vinculum are subject: hence, whether or not a proposed fluxion can have its fluent assigned, we must see if the fluxion has the necessary conditions. Expressions such as $\frac{x^{\cdot}}{x}$, $\frac{x^{\cdot}}{1+x}$, $\frac{x^{\cdot}}{\sqrt{1-x^2}}$, &c. have not these conditions; and consequently there is no function φx of x, such that the second term of the developement of $\varphi(x+x^{\cdot})$ is equal either to $\frac{x^{\cdot}}{x}$, or $\frac{x^{\cdot}}{1+x}$, or $\frac{x^{\cdot}}{\sqrt{1-x^2}}$, or, &c. There are, however, integral equations from which such expressions may be derived. Thus, let $x = \varepsilon^z$, then $\frac{x^{\cdot}}{x} = z^{\cdot}$, let $1 + x = \varepsilon^z \therefore \frac{x^{\cdot}}{1+x} = z^{\cdot}$, let $x = \frac{\varepsilon^{z\sqrt{-1}} - \varepsilon^{-z\sqrt{-1}}}{2\sqrt{-1}} \therefore \frac{x^{\cdot}}{\sqrt{1-x^2}} = z^{\cdot}$.

Now, from these equations, the differential equations $\frac{x^{\cdot}}{x} = z^{\cdot}$, $\frac{x^{\cdot}}{1+x} = z^{\cdot}$, $\frac{x^{\cdot}}{\sqrt{1-x^2}}$, &c. may, by expunging the exponential quantities, be derived; consequently, if the symbol $\int$ is to designate a reverse operation, I can only know what that reverse operation is, by attending to the manner by which the expressions affected with the symbol $\int$ were derived. Hence,

VI. $\int \frac{x^{\cdot}}{x} = z$ when $x = \varepsilon^{z}$.

$\int \frac{x^{\cdot}}{1+x} = z$ when $1 + x = \varepsilon^{z}$.

$\int \frac{x^{\cdot}}{\sqrt{1-x^2}} = z$ when $x = (2\sqrt{-1})^{-1}\{\varepsilon^{z\sqrt{-1}} - \varepsilon^{-z\sqrt{-1}}\}$.

In like manner,

$\int x^{\cdot}\,(1+x^2)^{-\frac{1}{2}} = z$, $x + \sqrt{1+x^2} = \varepsilon^{z}$ or $x = \frac{\varepsilon^{z} - \varepsilon^{-z}}{2}$.*

$\int x^{\cdot}\,(2x+x^2)^{-\frac{1}{2}} = z$, $1 + x + \sqrt{2x+x^2} = \varepsilon^{z}$.

$\int \frac{2x^{\cdot}}{1-x^2} = z$, $\frac{1+x}{1-x} = \varepsilon^{z}$ or $x = \frac{\varepsilon^{z}-1}{\varepsilon^{z}+1}$.

$\int \frac{2x^{\cdot}}{x\sqrt{1+x^2}} = z$, $\frac{\sqrt{1+x^2}-1}{\sqrt{1+x^2}+1} = \varepsilon^{z}$ or $\sqrt{1+x^2} = \frac{1+\varepsilon^{z}}{1-\varepsilon^{z}}$,

or $x = \frac{2}{\varepsilon^{-\frac{z}{2}} - \varepsilon^{\frac{z}{2}}}$.

Again, suppose

$x = \{2\sqrt{-1}\}^{-1}\{\varepsilon^{z\sqrt{-1}} - \varepsilon^{-z\sqrt{-1}}\} \therefore x^{\cdot} = 2^{-1}z^{\cdot}\{\varepsilon^{z\sqrt{-1}} + \varepsilon^{z\sqrt{-1}}\}$,

but $\sqrt{1-x^2} = 2^{-1}, (\varepsilon^{z\sqrt{-1}} + \varepsilon^{-z\sqrt{-1}}\}$; consequently $x^{\cdot} = z^{\cdot}\sqrt{1-x^2}$,

or $z^{\cdot} = \frac{x^{\cdot}}{\sqrt{1-x^2}}$: hence, reversely,

$\int \frac{x^{\cdot}}{\sqrt{1-x^2}} = z$, x being $= (2\sqrt{-1})^{-1}.\{\varepsilon^{z\sqrt{-1}} - \varepsilon^{-z\sqrt{-1}}\}$.

In like manner,

$\int \frac{-x^{\cdot}}{\sqrt{1-x^2}} = z$, $x = 2^{-1}.\{\varepsilon^{z\sqrt{-1}} + \varepsilon^{-z\sqrt{-1}}\}$.

$\int \frac{x^{\cdot}}{\sqrt{2x-x^2}} = z$, $x = (1 - 2^{-1}.\{\varepsilon^{z\sqrt{-1}} + \varepsilon^{-z\sqrt{-1}}\})$.

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* I take no notice, at present, of the arbitrary quantities which may be introduced in the integration of these equations.

$$\int \frac{x^{\cdot}}{1-x^2} = z,\ x = \frac{\varepsilon^{z\sqrt{-1}} - \varepsilon^{-z\sqrt{-1}}}{\sqrt{-1}\left\{\varepsilon^{z\sqrt{-1}} + \varepsilon^{-z\sqrt{-1}}\right\}},\ \text{or} \frac{\varepsilon^{2z\sqrt{-1}} - 1}{\sqrt{-1}\,(\varepsilon^{2z\sqrt{-1}} + 1)}.$$

$$\int \frac{x^{\cdot}}{x\sqrt{x^{\cdot}-1}} = z,\quad x = \frac{2}{\varepsilon^{z\sqrt{-1}} + \varepsilon^{-z\sqrt{-1}}}.$$

And a variety of forms may be obtained, by substituting for x different functions of x, in the expression $\frac{x^{\cdot}}{\sqrt{1-x^2}}$. Hence, if the symbol $\int$ is made to denote a reverse operation, the integral equations of the preceding differential equations have been rightly assigned. All other methods of assigning the integrals, by the properties of logarithms, by circular arcs, by logarithmic and hyperbolic curves,* are indirect, foreign, and ambiguous.

VII. An instance or two will shew the advantage of adhering to the true and natural derivation of analytical expressions. Let x and y be the co-ordinates of a circle; then, $1 = x^2 + y^2$, and $y = \sqrt{(1-x^2)}$, now (arc)$^{\cdot}$ or $z^{\cdot} = \sqrt{(x^{\cdot 2} + y^{\cdot 2})}$ =, in this instance, $x^{\cdot}\,(1-x^2)^{-\frac{1}{2}}$: but it has appeared, that if $x = \left\{2\sqrt{-1}\right\}^{-1}\left\{\varepsilon^{z\sqrt{-1}} - \varepsilon^{-z\sqrt{-1}}\right\}$, $z^{\cdot} = x^{\cdot}\,(1-x^2)^{-\frac{1}{2}}$; consequently, in a circle, the co-ordinate x, or, in the language of trigonometry, the sine x = developement of

$$(2\sqrt{-1})^{-1}.\left\{\varepsilon^{z\sqrt{-1}} - \varepsilon^{-z\sqrt{-1}}\right\} = z - \frac{z^3}{1.2.3} + \frac{z^5}{1.2.3.4.5} - \&c.$$

and y or cosine $= 2^{-1}.\left\{\varepsilon^{z\sqrt{-1}} + \varepsilon^{-z\sqrt{-1}}\right\} = 1 - \frac{z^2}{1.2} + \frac{z^4}{1.2.3.4} - \&c.$

1. This method of determining the series for the sine in terms of the arc, is, I think, simple, direct, and exact; it requires no assumption of a series with indeterminate coefficients, nor

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* By the strange way of determining the meaning and value of analytical expressions from geometrical considerations, it should seem, as if certain curves were believed to have an existence independent of arbitrary appointment.

any preparatory process to shew that the value of the first coefficient must $= 1$.*

VIII. Euler demonstrated this formula to be true, viz. $\frac{\text{Arc}}{2} = \text{sin. arc} - \frac{1}{2}\,\text{sin. } 2\,\text{arc} + \frac{1}{3}\,\text{sin. } 3\,\text{arc} - \frac{1}{4}\,\text{sin. } 4\,\text{arc} + \&\text{c.}$ The following is its analytical deduction,

$$z^{\cdot} = z^{\cdot}\left\{\frac{\varepsilon^{z\sqrt{-1}}+1}{\varepsilon^{z\sqrt{-1}}+1}\right\} = z^{\cdot}\left\{\frac{\varepsilon^{z\sqrt{-1}}}{\varepsilon^{z\sqrt{-1}}+1}\right\} + z^{\cdot}\left\{\frac{1}{\varepsilon^{z\sqrt{-1}}+1}\right\}$$

$$= z^{\cdot}\left\{\frac{\varepsilon^{z\sqrt{-1}}}{\varepsilon^{z\sqrt{-1}}+1}\right\} + z^{\cdot}\left\{\frac{\varepsilon^{z-\sqrt{-1}}}{\varepsilon^{-z\sqrt{-1}}+1}\right\}$$

$$= z^{\cdot}\left\{\begin{array}{l} \varepsilon^{z\sqrt{-1}} - \varepsilon^{2z\sqrt{-1}} + \varepsilon^{3z\sqrt{-1}} - \&\text{c.} \\ + \varepsilon^{-z\sqrt{-1}} - \varepsilon^{-2z\sqrt{-1}} + \varepsilon^{-3z\sqrt{-1}} - \&\text{c.} \end{array}\right\}$$

$$\therefore z = \frac{1}{\sqrt{-1}}\left\{\begin{array}{l} \varepsilon^{z\sqrt{-1}} - \frac{\varepsilon^{2z\sqrt{-1}}}{2} + \frac{\varepsilon^{3z\sqrt{-1}}}{3} - \&\text{c.} \\ - \varepsilon^{-z\sqrt{-1}} + \frac{\varepsilon^{-2z\sqrt{-1}}}{2} - \frac{\varepsilon^{-3z\sqrt{-1}}}{3} + \&\text{c.} \end{array}\right\}$$

and $\frac{z}{2} = (2\sqrt{-1})^{-1}.\left\{\varepsilon^{z\sqrt{-1}} - \varepsilon^{-z\sqrt{-1}}\right\} - \frac{1}{2}(2\sqrt{-1})^{-1}. \left\{\varepsilon^{2z\sqrt{-1}} - \varepsilon^{-2z\sqrt{-1}}\right\}. + \frac{1}{3}(2\sqrt{-1})^{-1}\left\{\varepsilon^{3z\sqrt{-1}} - \varepsilon^{3z\sqrt{-1}}\right\} - \&\text{c.}$ which is the analytical translation of Euler's formula.

IX. Euler likewise shewed that

$$\text{sin. } x = 2^n.\ \text{cos. } \frac{x}{2}.\ \text{cos. } \frac{x}{4}.\ \text{cos. } \frac{x}{8} \ldots\ldots\ \text{cos. } \frac{x}{2^n} \cdot \text{sin. } \frac{x}{2^n}.$$

Which may be thus demonstrated,

$$\text{sin. } x = (2\sqrt{-1})^{-1}\left\{\varepsilon^{x\sqrt{-1}} - \varepsilon^{-x\sqrt{-1}}\right\};$$

but $(2\sqrt{-1})^{-1}\left\{\varepsilon^{x\sqrt{-1}} - \varepsilon^{-x\sqrt{-1}}\right\} = 2\,.\,2^{-1}\left\{\varepsilon^{\frac{x}{2}\sqrt{-1}} + \varepsilon^{-\frac{x}{2}\sqrt{-1}}\right\},$

$$(2\sqrt{-1}\}^{-1}\ \varepsilon^{\frac{x}{2}\sqrt{-1}} - \varepsilon^{-\frac{x}{2}\sqrt{-1}}\}$$

$$= 2\,.\,2^{-1}\left(\varepsilon^{\frac{x}{2}\sqrt{-1}} + \varepsilon^{-\frac{x}{2}\sqrt{-1}}\right\}.\ 2\,.\,2^{-1}\left(\varepsilon^{\frac{x}{4}\sqrt{-1}} + \varepsilon^{-\frac{x}{4}\sqrt{-1}}\right).$$

$$(2\sqrt{-1})^{-1}.\left\{\varepsilon^{\frac{x}{4}\sqrt{-1}} - \varepsilon^{-\frac{x}{4}\sqrt{-1}}\right\}$$

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* See Lagrange, Fonctions Analytiques. p. 26. Lacroix, Traité du Calcul. differentiel, &c. p. 56. Le Seur, Sur le Calcul. diff. p. 105. Euler, Anal. Inf. Art. 133, 134.

$$= 2\,.\,2^{-1}\left(\varepsilon^{\frac{x}{2}\sqrt{-1}}+\varepsilon^{-\frac{x}{2}\sqrt{-1}}\right).\,2\,.\,2^{-1}\left(\varepsilon^{\frac{x}{4}\sqrt{-1}}+\varepsilon^{-\frac{x}{4}\sqrt{-1}}\right)$$
$$.\,2\,.\,2^{-1}\left\{\varepsilon^{\frac{x}{8}\sqrt{-1}}+\varepsilon^{-\frac{x}{8}\sqrt{-1}}\right\}\left\{2\sqrt{-1}\right\}^{-1}\left(\varepsilon^{\frac{x}{8}\sqrt{-1}}-\varepsilon^{-\frac{x}{8}\sqrt{-1}}\right),$$

or, generally,

$$= 2^n.\,2^{-1}\left\{\varepsilon^{\frac{x}{2}\sqrt{-1}}+\varepsilon^{-\frac{x}{2}\sqrt{-1}}\right\}.\,2^{-1}\left\{\varepsilon^{\frac{x}{4}\sqrt{-1}}+\varepsilon^{-\frac{x}{4}\sqrt{-1}}\right\}.\,2^{-1}$$
$$\left(\varepsilon^{\frac{x}{8}\sqrt{-1}}+\varepsilon^{-\frac{x}{8}\sqrt{-1}}\right\}\ldots.$$
$$..\,2^{-1}\left\{\varepsilon^{\frac{x}{2^n}\sqrt{-1}}+\varepsilon^{-\frac{x}{2^n}\sqrt{-1}}\right\}.\,(2\sqrt{-1})^{-1}\left\{\varepsilon^{\frac{x}{2^n}\sqrt{-1}}-\varepsilon^{-\frac{x}{2^n}\sqrt{-1}}\right).$$

Which is the analytical translation of sin. $x = 2^n.\,\cos.\frac{x}{2}\,.\,\cos.\frac{x}{4}$ &c. Euler, and after him other authors, have demonstrated these formulas by the aid of logarithms, and of theorems drawn from geometry.

X. Euler and Lagrange have treated certain differential equations, which are said to admit for their complete integration an algebraic form, although the integration of each particular term depends on the quadrature of the circle and hyperbola. I purpose to integrate these differential equations, by the method adopted in Articles V. VI.

Let fx, fy, denote functions of x and y.

Suppose the differential equation to be

$\frac{\dot{x}}{x}+\frac{\dot{y}}{y}=0$; then $fx+fy=a$ when $x=\varepsilon^{fx}$, $y=\varepsilon^{fy}$. Hence, $xy=\varepsilon^{fx+fy}=\varepsilon^{a}=A$, a constant quantity.

2dly. Let $\frac{\dot{x}}{\sqrt{1-x^2}}+\frac{\dot{y}}{\sqrt{1-y^2}}=0$

$\therefore fx+fy=a$, x being $=\left\{2\sqrt{-1}\right\}^{-1}.\left(\varepsilon^{fx\sqrt{-1}}-\varepsilon^{-fx\sqrt{-1}}\right)$, and $y=2\sqrt{-1})^{-1}.\left(\varepsilon^{fy\sqrt{-1}}-\varepsilon^{-fy\sqrt{-1}}\right)$; or $\sqrt{(1-x^2)}=2^{-1}.\left(\varepsilon^{fx\sqrt{-1}}+\varepsilon^{-fx\sqrt{-1}}\right)$, and $\sqrt{1-y^2}=2^{-1}.\left(\varepsilon^{fy\sqrt{-1}}+\varepsilon^{-fy\sqrt{-1}}\right)$.

Hence, $x.\sqrt{(1-y^2)}+y\sqrt{(1-x^2)}$

$$=(2\sqrt{-1})^{-1}.\left\{\varepsilon^{(fx+fy)\sqrt{-1}}-\varepsilon^{-(fx+fy)\sqrt{-1}}\right\}$$
$$=(2\sqrt{-1})^{-1}.\left\{\varepsilon^{a\sqrt{-1}}-\varepsilon^{-a\sqrt{-1}}\right\}=A,\ \text{a constant quantity.}$$

3dly. Let $\frac{x^{\cdot}}{\sqrt{a+bx+cx^2}}+\frac{y^{\cdot}}{\sqrt{a+by+cy^2}}=0$

$$\therefore \frac{x^{\cdot}}{\sqrt{c}\sqrt{x^2+\frac{bx}{c}+\frac{a}{c}}}+\frac{y^{\cdot}}{\sqrt{c}\sqrt{(y^2+\frac{by}{c}+\frac{a}{c})}}=0.$$

Let $\alpha+\frac{b}{2c}=v$, $y+\frac{b}{2c}=v'$ and $r^2=\frac{\alpha}{c}-\frac{b^2}{4c^2}$

$$\therefore \frac{v^{\cdot}}{\sqrt{c}\sqrt{v^2+r^2}}+\frac{v'^{\cdot}}{\sqrt{c}\sqrt{v'^2+r^2}}=0,$$

taking the integrals

$$c^{-\frac{1}{2}}\left\{V+V'\right\}=\alpha,\ v=\frac{\varepsilon^{V}-r^2\varepsilon^{-V}}{2},\ v'=\frac{\varepsilon^{V'}-r^2\varepsilon^{-V'}}{2}$$

$$\therefore v\sqrt{r^2+v'^2}+v'\sqrt{(r^2+v^2}=\frac{\varepsilon^{V+V'}-r^4\varepsilon^{-(V+V')}}{2}=\frac{\varepsilon^{\alpha\sqrt{c}}-r^4\varepsilon^{-\alpha\sqrt{c}}}{2}$$

$=$ A, and restoring the values of x and y,

$$\frac{2cx+b}{c}\sqrt{(a+by+cy^2)}+\frac{2cy+b}{c}\sqrt{(a+bx+cx^2)}=A'.$$

. By the above operation it appears, that certain algebraical expressions, as $x\sqrt{1-y^2}+y\sqrt{1-x^2}$, $\frac{2cx+b}{c}\sqrt{a+by+cy^2}$ &c. may be deduced, which answer the equations $\int\frac{x^{\cdot}}{\sqrt{1-x^2}}+\int\frac{y^{\cdot}}{\sqrt{1-y^2}}$ &c. But, strictly speaking, such algebraical expressions are not the integrals: they are rather expressions deduced from the true integral equations, from which other algebraical expressions, besides those put down, might be deduced.*

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* For the integration of this sort of differential equations, see Mem. de Turin. Vol. IV. p. 98. " Sur l'Integration de quelques Equations differentielles, dont les indeterminées sont separées, mais dont chaque Membre en particulier n'est point integrable." In this Memoir are given three different methods of integrating $x^{\cdot}(1-x^2)^{-\frac{1}{2}}=y^{\cdot}(1-y^2)^{-\frac{1}{2}}$; by circular arcs and certain trigonometrical theorems, by impossible logarithms, and by partial integrations. Strictly speaking, all these methods are indirect; and the two first are only different but circuitous modes of expressing the method given in Art. X. See likewise EULER, Calc. integral Vol. II. Novi Comm. Petrop. Tom. VI. p. 37. Tom. VII. p. 1. It is to be observed, that in the present state of analytic science, there is no certain and direct method of integrating differential equa-

XI. In the irreducible case of cubic equations, the root, it is said, may be exhibited by means of certain lines drawn in a circle. There is, however, independently of all geometrical considerations, a method of analytically expressing the root; and, from the analytical expression, although it is not the formula which from the time of CARDAN mathematicians have been seeking, the value of the root may in all cases be arithmetically computed; but, previously, it is necessary to shew what are the different symbols that may be substituted for z in the equations, $x = (2\sqrt{-1})^{-1}\left\{\varepsilon^{z\sqrt{-1}} - \varepsilon^{-z\sqrt{-1}}\right\}$, and $\sqrt{(1-x^2)} = 2^{-1}\left\{\varepsilon^{z\sqrt{-1}} + \varepsilon^{-z\sqrt{-1}}\right\}$. Let $x = 1$, and π be the value of z that answers the equations $1 = (2\sqrt{-1})^{-1}\left\{\varepsilon^{\pi\sqrt{-1}} - \varepsilon^{-\pi\sqrt{-1}}\right\}$and $0 = 2^{-1}(\varepsilon^{\pi\sqrt{-1}} + \varepsilon^{-\pi\sqrt{-1}})$, which value of π may be numerically computed from the expression .. $\pi = z = x + \frac{x^3}{2.3} + \frac{3x^5}{5.8} + \frac{5x^7}{7.16} +$ &c. $(x = 1)$.

Hence, $\varepsilon^{\pi\sqrt{-1}} = - \varepsilon^{-\pi\sqrt{-1}} = \sqrt{-1}$ $\therefore \varepsilon^{2\pi\sqrt{-1}} = \varepsilon^{-2\pi\sqrt{-1}} = -1$ $\therefore \varepsilon^{4\pi\sqrt{-1}} = \varepsilon^{-4\pi\sqrt{-1}} = 1$ $\therefore$, $\varepsilon^{8\pi\sqrt{-1}} = \varepsilon^{-8\pi\sqrt{-1}} = 1$ $\therefore$ $\varepsilon^{16\pi\sqrt{-1}} = \varepsilon^{-16\pi\sqrt{-1}} = 1$ &c. (for since $\varepsilon^{-m\pi\sqrt{-1}} = \frac{1}{\varepsilon^{m\pi\sqrt{-1}}}$, and $\varepsilon^{m\pi\sqrt{-1}} = \varepsilon^{-m\pi\sqrt{-1}} = \frac{1}{\varepsilon^{m\pi\sqrt{-1}}}$ $\therefore \varepsilon^{2m\pi\sqrt{-1}} = 1$).

Again, since $\varepsilon^{4\pi\sqrt{-1}} = 1$ and $\varepsilon^{8\pi\sqrt{-1}} = 1$, $\varepsilon^{12\pi\sqrt{-1}} = 1$; and

tions such as $x \cdot \left\{a + bx + cx^2 + dx^3 + \varepsilon x^4\right\}^{-\frac{1}{2}} + y \cdot \left\{a + by + cy^2 + dy^3 + \varepsilon y^4\right\}^{-\frac{1}{2}} = 0$, because no analytical expression or equation of a finite form has hitherto been invented, from which, according to the processes of the differential Calculus, such differential equations may be deduced. To find the algebraical expressions which answer to these equations, recourse must be had to what are properly to be denominated artifices. For such, see Mem. de Turin. Vol. IV. Comm. Petr. Tom. VI. VII. LAGRANGE, Fonct. Analyt. p. 80. LACROIX, Calc. diff. p. 427, &c.

generally $\varepsilon^{4n\pi\sqrt{-1}} = \varepsilon^{-4n\pi\sqrt{-1}} = 1$, n any number of the progression 0, 1, 2, 3, 4, &c.

And, since $\varepsilon^{2\pi\sqrt{-1}} = \varepsilon^{-2\pi\sqrt{-1}} = -1 \therefore \varepsilon^{2\pi\sqrt{-1}} \times \varepsilon^{4n\pi\sqrt{-1}} = \varepsilon^{-2\pi\sqrt{-1}} \times \varepsilon^{-4n\pi\sqrt{-1}} = -1$, or $\varepsilon^{(2n+1)\,2\pi\sqrt{-1}} = \varepsilon^{-(2n+1)2\pi\sqrt{-1}} = -1$, n any number of the progression 0, 1, 2, 3, 4, 5, &c.

Hence it appears, that if $x = (2\sqrt{-1})^{-1}\left\{\varepsilon^{z\sqrt{-1}} - \varepsilon^{-z\sqrt{-1}}\right\}$, $x \times 1 = \left\{2\sqrt{-1}\right\}^{-1}\left\{\varepsilon^{z\sqrt{-1}} - \varepsilon^{-z\sqrt{-1}}\right\} \times \varepsilon^{4n\pi\sqrt{-1}} =$ (since $\varepsilon^{4n\pi\sqrt{-1}} = \varepsilon^{-4n\pi\sqrt{-1}}$) $(2\sqrt{-1})^{-1}\left\{\varepsilon^{(4n\pi+z)\sqrt{-1}} - \varepsilon^{-(4n\pi+z)\sqrt{-1}}\right\}$.

Again, since $\varepsilon^{(2n+1)2\pi\sqrt{-1}} = \varepsilon^{-(2n+1)\,2\pi\sqrt{-1}} = -1$

$x \times -1 = (2\sqrt{-1})^{-1}\left\{\varepsilon^{z\sqrt{-1}} - \varepsilon^{-z\sqrt{-1}}\right\}\varepsilon^{(2n+1)\pi\sqrt{-1}}$

$= (2\sqrt{-1})^{-1}\{-\varepsilon^{((2n+1)2\pi-z)\sqrt{-1}} - \varepsilon^{-((2n+1)2\pi-z)\sqrt{-1}}$,

consequently,

$x = (2\sqrt{-1})^{-1}\left\{\varepsilon^{((2n+1)2\pi-z)\sqrt{-1}} - \varepsilon^{-((2n+1)2\pi-z)\sqrt{-1}}\right\}$,

or the equation $x = (2\sqrt{-1})^{-1}\left\{\varepsilon^{z\sqrt{-1}} - \varepsilon^{-z\sqrt{-1}}\right\}$ is true, when instead of z is put $(4\pi+z)$ or $(8\pi+z)$, or generally $(4n\pi+z)$; and is moreover true, when instead of z is put

$(2\pi-z)$, $(6\pi-z)$, or generally $(2n+1)\;2\pi-z$.

In like manner, the equation $\sqrt{1-x^2} = 2^{-1}\left\{\varepsilon^{z\sqrt{-1}} + \varepsilon^{-z\sqrt{-1}}\right\}$ is true, when instead of z is put

$4\pi+z$, $8\pi+z$, or $12\pi+z$, or generally $4n\pi+z$;

and is moreover true, when instead of z is put

$4\pi-z$, ,$8\pi-z$, or $12\pi-z$, or generally $4n\pi-z$.

Let now $x^3 - qx = r$, then, by CARDAN's solution,

$$x = \sqrt[3]{\left(\frac{r}{2} + \sqrt{\left(\frac{r}{4} - \frac{q^3}{27}\right)}\right)} + \sqrt[3]{\left(\frac{r}{2} - \sqrt{\left(\frac{r^2}{4} - \frac{q^4}{27}\right)}\right)};$$

put $\frac{r}{2} = a, \frac{r^2}{4} - \frac{q^3}{27} = -b^2$, then $x = \sqrt[3]{(a+b\sqrt{-1})} + \sqrt[3]{a(-b\sqrt{-1})}$.

Let $a + b\sqrt{-1} = m\varepsilon^{z\sqrt{-1}} \therefore a - b\sqrt{-1} = m\varepsilon^{-z\sqrt{-1}}$

$a^2+b^2=m^2$, $a=m\left\{\frac{\varepsilon^{z\sqrt{-1}}+\varepsilon^{-z\sqrt{-1}}}{2}\right\}$, $b=m\left\{\frac{\varepsilon^{z\sqrt{-1}}-\varepsilon^{-z\sqrt{-1}}}{2\sqrt{-1}}\right\}$, or $2^{-1}.\left\{\varepsilon^{z\sqrt{-1}}+\varepsilon^{-z\sqrt{-1}}\right\}=\frac{a}{\sqrt{a^2+b^2}}$, and $(2\sqrt{-1})^{-1}\left\{\varepsilon^{z\sqrt{-1}}-\varepsilon^{-z\sqrt{-1}}\right\}=\frac{b}{\sqrt{a^2+b^2}}$; but, from what has been premised, these equations are true, when instead of z is put $\theta+z$, or $2\theta+z$, or $4\theta+z$, or generally $n\theta+z$, $(4\pi=\theta)$.

Hence, $\sqrt[3]{(a+b\sqrt{-1})}+\sqrt[3]{(a-b\sqrt{-1})}$ is $m^{\frac{1}{3}}\left\{\varepsilon^{\frac{z}{3}\sqrt{-1}}+\varepsilon^{-\frac{z}{3}\sqrt{-1}}\right\}$, or $m^{\frac{1}{3}}\left\{\varepsilon^{\frac{\theta+z\sqrt{-1}}{3}}+\varepsilon^{\frac{-\theta+z\sqrt{-1}}{3}}\right\}$, or generally $m^{\frac{1}{3}}\left\{\varepsilon^{\frac{n\theta+z\sqrt{-1}}{3}}+\varepsilon^{\frac{-(n\theta+z)\sqrt{-1}}{3}}\right\}$: there are, however, only 3 different values of x, for the index of ε in the fourth value is $\frac{3\theta+z\sqrt{-1}}{3}$, and $\varepsilon^{\frac{3\theta+z\sqrt{-1}}{3}}=\varepsilon^{\theta\sqrt{-1}}\times\varepsilon^{\frac{z}{3}\sqrt{-1}}=1\times\varepsilon^{\frac{z}{3}\sqrt{-1}}$ $\therefore$ the fourth value is the same as the first. Again, the index of ε in the fifth value is $\frac{4\theta+z}{3}\sqrt{-1}$; but $\varepsilon^{\frac{(4\theta+z)}{3}\sqrt{-1}}=\varepsilon^{\theta\sqrt{-1}}\times\varepsilon^{\frac{\theta+z}{3}\sqrt{-1}}=1\times\varepsilon^{\frac{\theta+z}{3}\sqrt{-1}}$. The 5th value is the same as 2d, and so on; and, consequently, the indices of ε in the 3 different values of x are $\pm\frac{z}{3}\sqrt{-1}$, $\pm\frac{\theta+z}{3}\sqrt{-1}$, $\pm\frac{2\theta+z}{3}\sqrt{-1}$.

If, instead of the index of ε in the 3d value, $\pm\frac{\theta-z}{3}\sqrt{-1}$ be put, the value of the root remains the same; for, since $\varepsilon^{\theta\sqrt{-1}}=\varepsilon^{-\theta\sqrt{-1}}=1$ $\therefore$ $x\times 1=m^{\frac{1}{3}}\times\left\{\varepsilon^{\frac{2\theta+z}{3}\sqrt{-1}}\times\varepsilon^{-\theta\sqrt{-1}}+\varepsilon^{\frac{-2\theta+z}{3}\sqrt{-1}}\times\varepsilon^{\theta\sqrt{-1}}\right\}=m^{\frac{1}{3}}\left\{\varepsilon^{\frac{-\theta-z}{3}\sqrt{-1}}+\varepsilon^{\frac{\theta-z}{3}\sqrt{-1}}\right\}$.

This mode of representing the roots is not, as has been already stated, according to the conditions* of the formula demanded by mathematicians. It enables us, however, immediately to ascertain that the roots are possible, and to calculate their approximate value; for, when $\sqrt{1-x^2}$ or $y = 2^{-1}$ $\left\{ \varepsilon^{z\sqrt{-1}} + \varepsilon^{-z\sqrt{-1}} \right\}$, $z = \int - \frac{y}{\sqrt{1-y^2}}$

$$= \alpha - \left\{ y + \frac{y^3}{3.2} + \frac{3y^5}{5.8} + \frac{5y^7}{7.16} + \&c. \right\},$$

when $z = 0$ $y = 2^{-1}\left\{ \varepsilon^0 + \varepsilon^{-0} \right\} = 1$

$$\therefore \alpha = 1 + \frac{1}{3.2} + \frac{3}{5.8} + \frac{5}{7.16} + \&c. \Big\} = \pi.$$

Hence, we may numerically approximate to the value of z from the expression $z = \pi - \left\{ y + \frac{y^3}{3.2} + \frac{3y^5}{5.8} + \&c. \right\}$ when y is given, and < 1. Now, in the case of the cubic equation, $y = \frac{a}{\sqrt{a^2+b^2}} = \frac{3r\sqrt{3}}{2q\sqrt{q}}$; and, since $\frac{r^2}{4} < \frac{q^3}{27}$ $\therefore \frac{3r\sqrt{3}}{2q\sqrt{q}}$ is < 1, consequently the value of z may be obtained; suppose it t, then the roots are to be approximated to, by means of the series that result from the developements of the forms by which they are represented; to wit,

$$2\sqrt{\frac{q}{3}}\left\{ 1 - \frac{t^2}{1.2.3^2} + \frac{1}{1.2.3.4} \cdot \frac{t^4}{3^4} - \&c. \right\}$$

$$2\sqrt{\frac{q}{3}}\left\{ 1 - \frac{(\theta+t)^2}{1.2.3^2} + \frac{1}{1.2.3.4} \cdot \frac{(\theta+t)^4}{3^4} - \&c. \right\}$$

$$2\sqrt{\frac{q}{3}}\left\{ 1 - \frac{(2\theta+t)^2}{1.2.3^2} + \frac{1}{1.2.3.4} \cdot \frac{(2\theta+t)^4}{3^4} - \&c. \right\}$$

Now these series converge; for, since t is finite, we must at length arrive at a term $\underset{n}{\mathrm{A}}$, in which $(n-1)\,n$ is $> \left(\frac{t}{3}\right)^2$; and, since $(n+1)$th term $\left(\underset{n+1}{\mathrm{A}}\right)$ is $= \underset{n}{\mathrm{A}} \times \left(\frac{t}{3}\right)^2 \times \frac{1}{n+1\,(n+2)} \therefore \underset{n+1}{\mathrm{A}}$ is

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* The conditions of the formula are, that it should be finite in regard to the number of terms, free from imaginary quantities, and containing only the coefficients q and r. See Mem. de l'Acad. 1738.

$< \overset{A}{n}$, a fortiori, $\overset{A}{n+1}$ is $< \overset{A}{n+1}$, and so on; the terms after the $n-1$th term constantly diminishing.*

The above method is purely analytical: it has no tacit reference to other methods; it does not virtually suppose the existence either of an hyperbola or circle. If practical commodiousness, however, be aimed at, it is *convenient* to give a different expression to the values of the roots, or to translate them into geometrical language: and this, because tables have been calculated, exhibiting the numerical values of the cosines, &c. of circular arcs. Now, since it has already appeared that the cosine of an arc $z = 2^{-1}\left\{\varepsilon^{z\sqrt{-1}} + \varepsilon^{-z\sqrt{-1}}\right\}$, the 3 roots of the equation $x^3 - qx = r$ may be said to equal

$$2\sqrt{\frac{q}{3}}\,.\,\cos.\,\frac{t}{3},\ 2\sqrt{\frac{q}{3}}\cos.\,\frac{\theta+t}{3},\ 2\sqrt{\frac{q}{3}},\ \cos.\,\frac{\theta-t}{3}.$$

XII. In the fifth volume of his *Opuscules*,† D'ALEMBERT

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* In the Phil. Trans. for 1801. p. 116, I mentioned M. NICOLE as the first mathematician who shewed the expression of the root in the irreducible case, when expanded, to be real. But the subjoined passage, in LEIBNITZ's Letter to WALLIS, causes me to retract my assertion. "Diu est quod ipse quoque judicavi $\sqrt{{}^3a+b\sqrt{-1}}$ "$+\sqrt{{}^3a+b\sqrt{1}} = z$ esse quantitatem realem, etsi speciem habeat imaginariæ; "ob virtualem nimirum imaginariæ destructionem, perinde ac in destructione actuali "$a+b\sqrt{-1} + a-b\sqrt{-1} = 2a$. Hinc, si ex $\sqrt{{}^3a\pm b\sqrt{-1}}$ extrahamus radicem "ope seriei infinitæ, ad inveniendum valorem ipsius z serie tali expressum, efficere "possumus, ut reapse evanescat imaginaria quantitas. Atque ita etiam, in casu imaginario, regulis Cardanicis cum fructu utimur," &c. Vol. III. p. 126. See also p. 54.

† "Elle etoit neanmoins d'autant plus essentielle, que l'expression de l'arc par le "sinus, fondée sur la serie connue, qui est l'intégrale de $\frac{dx}{\sqrt{1-x^2}}$, ne peut etre regardée "comme exacte, c'est a dire, comme representant à la fois tous les arcs qui ont le "même sinus; puisque cette serie ne represente evidemment qu'un seul des arcs qui "repondent au sinus dont il s'agit, savoir, le plus petit de ces arcs, celui qui est inférieur, ou tout au plus egal, a 90 degrés. Cependant, c'est d'un autre côté une sorte "de paradoxe remarquable, que l'expression de l'arc par le sinus ne representant qu'un

mentions it as a remarkable paradox, that the series for the arc in terms of the sine represents only one arc, viz. the arc less than 90 degrees; whereas the series for the sine, produced by reversion from the former series, exhibits all possible arcs that have the same sine. I shall endeavour to solve this paradox, which, I think, originated partly from the introduction of geometrical considerations into an analytical investigation, by which the true derivation of certain expressions was concealed.

It has appeared that the equation $x=(2\sqrt{-1})^{-1}\left\{\varepsilon^{z\sqrt{-1}}-\varepsilon^{-z\sqrt{-1}}\right\}$ is true, when instead of z is put, $\theta+z$, or $2\theta+z$, ... or $n\theta+z$, or $\frac{\theta}{2}-z$, or $\frac{3\theta}{2}-z$ .... or $\frac{2n+1}{2}\theta-z$.

Now, if the fluxions of these equations are taken, and the equations cleared of exponential quantities, there results from each the same equation, to wit, $z^{\cdot}=\frac{x^{\cdot}}{\sqrt{1-x^2}}$. Hence, if the symbol $\int$ denotes the operation by which we are to ascend to the original equations from which $z^{\cdot}=\frac{x^{\cdot}}{\sqrt{1-x^2}}$ is derived, the only strict consequence from $\int z^{\cdot}=\int\frac{x^{\cdot}}{\sqrt{1-x^2}}$ is that $x=(\sqrt{-1})^{-1}\left\{\varepsilon^{z\sqrt{-1}}\varepsilon-{}^{-z\sqrt{-1}}\right\}$, or $=(2\sqrt{-1})^{-1}$

$$\left\{\varepsilon^{(\theta+z)\sqrt{-1}}-\varepsilon^{-(\theta+z)\sqrt{-1}}\right\},$$

or generally $=(2\sqrt{-1})^{-1}\left\{\varepsilon^{(n\theta+z)\sqrt{-1}}-\varepsilon^{-(n\theta+z)\sqrt{-1}}\right\}$, or

$$=(2\sqrt{-1})^{-1}\left\{\varepsilon^{\frac{2n+1}{2}\theta\sqrt{-1}}-\varepsilon^{\frac{-(2n+1)}{2}\theta\sqrt{-1}}\right\}.$$

"seul arc de 90 degrés au plus, l'expression du sinus par l'arc, qu'on peut deduire (par "la méthode du retour de suites) de l'expression de l'arc par le sinus, represente "exactement, etant poussée a l'infini, le sinus de tous les arcs possibles, plus petits "ou plus grands que 90°, et même que la circonference ou demi circonference, prise "tant de fois qu'on voudra. Je laisse à d'autres geometres, le soin d'éclaircir ce "mystère, ainsi que plusieurs autres," &c. p. 183.

Hence, to answer the equation $z^{\cdot} = \frac{x^{\cdot}}{\sqrt{1-x^2}}$,

$$x \text{ may} = z - \frac{z^3}{1.2.3} + \frac{z^5}{1.2.3.4.5} - \&c.$$

$$\text{or } z' - \frac{z'^3}{1.2.3} + \frac{z'^5}{1.2.3.4.5} - \&c.$$

$$\text{or } z'' - \frac{z''^3}{1.2.3} + \frac{z''^5}{1.2.3.4.5} - \&c.$$

$\{z', z'', z''', \&c.$ representing $\theta + z$, $2\theta + z$, $3\theta + z$, &c.$\}$. Suppose now it is necessary to deduce z; z', z'', &c. in terms of x and its powers, by reversion of series. What does the reversion of series mean? Merely this: a certain method or operation, according to which, one quantity being expressed in terms of another, the second may be expressed in terms of the first. Hence, in all similar series, the operation must be the same; consequently, the result, which is merely the exhibition of a formula, must be the same; so that, whatever is the series in terms of x, produced by *reversion* in

$x = z - \frac{z^3}{1.2.3} + \frac{z^5}{1.2.3.4.5} - \&c.$ the same must be produced by reversion in $x = z' - \frac{z'^3}{1.2.3} + \frac{z'^5}{1.2.3.4.5} - \&c.$

$$\text{in } x = z'' - \frac{z''^3}{1.2.3} + \&c.$$

&c.

The series produced by reversion in these cases is, $x + \frac{x^3}{3.2} + \frac{3x^5}{5.8} + \&c.$ Hence it appears, that we know, a priori, that must happen which D'ALEMBERT considers as a paradox to have happened. Why this paradox found reception in the mind of this acute mathematician, I have stated, as my opinion, one cause to have been, an inattention, from geometrical considerations, to the real origin and derivation of certain expressions that appeared in the course of the calculation. Another cause I apprehend was, the want of precise notions on the force and signification of the symbol $=$. It is true that its signification entirely depends on definition; but, if the definition given of it in elementary treatises be adhered to, I believe it will be impossible to shew the justness and legitimacy of most mathematical processes. It scarcely ever denotes numerical equality. In its general and extended meaning,* it denotes the result of certain operations. Thus, when from

$x = z - \frac{z^3}{1.2.3} + \frac{z^5}{1.2.3.4.5}$, $x = z' - \frac{z^3}{1.2.3}$ &c.

z or z' is inferred $= x + \frac{x^3}{3.2} + \frac{3x^5}{5.8}$ &c. nothing is affirmed concerning a numerical equality; and all that is to be understood is, that $x + \frac{x^3}{3.2} + \frac{3x^5}{5.8} +$ &c. is the result of a certain operation performed on $x = z - \frac{z^3}{1.2.3} + \frac{z^5}{1.2.3.4.5} -$ &c.

XIII. It appears then, that according to the reversion of series, z, z', z'', &c. must all be represented by the same series, proceeding according to the powers of x; but, if a form for z be required, which shall in all cases afford us a means of numerically computing its value, such a form must involve certain arbitrary quantities. These arbitrary quantities are to be determined by conditions which depend either on the original form of the equation between x and z, or on the nature of the object to which the calculus is applied.

Let now $\int \frac{\dot{x}}{\sqrt{1-x^2}}$ mean† $x + \frac{x^3}{3.2} + \frac{3x^5}{5.8} +$ &c.

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* This is consistent with what I advanced in the Phil. Trans. for 1801. p. 99, concerning the meaning of the symbols $\times$ $+$, &c. It is beside my present purpose, to insist farther on the necessity of attaching precise notions to the symbols employed in calculation; and the subject deserves a separate and ample discussion.

† It is not so easy to prove as it may be imagined, that $\int \frac{\dot{x}}{\sqrt{1-x^2}} = x + \frac{x^3}{3.2} + \frac{3x^5}{5.8} +$ &c.

then, if z represent the arc of a circle, and x the sine, this equality* $z = x + \frac{x^3}{3.2} + \frac{3.x^5}{5.8} + \&c.$ is subject to restrictions, for x cannot exceed 1; consequently, the greatest value of z that can be determined from the equation, must be so determined by putting $x = 1$. Let $\pi = 1 + \frac{1}{3.2} + \frac{3}{5,8}$ &c.

Now, from the definition of sine and the nature of the circle, the arcs $2\pi - z$, $6\pi - z \ldots (2n+1)\, 2\pi - z \ldots 4\pi + z \ldots 8\pi + z \ldots 4n\pi + z$, have the same sine; let these arcs be z, z', z'', z''', &c.
and let $x + \frac{x^3}{3.2} + \frac{3.x^5}{5.8} + \&c. = X$,
then $z' = 2\pi - X$, $z'' = 6\pi - X$, &c. or generally
$z''^{\cdots m} = \{2n+1\}\, 2\pi - X$, or $= 4n\pi + X$,
n any number of the progression 0, 1, 2, 3, 4, &c.

Or thus, from the conditions contained in the form of the equation between z and x,
since $\sqrt{1-x^2} = 2^{-1}.\{\varepsilon^{z\sqrt{-1}} + \varepsilon^{-z\sqrt{-1}}\} = 1 - \frac{z^2}{1.2} + \&c.$
there is no possible value of z that answers the equation when x is > 1,
Let $\int \frac{\dot{x}}{\sqrt{1-x^2}} = X + \alpha$ $\therefore$ when z and x begin together,
$\alpha = o$ and $z = X$.

But the equation $\frac{\dot{x}}{\sqrt{1-x^2}} = \dot{z}$ may be derived from $x = (2\sqrt{-1})^{-1}$ $\{\varepsilon^{z\sqrt{-1}} - \varepsilon^{-z\sqrt{-1}}\}$,
when instead of z is put $2\pi - z$, $6\pi - z \ldots (2n+1)\, 2\pi - z$,

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* In the expression $z = x + \frac{x^3}{3.2} + \frac{3x^5}{5.8} + \&c.$ considered abstractedly from its origin and application, there is nothing that limits the value of x. Again, by applying the operation of reversion, x is represented by this form, $z - \frac{z^3}{1.2.3} + \frac{z^5}{1.2.3.4.5}$ &c. But there is no method, I believe, of proving (I purposely exclude that unproved proposition that every equation has as many roots as dimensions) that instead of z in $x - z + \frac{z^3}{1.2.3} - \&c. = o$, other quantities, as z', z'', &c. may be substituted.

$$\text{or } 4\pi+z \ \ldots\ldots \qquad 4n\pi+z.$$

Hence, z' or $2\pi-z=-X+\alpha$. Let $z=o \therefore X=o \therefore 2\pi=\alpha$. Again, z'' or $6\pi-z=-X+\alpha$. Let $z=o \therefore X=o \therefore 6\pi=\alpha$. Hence, the arbitrary quantity α may generally be represented by $(2n+1)2\pi$, or by $4n\pi \therefore z''\cdots{}^{m}=(2n+1)2\pi-X$,

$$\text{or } = 4n\pi+X.$$

XIV. I shall now shew, by a purely analytical process, what are the divisors of $x^n \mp a^n$. It seems a very strange and absurd method, to refer to the properties of geometrical figures, for the knowledge of the composition of analytical expressions.

Let $x=m^{\frac{1}{n}}\,\varepsilon^{\frac{z}{n}\sqrt{-1}} \therefore a^n=m\,\varepsilon^{z\sqrt{-1}} \therefore m=\frac{a^n}{\varepsilon^{z\sqrt{-1}}}$, and m will be always positive, if $\varepsilon^{z\sqrt{-1}}=1$. But (Art. XI.) the values of z that answer the equation $\varepsilon^{z\sqrt{-1}}=1$, are $o\theta, \pm\theta, \pm 2\theta, \pm 3\theta$, or generally $\pm s\theta$, s, any number of the progression 0, 1, 2, 3, &c.

Hence, $x=a\,\varepsilon^{\frac{\pm s\theta}{n}\sqrt{-1}}$ generally,

or values of x are a, $a\varepsilon^{\frac{\theta}{n}\sqrt{-1}}$, $a\,\varepsilon^{\frac{-\theta}{n}\sqrt{-1}}$, $a\,\varepsilon^{\frac{2\theta}{n}\sqrt{-1}}$, $a\,\varepsilon^{\frac{-2\theta}{n}\sqrt{-1}}$, &c.

$\therefore x^n-a^n=(x-a)\left(x^2-a\left\{\varepsilon^{\frac{\theta}{n}\sqrt{-1}}+\varepsilon^{\frac{-\theta}{n}\sqrt{-1}}\right\}+a^2\right)\left(x^2-a\left\{\varepsilon^{\frac{2\theta}{n}\sqrt{-1}}+\varepsilon^{\frac{-2\theta}{n}\sqrt{-1}}\right\}+a^2\right)$, &c. n being odd;

when n is even, (and of the form $2p$, p odd,) there must be a number (s) in the progression (0, 1, 2, 3, &c.) that $=\frac{n}{2}$; consequently, there must be a value of x, $a\,\varepsilon^{\frac{s\theta}{n}\sqrt{-1}}=a\varepsilon^{\frac{\theta}{2}\sqrt{-1}}=-a$, since (Art. XI.) $\varepsilon^{2\pi\sqrt{-1}}$, or $\varepsilon^{\frac{\theta}{2}\sqrt{-1}}=-1$.

Hence, a quadratic divisor of x^n-a^n will be $(x-a).(x+a)$, or x^2-a^2; when n is even, and of the form $4p$, p even or odd, there must be a number (s) in the progression (0, 1, 2, 3 ....) $=\frac{n}{4}$; consequently, there must be a value of x, $a\varepsilon^{\frac{\pm s\theta}{n}\sqrt{-1}} = a\varepsilon^{\frac{\pm\theta}{4}\sqrt{-1}} = a \times \pm\sqrt{-1}$, since (Art. XI.) $\varepsilon^{\pm\pi\sqrt{-1}}$, or $\varepsilon^{\frac{\pm\theta}{4}\sqrt{-1}} = \pm\sqrt{-1}$.

Hence, one quadratic divisor of $x^n - a^n$ will be of the form $x^2 + a^2 = (x + a\sqrt{-1}).\ (x - a\sqrt{-1})$; another, as it has been already shewn, will be of the form $x^2 - a^2$.

There are only n different divisors, for (n odd) the $(n-1)$th and nth divisors are comprised under the form $x = a\varepsilon^{\frac{\pm\overline{n-1}}{2n}\theta\sqrt{-1}}$; the succeeding divisors would be comprised under the form $x = a\varepsilon^{\frac{\pm n+1}{2n}\theta\sqrt{-1}} = a\varepsilon^{\pm\theta\sqrt{-1}} \times \varepsilon^{\frac{\mp n-1}{2n}\theta\sqrt{-1}} = a\varepsilon^{\frac{\mp n-1}{2n}\theta\sqrt{-1}}$, (since $\varepsilon^{\pm\theta\sqrt{-1}} = 1$) the same as preceding form.

If $x^n + a^n = o$, then $m = \frac{-a^n}{\varepsilon^{x}\sqrt{-1}}$, to have m always positive.

Let $\varepsilon^{z\sqrt{-1}} = -1$, then (Art. XI.) the values of z are $\pm 2\pi, \pm 6\pi$ ...&c.

Let $2\pi = \rho$, then generally $\varepsilon^{(2s+1)\rho\sqrt{-1}} = -1$; consequently, $x = a\varepsilon^{\frac{\pm(2s+1)}{n}\rho\sqrt{-1}}$, s any number of the progression 0, 1, 2, &c. or the values of x are $a\varepsilon^{\frac{\pm\rho}{n}\sqrt{-1}}$, $a\varepsilon^{\frac{\pm 3\rho}{n}\sqrt{-1}}$ &c.

or $x^n + a^n = (x^2 - a\left\{\varepsilon^{\frac{\rho}{n}\sqrt{-1}} + \varepsilon^{\frac{-\rho}{n}\sqrt{-1}}\right\} + a^2) . (x^2 - a\left\{\varepsilon^{\frac{3\rho}{n}\sqrt{-1}} + \varepsilon^{\frac{-3\rho}{n}\sqrt{-1}}\right\} + a^2)$ &c.

When n is odd, there must be a number $(2s+1)$ in the progression (1, 3, 5, 7, &c.) $= n$; consequently, one value of x must

$= a\varepsilon^{\rho\sqrt{-1}} = -a$, or $x + a$ must be a divisor of $x^n + a^n$.

XV. Resolution of $x^{2n} - 2la^n x^n + a^{2n}$ into its quadratic factors $l \angle 1$.

Now, from the equation $x^n = a^n\{l \pm \sqrt{l^2 - 1}\} = \mathrm{A} \pm \mathrm{B}\sqrt{-1}$.

Let $x = m^{\frac{1}{n}}\varepsilon^{\frac{\pm z}{n}\sqrt{-1}}$ $\therefore m\varepsilon^{z\sqrt{-1}} = \mathrm{A} + \mathrm{B}\sqrt{-1}$, $m\varepsilon^{-z\sqrt{-1}} = \mathrm{A} - \mathrm{B}\sqrt{-1}$, $m = \sqrt{(\mathrm{A}^2 + \mathrm{B}^2)}$, $2^{-1}\{\varepsilon^{z\sqrt{-1}} + \varepsilon^{-z\sqrt{-1}}\} = \frac{\mathrm{A}}{\sqrt{\mathrm{A}^2+\mathrm{B}^2}} = l$,

$$(2\sqrt{-1})^{-1}\{\varepsilon^{z\sqrt{-1}} - \varepsilon^{-z\sqrt{-1}}\} = \frac{\mathrm{B}}{\sqrt{\mathrm{A}^2+\mathrm{B}^2}} = \sqrt{1 - l^2};$$

but (Art. XI.) these equations are true, when instead of z are put $\theta + z$, $2\theta + z$, $3\theta + z$ .... generally $s\theta + z$.

Hence, the general value of x is $a\varepsilon^{\frac{\pm s\theta + z}{n}\sqrt{-1}}$, and the values of x are $a\varepsilon^{\frac{\pm z}{n}\sqrt{-1}}$, $a\varepsilon^{\frac{\pm\theta + z}{n}\sqrt{-1}}$, $a\varepsilon^{\frac{\pm 2\theta + z}{n}\sqrt{-1}}$,

or $x^{2n} - 2la^n x^n + a^{2n} = \left(x^2 - a\left\{\varepsilon^{\frac{z}{n}\sqrt{-1}} + \varepsilon^{\frac{-z}{n}\sqrt{-1}}\right\} + a^2\right) \times \left(x^2 - a\left\{\varepsilon^{\frac{\theta + z}{n}\sqrt{-1}} + \varepsilon^{\frac{-\theta + z}{n}\sqrt{-1}}\right\} + a^2\right)$ × &c.

XVI. Such are the analytical processes according to which the resolutions of $x^n \mp a^n$, $x^{2n} \mp 2la^n x^n + a^{2n}$ are effected; and thence the fluents of $\frac{\dot{x}}{x^n \mp a^n}$, $\frac{\dot{x}}{x^{2n} - 2la^n x^n + a^{2n}}$, &c. &c. may be obtained, by resolving the fractions $\frac{1}{x^n - a^n}$ &c. into a series of partial fractions, of the form $\frac{\mathrm{A}x + \mathrm{B}}{x^2 + 2\alpha x + \alpha^2 + \beta^2}$.

Since the above resolution of $x^n \mp a^n$ into its quadratic factors would, it appears to me, be strictly true, if such a curve as the circle had never been invented, nor its properties investigated, it is erroneous to suppose that the theorem of Cotes is essentially necessary for the integration of certain differential forms.

That analytical science was advanced by the discovery of this theorem, is indeed true; but the circle and its lines were no farther useful or necessary, than as they afforded a mode of expressing, in geometrical language, an analytical truth. What is analytically expressed, may be analytically combined and resolved; and, if Cotes, by the properties of figures, has expressed his discovery, it is because the mathematicians of the time in which he lived, were more skilful and dexterous with the geometrical method than with the analytical.

In order to demonstrate Cotes's property of the circle, considered as such, one of two different methods must be pursued. Either let the demonstration be strictly geometrical, according to the method of the ancients, or as completely analytical as possible; that is, let the demonstration be effected by the analytical method, from as few fundamental principles as possible. I know not on what grounds of perspicuity and rigour, the propriety of a demonstration half geometrical, half algebraical, can be established; for, besides the want of symmetry in such a demonstration, in strictness of reasoning, a separate discussion is necessary, to shew the propriety and justness of the application of analysis to certain properties of extension demonstrated geometrically.

It is beside my present purpose, to inquire whether Cotes's theorem can be demonstrated strictly after the method of the ancients: hitherto it has not been so demonstrated. To demonstrate it analytically, in the most simple and direct manner, we must proceed from as few fundamental principles as possible;* and give to the quantities concerned, their true and natural

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* For the analytical demonstration, all that is necessary to be known, is what is proved in the 47th of the Elements.

representation. I think, therefore, the analytical demonstration in which the symbol $\sqrt{-1}$ is introduced, (for the cosine of an arc cannot be adequately and abridgedly represented in terms of the arc, except by means of the symbol $\sqrt{-1}$,) to be the most simple and direct that can be exhibited. I have endeavoured, in a former paper, to shew that demonstration with such symbols as $\sqrt{-1}$ may be strict and rigorous.

XVII. One or two more instances of the advantage accruing to calculation, from giving to quantities in analytical investigation their true analytical representation, I now offer, in the demonstrations of the series for the chord of the supplement of a multiple arc, in terms of the chord of the supplement of the simple arc, for the sine of the multiple arc, &c.

Chord $2\pi - z = (\sqrt{-1})^{-1}\left\{\varepsilon^{\frac{(2\pi-z)}{2}\sqrt{-1}} - \varepsilon^{\frac{-(2\pi-z)}{2}\sqrt{-1}}\right\}$,

$= \varepsilon^{\frac{z}{2}\sqrt{-1}} + \varepsilon^{\frac{-z}{2}\sqrt{-1}}$, since $\varepsilon^{\pi\sqrt{-1}} = \sqrt{-1}$, and $\varepsilon^{-\pi\sqrt{-1}} = -\sqrt{-1}$.

Again, chord $(2\pi - nz) = \varepsilon^{\frac{nz}{2}\sqrt{-1}} + \varepsilon^{\frac{-nz}{2}\sqrt{-1}}$. Let $\varepsilon^{\frac{z}{2}\sqrt{-1}} = \alpha$, $\varepsilon^{\frac{-z}{2}\sqrt{-1}} = \beta \;\therefore\; \alpha\beta = 1$; what we have to do then, is to find $\alpha^n + \beta^n$ in terms of $\alpha + \beta$; and, for facility of computation, a new mode of notation may be advantageously introduced, which requires a brief explanation only.*

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* I had obtained the forms for chords nz, &c. given in the following pages, by actually expressing in terms of n and b, the coefficient of x^n, in the developement of the trinomial $\left\{1 + bx + x^2\right\}^m$, when the very admirable work of ARBOGAST, Du Calcul des Derivations, came to my hands. The great simplicity and convenience of his notation have caused me to adopt it, although it does not harmonize well with the fluxionary notation which I have employed in the present Paper.

By Art. V. $\varphi(x+o) = \varphi x + Po + \frac{Qo^2}{1.2} + \frac{Ro^3}{1.2.3}$ &c.
Let D be the note of the operation to be performed on φx, in order to deduce P, then $P = \mathrm{D}\varphi x$, $Q = \mathrm{D}P = \mathrm{DD}\varphi x = \mathrm{D}^2\varphi x$ &c.
Hence, $\varphi(x+o) = \varphi x + \mathrm{D}\varphi x o + \frac{\mathrm{D}^2\varphi x . o^2}{1.2} +$ &c.
or, representing $\frac{\mathrm{D}^n \varphi x}{1.2.3 \ldots n}$ by $\underset{c}{\mathrm{D}}^n \varphi . x$,
$\varphi(x+o) = \varphi x + \mathrm{D}\varphi x . o + \underset{c}{\mathrm{D}}^2 \varphi x . o^2 + \underset{c}{\mathrm{D}}^3 \varphi x . o^3 +$ &c.

To resume the demonstration:

$$\frac{1}{1+\alpha x} + \frac{1}{1+\beta x} = \frac{2+(\alpha+\beta)x}{1+(\alpha+\beta)x+\alpha\beta x^2} = \frac{2+bx}{1+bx+cx^2},$$

now $\frac{1}{1+\alpha x} = 1 - \alpha x + \alpha^2 x^2 \ldots\ldots \pm \alpha^n x^n \ldots$

and $\frac{1}{1+\beta x} = 1 - \beta x + \beta^2 x^2 \ldots\ldots \pm \beta^n x^n$

$\therefore$ term affected with x^n in developement of $\frac{1}{1+\alpha x} + \frac{1}{1+\beta x}$ is $\pm \left\{\alpha^n + \beta^n\right\}$.

Again, $\frac{2+bx}{1+bx+cx^2} = (2+bx)(1+bx+cx^2)^{-1}$

now $(1+bx)^{-1} = 1^{-1} + \mathrm{D}1^{-1} . bx + \underset{c}{\mathrm{D}}^2 1^{-1} . b^2x^2 + \underset{c}{\mathrm{D}}^3 1^{-1} . b^3x^3 +$ &c.
for b put $b + cx$, and for b^m, $\{b + cx\}^m$, or $b^m + \mathrm{D}b^m . cx + \underset{c}{\mathrm{D}}^2 b^m . c^2x^2$ &c.
then

$$(1+bx+cx^2)^{-1} = 1^{-1} + \mathrm{D}1^{-1} . bx + \mathrm{D}1^{-1} . bx . cx$$
$$+ \underset{c}{\mathrm{D}}^2 1^{-1} . b^2x^2 + \underset{c}{\mathrm{D}}^2 1^{-1} . \mathrm{D}b^2 . cx^3 + \underset{c}{\mathrm{D}}^2 1^{-1} c^2x^4 \text{ \&c.}$$
$$+ \underset{c}{\mathrm{D}}^3 1^{-1} . b^3x^3 + \underset{c}{\mathrm{D}}^3 1^{-1} . \mathrm{D}b^3 . cx^4 \text{ \&c.}$$
$$+ \underset{c}{\mathrm{D}}^4 1^{-1} . b^4x^4 \text{ \&c.}$$

Hence, terms affected with x^n and x^{n-1} are

$$\underset{c}{\mathrm{D}}^n 1^{-1} . b^n + \underset{c}{\mathrm{D}}^{n-1} 1^{-1} . \mathrm{D}b^{n-1} c + \underset{c}{\mathrm{D}}^{n-2} 1^{-1} . \underset{c}{\mathrm{D}}^2 b^{n-2} c^2 +$$
$$\underset{c}{\mathrm{D}}^{n-3} 1^{-1} . \underset{c}{\mathrm{D}}^3 b^{n-3} c^3 + \text{ \&c.}$$

and $\underset{c}{\mathrm{D}}^{n-1} 1^{-1} . b^{n-1} + \underset{c}{\mathrm{D}}^{n-2} 1^{-1} . \mathrm{D}b^{n-2} c +$ &c. Now, the mth term from the beginning in first series, is $\underset{c}{\mathrm{D}}^{n-m}\,1^{-1}\,.\,\underset{c}{\mathrm{D}}^{m}\,b^{n-m}\,c^{m}$;

which, n even and $m = \frac{n}{2} = \underset{c}{\mathrm{D}}^{m}\,1^{-1}\,.\,c^{m}$

n odd and $m = \frac{n-1}{2} = \underset{c}{\mathrm{D}}^{m+1}\,1^{-1}\,.\,\overline{m+1}\,b.$

At these terms the series terminates; all the succeeding terms being equal 0, since $\mathrm{D}^{m+1}\,b^{m}.\ \mathrm{D}^{m+2}\,b^{m-1}$, are respectively $= m\,.\,\overline{m-1}\,\overline{m-2}\ldots 3.2.1.0 = \overline{m-1}\,\overline{m-2}\ldots 3.2.1.0$ and $=0$.

Hence, the series written in a reverse order is (n even)

$$\underset{c}{\mathrm{D}}^{m}1^{-1}.c^{m} + \underset{c}{\mathrm{D}}^{m+1}1^{-1}.\underset{c}{\mathrm{D}}^{m-1}.b^{m+1}.c^{m-1} + \underset{c}{\mathrm{D}}^{m+2}1^{-1}.\underset{c}{\mathrm{D}}^{m-2}b^{m+2}.c^{m-2}\ldots\ \underset{c}{\mathrm{D}}^{n}1^{-1}.b^{n}$$

(n odd)

$$\text{or } \underset{c}{\mathrm{D}}^{m+1}1^{-1}\,.\,\underset{c}{\mathrm{D}}^{m}\,b^{m+1}.c^{m} + \underset{c}{\mathrm{D}}^{m+2}1^{-1}\,.\,\mathrm{D}^{m-1}b^{m+2}.c^{m-1} + \&c.\ \ldots\ldots\ldots\ \underset{c}{\mathrm{D}}^{n}1^{-1}\,.\,b^{n}$$

Now, $\underset{c}{\mathrm{D}}^{n}1^{-1} = \frac{-1\,.\,-2\,.\,-3\ldots -n}{1\,.\,2\,.\,3\ldots n} = 1$ (n even) or $= -1$ (n odd)

and ∴ the former series becomes

$\pm b_{n} \pm \mathrm{D}\,b^{n-1}.c \mp \underset{c}{\mathrm{D}}^{2}\,b^{n-2}.c^{2} \pm$ &c. and consequently, the term affected with x^{n} in $(2 + bx)\,(1 + bx + cx^{2})^{-1}$

$$\text{is}\left\{\begin{array}{llll} \pm\, 2\,b^{n} & \mp\, 2\,\mathrm{D}\,b^{n-1}.c & \pm\, 2\,\underset{c}{\mathrm{D}}^{2}\,b^{n-2}.c^{2} & \mp\ \&c. \\ \mp\ \ b^{n} & \pm\ \ \mathrm{D}\,b^{n-2}.bc & \mp\ \ \underset{c}{\mathrm{D}}^{2}\,b^{n-3}.bc^{2} & \mp\ \&c. \end{array}\right\}$$

$$\text{or} \pm b^{n} \mp \frac{n}{n-1}\,\mathrm{D}\,b^{n-1}.c^{2} \pm \frac{n}{n-2}\,\mathrm{D}^{2}\,b^{n-2}.c^{3} \mp \frac{n}{n-3}\,\mathrm{D}^{3}\,b^{n-3}.c^{3} \pm \&c.$$

$$\left\{\begin{array}{l} \qquad\text{for, since } \underset{c}{\mathrm{D}}^{m}\,b^{n-m-1} \times b = \frac{n-2\,m}{n-m}\,\underset{c}{\mathrm{D}}^{m}\,b^{n-m} \\ \therefore \pm 2\,\underset{c}{\mathrm{D}}^{m}\,b^{n-m} \mp \underset{c}{\mathrm{D}}^{m}\,b^{n-m-1} \times b = \frac{n}{n-m}\,\underset{c}{\mathrm{D}}^{m}\,b^{n-m} \end{array}\right\}$$

Hence, $\alpha^{n} + \beta^{n} = b^{n} - \frac{n}{n-1}\,\mathrm{D}\,b^{n-1} + \frac{n}{n-2}\,\underset{c}{\mathrm{D}}^{2}\,b^{n-2} -$ &c. c being $= \alpha\beta = 1$.

The law of the series is truly and unambiguously represented, by means of the symbol or note of derivation D; but, if it is required to express the law numerically, in terms of n, since

$$\underset{c}{\mathrm{D}}^{m}\, b^{n-m} = \frac{(n-m)\; n-m-1)\;(n-m-2)\;\ldots\ldots\;(n-2m+1)}{1\,.\,2\,.\,3\;\ldots\ldots\ldots\ldots\;m}\, b^{n-2m}$$

$$\alpha^{n}+\beta^{n}=b^{n}-n\,b^{n-2}+\frac{n\,.\,n-3}{1\,.\,2}\,b^{n-4}-\frac{n\,.\,(n-4)\;(n-5)}{1\,.\,2\,.\,3}\,b^{n-6}+\text{\&c.}$$

the series for the chord of the supplement of a multiple arc, in terms of the chord (b) of the supplement of the simple arc.

XVIII. Similar series may be found for the sines and cosines of multiple arcs; thus,

$$\cos.z=2^{-1}\left\{\varepsilon^{z\sqrt{-1}}+\varepsilon^{-z\sqrt{-1}}\right\},\ \cos.nz=2^{-1}\left\{\varepsilon^{nz\sqrt{-1}}+\varepsilon^{-nz\sqrt{-1}}\right\}.$$

Now, $\alpha=\varepsilon^{z\sqrt{-1}}\ \therefore\ \alpha^{n}=\varepsilon^{nz\sqrt{-1}}$. Let $\cos.z=p$,

$$\therefore\ \alpha+\beta=2p=b,$$

$$\therefore\ \cos.nz=\frac{\alpha^{n}+\beta^{n}}{2}=\tfrac{1}{2}.\left(2^{n}p^{n}-n\,.\,2^{n-2}p^{n-2}+\frac{n\,.\,n-3}{1\,.\,2}\,2^{n-4}p^{n-4}-\text{\&c.}\right)$$

$$=2^{n-1}p^{n}-n\,.\,2^{n-3}p^{n-2}+\frac{n\,.\,n-3}{1\,.\,2}\,2^{n-5}p^{n-4}-\text{\&c.}\quad\ldots\ldots$$

$$\text{or}=\tfrac{1}{2}\left\{(2p)^{n}-n\,.\,(2p)^{n-2}+\frac{n\,.\,n-3}{1\,.\,2}\,(2p)^{n-4}-\text{\&c.}\right\}$$

Suppose it were required to write the series in an inverse order: let n be even, then the series $b^{n}-\frac{n}{n-1}\,\mathrm{D}\,b^{n-1}$ &c. terminates at a term $\frac{n}{n-m}\,\underset{c}{\mathrm{D}}^{m}\,b^{n-m}$, $m=\frac{n}{2}$, and $\therefore\ \frac{n}{n-m}=2$, and

$$\alpha^{n}+\beta^{n}=\pm 2\mp\frac{n}{n-m+1}\,\underset{c}{\mathrm{D}}^{m-1}\,b^{n-m+1}\pm\frac{n}{n-m+2}\,\underset{c}{\mathrm{D}}^{m-2}\,b^{n-m+2}\mp\text{\&c.}$$

or, in terms of n,

$$=\pm 2\mp\frac{n\,.\,n}{1\,.\,2\,.\,2}\,b^{2}\pm\frac{\left(n\,.\,\frac{n}{2}\,.\,\frac{n}{2}+1\,.\,\frac{n}{2}-1\right)}{1\,.\,2\,.\,3\,.\,4}\,b^{4}\mp\text{\&c.}$$

Consequently, $\cos.nz=\frac{\alpha^{n}+\beta^{n}}{2}=\pm 1\mp\frac{n^{2}p^{2}}{1\,.\,2}\pm\frac{n^{2}\,.\,(n^{2}-4)\,p^{4}}{1\,.\,2\,.\,3\,.\,4}$ &c.

Where the upper or lower sign takes place, as n is of the form $4s$, (s an even or odd number), or $2s$, (s an odd number); let n be odd, then the series terminates at a term $\frac{n}{n-m}\,\underset{c}{\mathrm{D}}^{m}\,b^{n-m}$,

$$m=\frac{n-1}{2},\ \text{and}\ \therefore\ \frac{n}{n-m}\,.\,\underset{c}{\mathrm{D}}^{m}\,b^{n-m}=n\,b,$$

$$\text{and}\ \alpha^{n}+\beta^{n}=\pm n\,b\mp\frac{n}{n-m+1}\,\underset{c}{\mathrm{D}}^{m-1}\,b^{n-m+1}\pm\text{\&c.}$$

or in terms of n

$$= \pm n b \mp \frac{n \,.\, (n^2 - 1)}{2^2 . 1 \,.\, 2 \,.\, 3} b^3 \pm \frac{n \,.\, (n^2 - 1)\ (n^2 - 9)}{2^4 \,.\, 1 \,.\, 2 \,.\, 3 \,.\, 4 \,.\, 5} b^5 \mp \&c.$$

Consequently,

$$\cos.\, n z = \frac{\alpha^n + \beta^n}{2} = \tfrac{1}{2} \left\{ \pm n b \mp \frac{n.(n^2-1)}{1 \,.\, 2 \,.\, 3} \frac{b^3}{2^2} \pm \frac{n.(n^2-1)\ (n^2-9)}{1.2.3.4.5} \frac{b^5}{2^4} \right\}$$

$$= \quad \pm n p \mp \frac{n.(n^2-1)}{1 \,.\, 2 \,.\, 3} p^3 \pm \frac{n.(n^2-1)\ n^2-9}{1 \,.\, 2 \,.\, 3.4.5} p^5 - \&c.$$

Where the upper or lower sign is to be used, as n is of the form $(4 s + 1)$, or $4 s + 3$.

XIX. Again, sine $z = (2 \sqrt{-1})^{-1} \left\{ \varepsilon^{z\sqrt{-1}} - \varepsilon^{-z\sqrt{-1}} \right\}$,
sine $n z = \left\{ 2 \sqrt{-1} \right\}^{-1} \left\{ \varepsilon^{n z\sqrt{-1}} - \varepsilon^{-n z\sqrt{-1}} \right\}$,
$\therefore$ it is necessary to find $\alpha^n - \beta^n$ in terms of $\alpha - \beta$.
Let n be odd,
then term affected with x^n in developement of $\left\{ \frac{1}{1 - \alpha x} + \frac{1}{1 + \beta x} \right\}$
$= \alpha^n - \beta^n$, and $\frac{1}{1 - \alpha x} + \frac{1}{1 + \beta x} = \frac{2 - (\alpha - \beta)\ x}{1 - (\alpha - \beta)\ x - \alpha \beta x^2} = \frac{2 - b x}{1 - b x - c x^2}$,
and the term affected with x^n in the developement of $(2 - b x)$
$(1 - b x - c x^2)^{-1} = b^n + \frac{n}{n-1} \mathrm{D} b^{n-1} . c + \frac{n}{n-2} \underset{c}{\mathrm{D}}^{2} b^{n-2} c^2 - \&c.$
or in terms of n $(c = 1)$

$$= b^n + n b^{n-2} + \frac{n \,.\, (n-3)\ (n-4)}{1 \,.\, 2} b^{n-4} - \&c.$$

but sine $z = \frac{b}{2 \sqrt{-1}} = p \therefore (b)^n = (2 \sqrt{-1}\, p)^n = \pm 2^n \sqrt{-1}\, p^n$,
where the upper or lower sign is to be used, according as n is of the form $4 s + 1$, or $4 s + 3$. Hence,

$$\text{sine}\, n z = \frac{\alpha^n - \beta^n}{2 \sqrt{-1}} = \pm 2^{n-1} p^n \mp 2^{n-3} . n p^{n-2} \pm 2^{n-5} . \frac{n.(n-3)\ (n-4)}{1 \,.\, 2 \,.\, 3} p^{n-4}$$

&c.

If it is required to write the series in a reverse order, it is to be observed, that the series $b^n + \frac{n}{n-1} \mathrm{D}\, b^{n-1}$ &c. terminates at a term $\frac{n}{n-m}\underset{c}{D}^m b^{n-m}$, $m=\frac{n-1}{2}$ $\therefore$ $\frac{n}{n-m}\underset{c}{D}^m b^{n-m}=\frac{nb}{2}$; consequently,

$$\alpha^n-\beta^n=\frac{nb}{2}-\frac{n}{n-m+1}\underset{c}{D}^{m-1}\,b^{n-m+1}\ \&c.$$

or in terms of n,

$$=\frac{nb}{2}+\frac{n.(n+1)\,(n-1)}{1.2.3}\cdot\frac{b^3}{2^2}+\frac{n.(n+1)\,(n-1)\,(n+3)\,n-3}{1.2.3.4.5}\cdot\frac{b^5}{2^4}\ \&c.$$

Hence, $\frac{\alpha^n-\beta^n}{2\sqrt{-1}}(\text{sine } nz)=p-\frac{n.(n^2-1)}{1.2.3}-p^3+\&c.$

XX. Let n be even, then term affected with x^n in developement of $\left\{\frac{1}{1-\alpha x}-\frac{1}{1+\beta x}\right\}=\alpha^n-\beta^n$.

Now $\frac{1}{1-\alpha x}-\frac{1}{1+\beta x}=\frac{(\alpha+\beta)x}{1-(\alpha-\beta)x-\alpha\beta x^2}$, and the term affected with x^{n-1} in the developement of $(1-bx-cx^2)^{-1}$ is $b^{n-1}+\text{D}\,b^{n-2}c+\underset{c}{D}^2 b^{n-2}c^2+\underset{c}{D}^3 b^{n-3}c^3+\&c.$

$\therefore$ term affected with x^n in $b^1 x\{1-bx-cx^2\}^{-1}$, $\{\alpha+\beta=b^1\}$ is $b^1\{b^{n-1}+\text{D}\,b^{n-2}c+\underset{c}{D}^2 b^{n-3}c^2+\&c.\}$

or in terms of n $(c=1)$

is $b^1\left\{b^{n-1}+\{n-2\}b^{n-3}+\frac{(n-3)\,n-4}{1.2}b^{n-5}-\&c.\right\}$

Hence, since sine $z=\frac{\alpha+\beta}{2\sqrt{-1}}=\frac{b}{2\sqrt{-1}}=p$ $\therefore$ $\frac{b^{n-1}}{\sqrt{-1}}=\pm 2^{n-1}p^{n-1}$,

and cosine $z=\frac{\alpha-\beta}{2}=\frac{b^1}{2}=p^1$ $\therefore$ sine $nz=\frac{\alpha^n-\beta^n}{2\sqrt{-1}}$

$$=p^1\left\{\pm 2^{n-1}p^{n-1}\mp 2^{n-3}.(n-2)p^{n-3}\pm\frac{2^{n-5}.(n-3)\,n-4}{1.2}\right\}\times$$

or $=p^1\{\pm(2p)^{n-1}\mp(n-2)\,(2p)^{n-3}\pm\&c.\}$

$\times\,p^{n-5}\mp$ &c. the upper signs taking place, if n is of the form $2s$ (s odd), the lower, if n is of the form $4s$, s even or odd.

If it is required to write the series in a reverse order, it is to be observed, that the series $b^{n-1}+\text{D}b^{n-2}+\&c.$ terminates at a term $\underset{c}{D}^m b^{n-m-1}$, when $m=\frac{n}{2}-1$; consequently, $\underset{c}{D}^m b^{n-m-1}=\frac{nb}{2}$, and $\therefore$ $\alpha^n-\beta^n=b^1\left\{\frac{nb}{2}+\underset{c}{D}^{m-1}b^{n-m}+\&c.\right\}$

$$=b^{1}\left\{\frac{nb}{2}+\frac{\left(\frac{n}{2}+1\right)\frac{n}{2}.\left(\frac{n}{2}-1\right)}{1.2.3}b^{3}+\text{\&c.}\right\}$$

$$=b^{1}\left\{\frac{nb}{2}+\frac{n\,(n^{2}-4)}{1.2.4}.\frac{b^{3}}{2^{3}}-\text{\&c.}\right\}$$

consequently, sine nz or $\frac{\alpha^{n}-\beta^{n}}{2\sqrt{-1}}$

$$=p^{1}\left\{np-\frac{n\,.\,(n^{2}-4)}{1.2.3}p^{3}+\frac{n\,.\,(n^{2}-4)\;n^{2}-9)}{1.2.3.4.5}p^{5}-\text{\&c.}\right\}$$

XXI. The sine nz (n even) may be expressed by series, in terms of the cosine of z;

thus, $\frac{1}{1-\alpha x}-\frac{1}{1-\beta x}=\frac{\{\alpha-\beta\}x}{1-\alpha+\beta x+cx^{2}}$,

and, equating the terms affected with x^{n} in each developement, we shall have

$$\sin.\,nz=p\left\{(2p^{1})^{n-1}-\frac{n-2}{1}(2p^{1})^{n-3}+\frac{(n-3)\,(n-4)}{1.2}(2p^{1})^{n-5}-\text{\&c.}\right\}$$

when n is even, a series may be found for sin. nz in terms of p (sin. z) only; but this series will not terminate as all the foregoing series do.

To find this series, expand $\sqrt{(1-p^{2})}=p^{1}$ into a series,

$$1^{\frac{1}{2}}-D\,1^{\frac{1}{2}}p^{2}+\underset{C}{D}^{2}\;1^{\frac{1}{2}}p^{4}-\text{\&c.}$$

then $\sin.\,nz=\left\{1-D\,1^{\frac{1}{2}}p^{2}+\underset{C}{D}^{2}\,1^{\frac{1}{2}}p^{4}-\text{\&c.}\right\}\times\left\{np-\frac{n.(n^{2}-2^{2})}{1.2.3.}p^{3}+\text{\&c.}\right\}$

$$=np+Ap^{3}+A_{,}p^{5}+A_{,,}p^{7}+\text{\&c.}$$

in which series, the law of the coefficients, or a general expression for $A_{,,\ldots n}$ may be found. But it cannot now be done, without too long a digression from the present objects of inquiry.

From what has been done, the series* of the chord of the

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* Demonstrations of these forms have been given by reversion of series, and by induction; which demonstrations are imperfect, since they do not exhibit the general law of the coefficients. See DE MOIVRE Miscell. analytica. Epistola de COTESII Inventis, &c. NEWTONI Opera omnia. p. 306. EULER in Analyt. inf. Cap. 14. WARING has deduced the chord of the supplement of a multiple arc, in terms of the chord of the supplement of the simple arc, from his theorem for the powers of roots:

multiple arc may be found in terms of the chord of the simple arc; for, chord $nz = \left\{\sqrt{-1}\right\} \left\{ \varepsilon^{\frac{nz}{2}\sqrt{-1}} - \varepsilon^{-\frac{nz}{2}\sqrt{-1}} \right\}$.

XXII. In the above demonstrations, no formulas are borrowed from geometry; and the general law of the coefficients is clearly expressed; it is, I think, most conveniently expressed by means of the symbol or note of derivation D. The operation which this symbol indicates is as certain as any other operation, whether arithmetical or algebraical.

XXIII. The demonstrations and method of deduction given in this paper shew, I think, with sufficient evidence, the introduction of geometrical expressions and formulas into analytical investigation to be perfectly unnecessary. It has appeared likewise, that such introduction embarrasses investigation, and causes ambiguity, by concealing the true derivation of expressions; and, although I do not wish to give importance to my own observations, by supposing a greater confusion of notion to exist than really does, yet, I think, in what has been written and said, there may be detected a lurking opinion, that the value of certain expressions essentially demand the existence of geometrical curves and figures, and the investigation of their properties.

XXIV. In the Appendix to the *Arithmetica Universalis*, p. 200. 219. &c. Newton, with great clearness and force of argument, has shewn the distinction to be made between the order of classing curves, analytically considered, that is, defined

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but the demonstration of the latter theorem is not, it appears to me, to be reckoned in the number of strict demonstrations. The only objection against the demonstration of the very learned and ingenious author of the Calcul des Derivations, is, that it is rather indirect, and blended with geometrical expressions and formulas.

by equations, and the order of classing them, considered as generated by description. Moreover, he animadverts on the custom of confounding the two sciences of algebra and geometry;* and, if any authority is attached to his assertion, that the two sciences ought not to be confounded together, the separation of geometry from algebra will thereby be equally urged as the separation of algebra from geometry. And it cannot be said with greater truth, that the simplicity of geometry is vitiated with algebraic equations, than that the simplicity of analysis is vitiated with geometrical forms and expressions. In fact, each science ought to be kept distinct; and be made to derive its riches from its proper sources.

XXV. It will not demand much meditation to be assured of this truth, that, in any mathematical investigation, the geometrical method, properly so called, is not essentially or absolutely necessary. The properties of extension and figure, to which this method has been especially appropriated, may be analytically treated; and here it is proper to state a distinction necessary to be made, between what may be called analytical geometry, and the application of analysis to geometry. The first does not suppose or require the existence of such a method as the geometrical; but, from a few fundamental principles, analytically investigates the properties of extension; whereas, in the latter, analysis is applied to propositions already established by the geometrical method: so that, strictly, to shew the justness and propriety of the application, a separate investigation is

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* "Multiplicationes, divisiones, et ejusmodi computa, in geometriam recens intro-
" ducta sunt: idque inconsulto, et contra primum institutum scientiæ hujus, &c.
" —— Proinde hæ duæ scientiæ confundi non debent, &c.—— Et recentes, utramque
" confundendo, amiserunt simplicitatem in quâ geometriæ elegantia omnis consistit."

necessary. We find, however, in general, a vague analogy substituted, as a connecting principle between the two methods.

XXVI. The application of algebra to geometry, gives to DESCARTES the fairest title to fame for mathematical invention; yet the cause and nature of the benefit conferred on science by that application, seems to be indistinctly apprehended.* For, the Analytical Calculus, when applied to geometry, was not enriched with the truths of the latter science, because some connecting principle had been discovered, or some process invented by which the property of the two methods became common, and might, from one to the other, without formality be transferred; but because the investigation of certain properties could not proceed, without first improving the means by which they were to be investigated. These means DESCARTES improved: he found, when certain conditions in problems concerning extension were translated into the language of algebra, that the process of deduction with the general terms was slow and incommodious, because, such was the low state of the algebraic Calculus, the relation between the general terms had not been established. The aim and merit of DESCARTES's speculations is to have established this relation. If illustration were needed to make my meaning clear, I should say that DESCARTES, NEWTON, and D'ALEMBERT, benefited science precisely after the same manner. The first applied the analytical Calculus to extension; the second to motion; the third to the equilibrium, resistance, &c. of fluids. As the object of investigation became

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* Thus far was the Analytical Calculus benefited by the existence of the geometrical method: certain properties of figure and extension, discovered by the latter, became to the former, objects of investigation.

more abstruse, it was found necessary to improve more and more the means or instrument of investigation.

XXVII. As the question concerning the respective advantages of the ancient geometry and modern analysis, is not foreign to the subject of this Paper, I shall briefly state it, and endeavour to afford the means of arriving therein at something like a precise determination.

The superiority of one method above another, must consist in being either more logically strict in its deductions, or more luminous, or more commodious for investigation. The discussion concerning the strictness and accuracy may, I conceive, be immediately put aside, since no method of deduction is essentially inaccurate; and, if in geometry the inferences are more strictly deduced than in the algebraic Calculus, the advantage is to be reckoned an accidental one, and arising from the great attention with which the former science has been cultivated.

One method may, however, be essentially more perspicuous and more commodious for investigation than another; or, in other words, the perspicuity and commodiousness of a method may depend on circumstances inherent in its nature and plan. Now, a person not sensible of the superior perspicuity of the geometrical method, would demand these circumstances, the necessary causes of perspicuity, to be pointed out to him; which might be done, by stating that geometry, instead of a generic term, employs, as a particular individual, the sign or representative of a genus; and that, as in algebra, the signs are altogether arbitrary, in geometry, they bear a resemblance to the things signified, and are called *natural* signs, since the figure of a triangle, or square, suggests to the mind the same tangible figure in Europe, that it does in America: and this resemblance of the sign to the thing signified, is supposed to be the chief cause of the superior clearness of geometrical demonstration.* Another cause may perhaps be thought to exist in this circumstance, that whatever is demonstrated, of a triangle or other diagram, considered as the representative of all triangles and diagrams, is moreover demonstrated of that individual triangle or diagram. A third, and more satisfactory cause than the last, may be, that in investigation, for the purpose of preventing ambiguity and mistake, it is frequently necessary to recur from the sign to the thing signified; which is more easily done, the less general and arbitrary the modes of representation are; and, consequently, in geometry more easily than in algebra.

I do not pretend to have assigned, accurately, and all, the causes of perspicuity of geometrical reasoning. It may depend on certain intellectual acts and processes, which it is beyond the power of philosophy to explain. The circumstance, however, of the signs employed in geometry being *natural* signs, will prove its perspicuity only to a certain extent, and in certain cases. It must fail to prove it, when the properties of solids are treated geometrically; because the representation of solids on a plane by diagrams, is not a natural representation, that is, would not suggest to all minds the same tangible portion of extension.

It must fail likewise to prove it, in questions concerning radii of curvature, areas of curves, &c. or in all questions in which the fluxionary or differential Calculus is usually employed. The

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* Does there not, however, here arise a consideration that takes away from the cause of the perspicuity of geometrical demonstration? For the reasoning with a diagram cannot be generally true, except the diagram be considered abstractedly, and independent of those peculiar and distinguishing properties that determine its individuality.

lines and mixtilinear triangles therein exhibited cannot be called natural signs, since they are only imperfect and inadequate representations of other imaginary lines and triangles, of which the mind must form what notion it can. Not, however, to infer want of perspicuity from inefficiency in the cause assigned, if we employ the geometrical method, or view its employment in investigation, concerning motion, curves, &c. it will not appear a perspicuous method; and, if instances of its obscurity were required of me, I could find them, even in the immortal work of the Principia. Whether we consider the fact, or speculate about the cause, I think the geometrical method can only be allowed to have superior evidence in investigations of a simple nature.

That the analytical calculus is more commodious for the deduction of truth than the geometrical, will not perhaps be contested; and, an examination into its nature, would shew why it is so well adapted for easy combination and extensive generalization. No language like the language of analysis, one of the greatest of modern mathematicians has observed, is capable of such elegance as flows from the developement of a long series of expressions connected one with the other, and all dependent on the same fundamental idea.

If we view what has been respectively done by each method, in the explanation of natural phenomena, the superiority of the one above the other will appear immense: yet the cultivators of geometry were men of consummate abilities, and possessed this great advantage, that the method or instrument of thought and reasoning which they employed had, during preceding times, received the greatest improvement. The analytical calculus, which has verified the principle of gravitation, was a hundred years ago in its infancy.

The question, then, concerning the respective advantages of the ancient geometry and modern analysis, may be comprised within a short compass. If mental discipline and recreation are sought for, they may be found in both methods; neither is essentially inaccurate; and, although in simple inquiries the geometrical has greater evidence, in abstruse and intricate investigation the analytical is most luminous: but, if the expeditious deduction of truth is the object, then I conceive the analytical calculus ought to be preferred. To arrive at a certain end, we should surely use the simplest means; and there is, I think, little to praise or emulate, in the labours of those who resolutely seek truth through the most difficult paths, who love what is arduous because it is arduous, and in subjects naturally difficult toil with instruments the most incommodious.

XXVIII. If in matters of abstract science deference is ever due to authority, it must be paid to that by which the study and use of the method of the ancients has been recommended. Newton has, however, brought forward no precise arguments in favour of synthesis; and it is easy to conceive, that he would be naturally attached to a method long known and familiar to him,[*] and by means of which he was enabled to connect his own theory of curvilinear motions, with the researches of the ancients on conic sections, and with Huygens's discoveries relative to central forces and the evolutes of curves.

The very ingenious and learned Matthew Stewart[†] endea-

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[*] The circumstance of mathematicians having acquired a considerable dexterity in the management of the geometrical method, seems to be the reason why they endeavoured to explain the doctrine of logarithms (a subject purely algebraical) by the introduction of the properties of curves.

[†] Words are frequently stated in a delusive and imposing manner, not always

voured to shew, that the geometrical calculus was competent to the explanation of natural phenomena; and with astonishing perseverance applied it to many investigations in physical astronomy. The labours of such a man are not hastily to be judged: yet every one must determine for himself; and to me it seems, his reasonings, from their intricacy, call up so great a *contention of the mind,* that they prove, in no small degree, the unfitness of the geometrical method in all abstruse and intricate investigations.

XXIX. It may, however, be asked, are not there some subjects of inquiry to which the geometrical method is better adapted than the analytical? and is not the theory of angular functions one of these subjects?* I apprehend not: for, if the conditions

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intentionally. Dr. STEWART, (Preface to Sun's Distance,) and after him his ingenious biographer, for the purpose of holding up the superior simplicity of the geometrical calculus, has said, that in order to understand his solution, a knowledge of the elements and conic sections only is requisite. But, in fact, the solution is effected by proposition heaped on proposition; and with equal truth and justness it might be said, that in order to understand the analytical solution, a knowledge only of common algebra is requisite; since the methods by which the solution is effected, are really and properly branches of algebra.

[*] D'ALEMBERT says, "there are cases in which analysis, instead of expediting, embarasses demonstration. These cases happen in the computation of angles: for angles can analytically be expressed only by their sines; and the expression of the sines of angles is often very complicated," &c. He adds, "that it must depend on mathematicians, whether the method of the ancients or the modern analysis is to be employed, since it would be difficult to give on this head exact and general rules." In the very case adduced, I think demonstration expedited by the analytical calculus; and, although $2^{-1}\left\{\varepsilon^{x\sqrt{-1}}+\varepsilon^{-x\sqrt{-1}}\right\}$ is not so speedily put down as cos. x; yet all processes of evolution, differentiation, integration, &c. are much more easily performed with the former expression than with the latter. Other instances of subjects of inquiry, to which the geometrical method is said to be peculiarly well adapted, have been adduced; but I still find no convincing reason, why a mathematician must submit to the necessity of

can be adequately and unambiguously stated in the general terms of algebra, then deduction with such terms may be strictly made, and expeditiously; since it is to be made according to a known and established process. I have shewn at some length, that reasoning may be conducted with terms which separately cannot be arithmetically computed: for the mere process of deduction, it is not necessary to have distinct and complete notions of the things signified by the general terms.

The principal object of the present paper is to shew, that the analytical calculus needs no aid from geometry, and ought to reject it, relying entirely on its own proper resources. By this means, it would gain perspicuity, precision, and conciseness; advantages not to be lightly estimated, by any one who has a regard to certainty and demonstration, or considers the bulk to which scientific treatises have of late years swelled.

In order to prove and illustrate the opinion I wished to establish, I directed my search to those cases which have been always thought to require the aid of the geometrical method. By a purely analytical process, I have traced the origin and derivation of certain fluxionary expressions, usually referred to logarithms and circular arcs. I have given demonstrations of the series for the sine of an arc in terms of the arc; of the analytical formula for the root of a cubic equation in the irreducible case; of the resolution of $x_n \mp a_n$ into quadratic factors; of the series for the chord, &c. of a multiple arc in terms of the simple arc, &c. which demonstrations, with as much confi-

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learning half a series of truths by one method, and half by another. These considerations, however, depreciate the value of the geometrical method only in one point of view; for, after all, the finest exemplar of clear and accurate reasoning is contained in the works of Euclid.

dence as I dare assume, knowing how fallaciously we judge of our own performances, I affirm to be strict and direct; established without artifices, and without foreign aid drawn from geometrical theorems and the properties of curves. In some parts of this paper, the subjects, for their importance, may be thought to be too slightly discussed; the fear of appearing prolix, has perhaps driven me into brevity and obscurity. In other parts, what I have advanced may be remote from common apprehension, or contrary to received opinion: but here I make no apology; for, what I have written, has been written only after long meditation, and from no love of singularity. "If I cannot add to truth," I do not desire distinction from "the "heresies of paradox."

VI. *Observations and Experiments upon oxygenized* and hyperoxygenized muriatic Acid; and upon some Combinations of the muriatic Acid in its three States. By* Richard Chenevix, *Esq. F. R. S. and M. R. I. A.*

Read January 28, 1802.

When Mr. Berthollet made known the combination of what was then called oxygenated muriatic acid with potash, he gave as his opinion, that the proportion of oxygen, relatively to the quantity of acid, was greater in the salt than in uncombined oxygenized muriatic acid. This conjecture was fairly founded upon the observation, that, in his mode of preparing this salt, a large portion of common muriate was formed in the liquor, along with the hyperoxygenized muriate. The Memoir which he published in the year 1788, is the last with which I am acquainted, upon this subject. It does not contain any thing that, considering the accuracy which is now required in experiments, amounts to a demonstration of the relative proportions of oxygen, in oxygenized and hyperoxygenized muriatic acids. Unfortunately, this chemist has not pursued his researches any farther; although, from his own words, we had every reason to hope that they would have been continued.

In the *Système des Connoissances chimiques* of Mr. Fourcroy,

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* I have preferred this word to *oxygenated,* because *ate* is the appropriate termination of certain salts formed by the acids in *ic.* Some further remarks upon this subject will be made in a work now in the press, entitled *Remarks upon Chemical Nomenclature.*

we find a summary of the experiments that had preceded the impression of his work, together with the following sentence. "Tous les muriates suroxygenés sont décomposés par les acides, "souvent avec une violente décrepitation, avec une dégagement "de vapeur jaune verdâtre, et une odeur très-forte. Cette vapeur "est de véritable acide muriatique suroxygéné. Elle est lourde, "tombe en goutellettes d'un jaune vert, et forme des stries "comme de l'huile, sur les corps auxquels elle adhère." This assertion carries no confirmation along with it; and does not amount so near to proof as the position of the former chemist: so that, in fact, the existence of hyperoxygenized muriatic acid, and of its combination with potash, rests, at present, upon the conjecture of Mr. BERTHOLLET; a conjecture however which, as well as his whole dissertation upon the subject, bears all the marks of genius which so strongly characterise every production of that sagacious philosopher. Some notice has been taken of other saline combinations, formed by causing a current of oxygenized muriatic acid to pass through solutions of the alkalis, or earths, or by otherwise combining them. Mess. D'OLFUS, GADOLIN, VAN-MONS, LAVOISIER, and others, have slightly mentioned some of these combinations. But, with the exception of Mr. BERTHOLLET, I know of no chemist who has approached so near to the real state of the combination of muriatic acid and oxygen with potash, as Mr. HOYLE, of Manchester. The true nature of this salt, however, is one of those things which many persons have credited without proof; and which many others have been on the eve of discovering.

I shall now proceed to lay before the Society, an account of the observations and experiments which have led me to conclude, that muriatic acid does exist in the form of oxygenized and hyperoxygenized muriatic acid, as announced in the title of the present communication; and that, in either state, it is capable of entering into saline combinations.

With this view, I shall describe,

1st. The means by which I think I have succeeded, in ascertaining the constituent parts, as well as the proportions, in oxygenized and hyperoxygenized muriatic acid.

2dly. I shall mention some of the combinations of the muriatic acid, in its three states.

In treating upon the first of these objects, I must in some measure anticipate the second; and must suppose some things known, which are hereafter to be described. This inconvenience is inevitable; as the natural order of things leads me to treat of the acid, before I consider the bodies into the composition of which it enters.

I exposed to the heat of a lamp, 100 grains of hyperoxygenized muriate of potash. It decrepitated gently, and in a short time melted. After remaining in fusion nearly an hour, I allowed it to cool: it crystallized as formerly, and had lost 2,5 per cent.. I increased the heat to redness, in a furnace. The salt boiled with a violent effervescence, and rapid disengagement of gaseous fluid, together with a thin white vapour, and then sunk suddenly into a white spongy mass. The loss of weight usually varied from 42 to 48 or 50 per cent.

I put 100 grains into a coated glass retort, luted to a small and perfectly dry receiver, having a tube communicating with a glass bell in the pneumatic tub. The fire had not been lighted very long, when a slight dew began to line the inside of the receiver; and, as soon as the retort was nearly red hot, a disengagement of gas, so sudden as almost to be explosive, took place. A quantity of thin white vapour arose, which afterwards was deposited, in the form of a white sublimate, in the receiver and the tube. When the extrication of gas had ceased, the apparatus was allowed to cool. The gas, with the usual corrections of temperature and pressure, measured 112,5 cubic inches, = 38,3 grains. The 2,5 mentioned above, as the loss of this salt at a low heat, were water. 53,5 remained in the retort; and the white sublimate in the tube and receiver amounted to 5.

The products of this operation were therefore,

Water - - - -	2,5
Oxygen - - - -	38,3
Salt in the tube and receiver -	5
Salt remaining in the retort - -	53,5
	99,3.

To find the proportions of oxygen and muriatic acid, in hyperoxygenized muriatic acid, it now only remains to determine the sum of the quantities of muriatic acid, contained in the 53,5 of the retort and the 5 of the tube and receiver. The 53,5 gave, by nitrate of silver, a precipitate corresponding to 18,21; and the 5, a precipitate corresponding to 1,76; in all, 20 of muriatic acid. Therefore, 38,3 of oxygen, and 20 of muriatic acid, combine to form 58,3 of hyperoxygenized muriatic acid; or, 100 of hyperoxygenized muriatic acid contain, within a fraction,

Oxygen - - -	65
Muriatic acid - - - -	35
	100.

And the elements of hyperoxygenized muriate of potash, should be thus stated:

Oxygen -	38,3	hyperoxygenized muriatic acid - -	58,3
Muriatic acid	20		
		Potash - - -	39,2
		Water - - -	2,5
			100,0.

It may be observed, that the 53,5 of the retort did not yield the same proportion of acid as the 5 of the tube and receiver. The fact is, that all muriates lose a little of their acid at a red heat, as I shall presently mention more particularly; and the small loss was, in all probability, owing to a portion of acid disengaged by the heat to which the salt was necessarily exposed during the operation.

Having thus ascertained the proportion of oxygen in hyperoxygenized muriatic acid, by means of its combination with potash, a ready method occurred to arrive at the knowledge of that contained in oxygenized muriatic acid. For this purpose, I disposed in the following manner, a WOULFE's apparatus, consisting of three bottles, and connected with the pneumatic tub. In the first bottle, I put a solution of potash,* in about six parts of water. In the second, a solution of the same; but so dilute, as that no part of the salt, which might be formed, should crystallize during the operation. About twenty parts of water was the proportion there employed. In the third bottle, I put common carbonate of potash. Through this apparatus, I sent a current of oxygenized muriatic acid, disengaged by sulphuric acid, from a mixture of muriate of soda and black oxide of manganese, in the well known manner. Crystals of hyperoxygenized muriate of potash were formed in the liquor of the first bottle; and, as long as they remained, I was certain, from previous experiment, that no sulphuric or muriatic acid could pass into the second bottle. The current was continued, till the liquor of that bottle contained an excess of acid. The carbonate of potash, in the third bottle, absorbed the superabundant vapours; and the

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* Whenever potash, soda, barytes, an acid, an alkali, water, or the names of other substances are used without an epithet, they are meant to denote them in that state which is commonly called *pure*.

pneumatic apparatus was ready to collect any gases that might be evolved. By these means, I obtained, in the second bottle, a solution of whatever substance might result from the action of potash upon hyperoxygenized muriatic acid.

I took a portion of this liquor, which I shall call *entire liquor,** and distilled it to dryness in a glass retort, taking care to screen it from light. A tube from the receiver communicated with the pneumatic tub. My object was to ascertain, whether the change observed by Mr. BERTHOLLET, in the distribution of the elements of oxygenized muriatic acid, to form, with potash, a simple and a hyperoxygenized muriate, really took place among those elements themselves, independently of any absorption of oxygen from the atmosphere, or extrication of it from the salt. Nothing but some water, and a few inches of the dilated air of the vessels, passed into the receiver and the pneumatic apparatus; and I found, in the retort, a saline mass,† perfectly dry and crystallized. Hence it is evident, that the same quantity of oxygen as that formerly contained in the oxygenized muriatic acid, which had been united to the alkali, to form the total mass of salt, was now condensed, in that part which had become hyperoxygenized muriate.

To ascertain this quantity, I dissolved 100 grains of the entire salt in water, and precipitated by nitrate of silver. I thus obtained a quantity of muriate of silver, which, by proportions previously determined, I knew to correspond to 84 of muriate

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* I am well aware that, upon philosophical principles, this appellation is objectionable; but, for the sake of brevity, I have used it as a temporary name, for a substance which has only a relative existence among chemical bodies.

† This salt, I shall call *entire salt.*

of potash: therefore, 16 were hyperoxygenized muriate of potash.* But, according to the proportions established above in hyperoxygenized muriate of potash, 16 of this salt contain 6 of oxygen, with 3,20 of acid, the remainder being alkali and water; and, by preliminary experiments, I found that 84 of muriate of potash contained 27,88 of muriatic acid. Therefore, 27,88 + 3,20 = 31,08 of muriatic acid, with 6 of oxygen, or, to reduce it to the quintal,

Muriatic acid - - -	84
Oxygen - - - -	16
	100, are the proportions

which combine to form oxygenized muriatic acid.

To corroborate this evidence, I distilled 100 grains of the entire salt mentioned above; and obtained nearly 16,5 cubic inches of oxygen gas; which as accurately corresponds with the trial by nitrate of silver, as can be expected in experiments of this nature.

Mr. BERTHOLLET, in his Memoir upon oxygenized muriatic acid, gives, if I understand him rightly, the following statement of the proportions, and of the means by which he obtained his results. He exposed to the light of the sun, 50 cubic inches of water, saturated with oxygenized muriatic acid; and collected in the pneumatic tub, 15 cubic inches of oxygen gas. I here neglect fractions; because our results appear, at first sight, to differ so widely as not to require great accuracy in giving their comparative statement. He then preci-

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* I must observe here, that hyperoxygenised muriate of potash does not, like simple muriate, decompose the salts of silver. This shall be further animadverted upon, and proved, in its proper place.

pitated, by nitrate of silver, the 50 cubic inches of liquor, which had become simple muriatic acid, and obtained 38,3 grains of muriate of silver. But, by experiments, I found that 38,3 of muriate of silver contain 65 of muriatic acid. Therefore, 65 of muriatic acid combine with 15 cubic inches* (= 8 grains) of oxygen, to form 73 of oxygenized muriatic acid. But 73 : 8 :: 100 : 11, or nearly. For this difference, however, it may be easy to account. Perhaps Mr. Berthollet's 50 cubic inches of oxygenized muriatic acid, contained originally a little simple muriatic acid; and he says besides, that he suspects all the oxygen was not disengaged. This indeed is most probable; and I am happy that I can reconcile the proportions which I have found, to the opinion of so skilful a chemist.

Mr. Cruikshank likewise, in his additional Observations upon Hydrocarbonates, has stated that 2,3 parts of oxygenized muriatic acid contain 1 of oxygen, or about 43,5 per cent. But this able chemist, to whom we are indebted for the discovery of the gaseous oxide of carbone, procured his oxygenized muriatic acid by a peculiar method, which I shall notice, in speaking of the action of acids upon hyperoxygenized muriate of potash. The substance he obtained was, in fact, not oxygenized muriatic acid gas, but a mixture of that gas with hyperoxygenized muriatic acid. I have not the smallest doubt of the accuracy of his statement; but, being the proportion of a mixture, it in no way contradicts either of those I have determined in this Paper.

Before I dismiss this part of the subject, I wish to anticipate an objection, founded upon an observation of Mr. Berthollet, which may be made to the above experiments. He says, that when the alkaline solution is very concentrate, an effervescence

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* Mr. Berthollet's proportions are in the old French weights and measures.

takes place during the whole of the saturation, and for some days after; and this effervescence, he attributes to the escape of oxygen. But I have already said, that no oxygen gas was disengaged in any part of my process; and no effervescence took place in any of the bottles, except the third; so that, no superabundance of oxygen could have passed from one into the other, nor could any diminution of the total quantity have been produced. By repeating the experiments, sometimes with a solution of alkali, and sometimes with water alone, in the first bottle, I obtained the liquor in the second bottle uniform in all cases. Indeed, as potash prepared in Mr. BERTHOLLET's manner, was not in such general use at the time he performed his experiments as at present, I suspect that a great part of this effervescence was owing to a disengagement of carbonic acid from the alkali.

Having thus proved the difference between the states of these two acids, I shall now proceed to the combination of each with salifiable bases.

OXYGENIZED MURIATES.

As many properties of the entire liquor, before it had been evaporated to dryness, had led me to imagine that the acid was united with the alkali, and remained in combination with it, in the state of oxygenized muriatic acid, till the moment of crystallization, I think it necessary to state at length the appearances which induced me to draw that conclusion, and the experiments which afterwards convinced me that it was erroneous.

A few drops of sulphuric acid, poured into some of the entire liquor, caused an effervescence, and a smell of hyperoxygenized muriatic acid.

Very strong acetic acid produced the same effect.

By other experiments, I had ascertained that acetic acid could not decompose any part of the entire salt; and hence I concluded, that in the entire liquor, before evaporation, some of the salt remained in the state of oxygenized muriate, the acid of which was expelled by the sulphuric or acetic acid; and, that it was not till the moment of crystallization, that the elements of the salt underwent a total resolution into muriate, and hyperoxygenized muriate, of potash. However, a small quantity of any of the very soluble neutral salts, such as nitrate or muriate of ammonia, or even a little alcohol, produced the same effects; and I was then convinced, that the effervescence was owing to some uncombined oxygenized muriatic acid gas, remaining in the liquor; and which was disengaged, in proportion as the water was taken from it, by the superior affinity of the salt, or the alcohol, I had used.

By some previous experiments, I had ascertained, as I have just mentioned, that acetic or acetous acids do not decompose hyperoxygenized muriate of potash. I sent a current of oxygenized muriatic acid through a solution of acetite of potash; and, upon evaporation, I found that the acetous acid had been disengaged, and that muriate, with hyperoxygenized muriate, of potash had been formed. But, from some trials, which I shall presently relate, I was induced to believe, that oxygenized muriatic acid attracts the salifiable bases with a much weaker affinity than acetous acid. It is well known that the contact of oxygenized muriatic acid with an alkali, is sufficient to produce a combination of that acid with the alkali; and, from the last-mentioned experiments it appears, that it is not absolutely necessary that the alkali should be in a free state. If it be combined with an acid weaker than hyperoxygenized muriatic acid, the original acid will be expelled; and muriate and hyperoxygenized muriate will be formed, as if the alkali had been free.

As a further proof, that the change in the distribution of oxygenized muriate of potash takes place at the moment of contact of the acid and the alkali, and consequently long before the crystallization, I mention the following experiments.

I precipitated, by nitrate of silver, 400 grains of the entire liquor, previously to its being evaporated; and obtained 71 grains of muriate of silver.

I evaporated to dryness, 400 grains of the same liquor, redissolved the residuum, and, by dropping in nitrate of silver, obtained 70 grains of muriate. The difference of one grain, in these experiments, does not amount to 0,2 of a grain of muriate of silver; and ought not to be regarded.

From these experiments, it is past all doubt, that the original entire liquor did not contain oxygenized muriate of potash. For, if such a combination had existed in it, I should have obtained a smaller portion of muriate of silver in the first than in the second case, on account of the total separation into muriate and hyperoxygenized muriate having not yet taken place.

We are not however to conclude, from these experiments, that there are no such things as oxygenized muriates. Although they cannot be exhibited in a palpable state, it is easy to demonstrate that they do really exist. I shall prove, in the proper place, that hyperoxygenized muriate of ammonia is not an incompatible combination; and must, for the present, assume the datum, in order that I may demonstrate the necessary existence of oxygenized muriates. Therefore: If muriatic acid, or if hyperoxygenized muriatic acid, be brought in contact with ammonia, the result will be muriate, or hyperoxygenized muriate, of ammonia. But, if the acid, disengaged by sulphuric acid, from a mixture of black oxide of manganese and muriate of soda, be sent through ammonia, both are decomposed. Hence it is evident, that the acid combines with the alkalis, in the state of oxygenized muriatic acid; and that the separation into muriate and hyperoxygenized muriate, is produced by a subsequent action, among the elements of oxygenized muriate of potash.

Upon the whole, it appears to me fair to conclude,

1st. That the salts of this genus do really exist, previous to the formation of hyperoxygenized muriate of potash.

2d. That the affinity exercised by hyperoxygenized muriatic acid for ammonia, and (by very strong analogy) for the other bases, is much greater than that of oxygenized muriatic acid. For, hyperoxygenized muriatic acid, as shall presently be shewn, having a much more powerful action upon all combustible bodies, whether simple or compound, than oxygenized muriatic acid, it would be natural to suppose that the former acid would act more powerfully upon the inflammable element of ammonia. But oxygenized muriatic acid combines with the hydrogen of that alkali; which, however, is not decomposed by hyperoxygenized muriatic acid; yet the affinity of hyperoxygenized muriatic acid for ammonia, is the only cause that determines the union of the acid and the alkali, without decomposition. But these affinities shall be more fully treated of, in speaking of hyperoxygenized muriate of ammonia.

ALKALINE AND EARTHY HYPEROXYGENIZED MURIATES.

Generic Characters.

Hyperoxygenized muriates are formed by passing a current of oxygenized muriatic acid through the basis, dissolved or suspended in water, as in the formation of the last mentioned genus. Their first formation is owing to the separation of the elements of an oxygenized muriate, into hyperoxygenized muriate and simple muriate; from which latter, they may be separated by crystallization, or by another process, which I shall mention, in treating of the earthy hyperoxygenized muriates. By simple trituration, they scintillate, with noise. They are decomposed by a low red heat; and give out a considerable quantity of oxygen, as they become simple muriates. They cannot be brought down, by any means that I have tried, to that diminished state of oxygenizement, which would constitute oxygenized muriates. They inflame all combustible bodies with violence, as is well known. They are soluble in water; many of them, in alcohol; and some are deliquescent. The acid is expelled, with particular phenomena, by sulphuric, nitric, and muriatic acids, without heat; and, a little below a boiling heat, by phosphoric, oxalic, tartareous, citric, and arsenic acids: but they are not acted upon by benzoic, acetic, acetous, boracic, prussic, or carbonic acids. Those vegetable acids which are powerful enough to decompose them, give out, towards the end, a gas of a peculiar nature, which has not so much smell as oxygenized muriatic acid gas, but which affects the eyes in an extraordinary manner, and promotes an uncommon and rather painful secretion of tears. I have not yet examined this gas, as there was invariably an inflammation of the mixture, with explosion and rupture of the vessels, almost as soon as it began to be evolved. When pure, the hyperoxygenized muriates do not precipitate any of the metallic salts, although I believe they decompose some. The order in which the bases seem to be attracted by the acid, is, potash, soda, barytes, strontia, lime, ammonia, magnesia, alumina, silica. The other earths I have not tried, and but few of the metallic oxides.

1st *Species. Hyperoxygenized Muriate of Potash.*

This salt is the best known of all the saline combinations of this acid. It has been erroneously considered as simply oxygenized, for its acid is really hyperoxygenized. It is soluble in about sixteen parts of cold water, but in much less of warm; and is easily separated, by crystallization, from muriate of potash. Alcohol can dissolve a small portion of it. It seems capable of existing in more states than one; for, in passing a current of oxygenized muriatic acid, very slowly, and in the dark, through a solution of potash, till saturated, I have obtained flexible and shining needle-like crystals. This leads me to suspect, either a hyperoxygenized muriate of potash with excess of acid, or that acid with a superaddition of oxygen. It would be superfluous to enter into a minute description of a substance so well known as hyperoxygenized muriate of potash; but, it being the substance whence I have chiefly attempted to disengage the acid, I shall enter into a particular detail of the action of the more powerful acids upon this salt.

If concentrate sulphuric acid be poured upon hyperoxygenized muriate of potash, a violent decrepitation, sometimes but rarely accompanied by a flash, takes place. A thick heavy vapour, of a greenish yellow colour, which rises with difficulty to the top of the vessel, if it be deep, is disengaged. The smell is not altogether unlike nitrous gas; but is peculiarly fetid, and may be compared to that which is emitted by brick-kilns, mixed with that of nitrous gas. It differs much from oxygenized muriatic acid gas; the latter being pungent and penetrating, the other heavy and oppressive; and it does not produce, at least in so great a degree, the catarrhal symptoms caused by the other. At the bottom of this vapour is a bright orange-coloured liquor, which has the same smell as the vapour. This is the acid contained in the salt; and I have proved it to be hyperoxygenized muriatic acid. But, although the salt from which the acid is disengaged be pure, the acid itself is never so; because the very act of disengaging it effects its decomposition, and some of it is converted into oxygenized muriatic acid. The colour of litmus paper, on this account, is generally destroyed by the liquor. I say on this account, because I have some reason to believe, from having observed this not to be uniformly the case, that hyperoxygenized muriatic acid reddens the vegetable blues. However, it must be considered, that the sulphuric acid used to disengage the hyperoxygenized muriatic acid is still present; and we can draw no certain conclusion, until we have obtained this acid free from all other substances. If to this mixture of hyperoxygenized muriate of potash and sulphuric acid, heat be applied, an exceedingly violent explosion, with a white and vivid flash, takes place, before the liquor has attained the temperature of 125 of FAHRENHEIT. In order to obtain this acid, I attempted to distil 500 grains, in a glass retort, in a water bath, with every precaution against such accidents as I could not but in some measure expect; when, almost as soon as I had kindled the fire, I saw, in the bottom of the retort, an extremely white, vivid, and rapid flash, which was immediately followed by a loud report. The retort was reduced almost to powder; so that scarcely any fragments of it could be found in the laboratory. The windows, and several glass vessels, were broken. I happened to be holding the neck of the retort, at the moment of the explosion, yet received no injury, except a slight contusion in the hand. But Dr. Vandier, a French gentleman of considerable chemical and medical talents, to whom I am indebted for much able assistance in my laboratory, was wounded in several places; particularly, the tunica conjunctiva of the eye was so lacerated, that a piece of it hung down, and, by getting under the inferior eyelid, caused the most painful irritation, and endangered his sight. One of the frontal arteries also was divided. I relate these circumstances thus fully, as the most effectual means of putting upon their guard, those who would repeat the experiment. If the sulphuric acid be dilute, heat may be applied with more safety; and the phenomena are different. The hyperoxygenized muriatic acid is disengaged from the basis; but, as the heat requisite to distil the acid is more than sufficient to decompose it, oxygenized muriatic acid comes over with it; and oxygen gas is collected in the pneumatic tub. If the distillation be continued, the same danger arises as in the former case, because the sulphuric acid becomes concentrated; and it would seem, that its action upon the salt is slight and partial at a low temperature, but violent and instantaneous when heated and concentrate. I could not, therefore hope, by these means, to obtain the acid disengaged and pure.

If the manner of bringing the sulphuric acid and the salt into contact be reversed, and the salt be dropped into the acid, the yellow vapours and the orange-coloured liquor are produced, but generally without decrepitation. If they be allowed to remain some days in contact, the vapours continue, and oxygen gas is constantly disengaged, even in the common light of the day, and at the temperature of the atmosphere. By cooling the first receiver with ice, I thought that I had once obtained this acid crystallized in the form of four-sided pyramids, of an orange colour. But, though I really believe this to have been the case, I do not positively affirm it.

Nitric acid produces nearly the same phenomena; but the smell and other properties are rather less distinct and marked, than with sulphuric acid.

Muriatic acid decomposes this salt, and unites to its basis; but neither the yellow vapours, nor the orange-coloured liquor, are produced. The circumstances which attend the contact of the acid and the salt, are as follows. If no more muriatic acid be present than is merely necessary to decompose the salt, I do not doubt that hyperoxygenized muriatic acid will be driven off, as little decomposed as with the other acids, supposing the action to be instantaneous; but, during the contact of these two bodies, the acid expelled must meet muriatic acid not yet combined, and, uniting with it, always forms a portion of oxygenized muriatic acid. The quantity of the last acid must vary, according to the quantity of muriatic acid employed, and not combined with the alkali. It was by this method that Mr. CRUICKSHANK obtained the muriatic acid gas, which he stated to contain 43,5 per cent. of oxygen.

Phosphoric and arsenic acids do not act upon this salt, till heated with it; and then much oxygen gas is evolved. These, therefore, afford no better method of disengaging hyperoxygenized muriatic acid without decomposition.

Oxalic, tartareous, and citric acids, act as I before mentioned; and the hyperoxygenized muriatic acid holds its place, in the order of affinities for potash, immediately before the benzoic.

I shall not stop to detail a number of amusing phenomena that may be produced, by projecting into the stronger acids, mixtures of combustible bodies, whether metallic or not, and hyperoxygenized muriate of potash. The cause of them is well understood, and the theory points them out: they are, therefore, no longer objects of philosophical admiration. But I must mention one experiment, which, had it succeeded, I should have thought important. I projected various mixtures of very minutely pulverised diamond and this salt, into the different acids; but found the diamond undiminished, by every attempt to combine it with oxygen in the humid way.*

Another, but imponderable, part of this salt, as indeed of all hyperoxygenized muriates, seems to be an extraordinary quantity of caloric. For, during their formation, scarcely any heat is disengaged, as by other acids; and, very little heat applied to the salts, gives the gaseous form to their oxygen.

An opinion has prevailed among some ingenious chemists, that, from a mixture of this salt with sulphuric acid, nitrous gas is disengaged, and sulphate of lime formed in the retort. But this is a mistake, arising, on the one hand, from the smell and vapour of the hyperoxygenized muriatic acid, and, on the other, from sulphate of lead, which the common sulphuric acid of this

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* I must confess, that the vivid flashes of light, emitted from the mixture of this salt and combustible bodies thrown into an acid, appear to me, in some measure, to prove the modification proposed by LEONHARDI, RICHTER, GREN, &c. to that part of the LAVOISIERIAN theory which regards the emission of light during combustion. Another testimony in favour of their modification, may be drawn from the vegetable kingdom. All plants growing in places deprived of light, are merely mucilaginous. But the mucilage of these plants burns without the emission of light. Light, therefore, appears not to be disengaged from oxygen; else, why not by this mucilage, as well as by other combustible bodies?

country frequently contains in solution, and which is precipitated from it by water. Before we assert a fact, we should be well assured of the pureness of our chemical agents. This supposed conversion of muriatic or hyperoxygenized muriatic acid into nitrous gas, will not pass for a decomposition, or a transmutation, of that refractory radical; and the idea of the change of potash into lime, is as erroneous as some other late assertions respecting the decomposition of the alkalis.

The proportions of this salt are, as I before stated,

Hyperoxygenized muriatic acid - -	58,3
Potash - - - - - -	39,2
Water - - - -	2,5
	100,0.

2d *Species.* *Hyperoxygenized Muriate of Soda.*

This salt is prepared in the same manner, and with the same phenomena, as the former. It is extremely difficult to obtain it pure, as it has nearly the same degree of solubility in water as muriate of soda. It is soluble in three parts of cold, and less of warm water; and is slightly deliquescent. It is soluble in alcohol; but this property alone is not sufficient to enable us to obtain it free from the muriate of soda, formed along with it in the entire liquor; as the latter salt, contrary to the assertions of all authors, is soluble in alcohol, and seems to be much more so, when accompanied by the hyperoxygenized muriate. It was by taking a large quantity of the entire salt, formed by sending a current of oxygenized muriatic acid gas through a solution of carbonate of soda, and repeatedly crystallizing in alcohol, that, with great difficulty, I obtained a little pure hyperoxygenized muriate of soda. It crystallizes in cubes, or in rhomboids little different from cubes. It produces a sensation of cold in the mouth; and its taste is easily distinguished from muriate of soda. It is decomposed by heat, by combustible bodies, and by acids, in the same manner as the former species; and the acid holds its place for soda, as for potash, immediately before the benzoic. The basis is separated by potash only. This salt is composed of,

Hyperoxygenized muriatic acid - -	66,2
Soda - - - -	29,6
Water - - - -	4,2
	100,0.

3d *Species. Hyperoxygenized Muriate of Barytes.*

The earthy bases seem to follow, in the order of affinities for this acid, at a great distance from the alkalis. They are all superseded by the two just mentioned; and it is much more difficult to accomplish their union with the acid, than is the case with potash or soda. The most advantageous method is, to pour warm water upon a large quantity of this earth, procured by Mr. VAUQUELIN's method; and to cause a current of oxygenized muriatic acid to pass through the liquor, kept warm; so that the barytes already dissolved being saturated, a fresh portion of it may be taken up by the water, and presented in a state of great division to the acid. This salt is soluble in about four parts of cold, and less of warm water. It crystallizes like the muriate of this earth; and resembles it so much in solubility, that I could not separate them effectually by crystallization repeated several times. At first, indeed, I despaired of ever obtaining any of the earthy hyperoxygenized muriates in a state sufficiently pure for analysis. If we consider them as a genus distinct from the alkaline hyperoxygenized muriates, a leading character may be, their great resemblance to their respective species of earthy muriates. I thought, however, that I might, if not by direct, at least by double affinity, decompose the one without the other; and phosphate of silver occurred to me as the most likely agent. If phosphate of silver be boiled with muriate of lime, of barytes, &c. a double decomposition ensues; and muriate of silver, together with phosphate of the earth, both insoluble, are precipitated. To increase the action, the phosphate of silver may be dissolved in a weak acid, such as the acetous; and, though the earthy phosphate be at first retained in solution, it will be separated by expelling the acid. The only condition absolutely necessary is, that the silver employed be free from copper. For, in preparing phosphate of silver by phosphate of soda, and by nitrate of silver thus impure, copper would be thrown down by the phosphoric acid; and the phosphate of copper would be afterwards decomposed by muriate of lime. Muriate of copper would therefore remain with the earthy hyperoxygenized muriates; or, what is still worse, a part of the muriatic acid being easily expelled from oxide of copper, the hyperoxygenized muriatic acid would be driven off from its basis, by the more powerful agency of the former. This salt has all the properties enumerated as belonging to the genus of hyperoxygenized muriates; and, with heat, the acid is expelled by all acids above the benzoic. I had hoped that, without distillation, I could procure the acid from the salt by means of sulphuric acid, which would have left an insoluble salt with barytes; but hyperoxygenized muriatic acid is so easily decomposed by light, that I have not yet obtained it, to my satisfaction, disengaged and pure. A fact well worthy of attention is, that the stronger acids disengage this acid with a flash of light, more frequently from the earthy than from the alkaline hyperoxygenized muriates; a phenomenon which, I suppose, depends upon the relative proportionate affinities, and consequently the greater rapidity of the disengagement. But, where all is hypothesis, it is useless to draw any inference from a single fact.

The proportions of this salt are,

Hyperoxygenized muriatic acid - -	47
Barytes - - - -	42,2
Water - - - -	10,8
	100,0.

4th Species. Hyperoxygenized Muriate of Strontia.

The foregoing observations apply to the formation of this salt, to the mode of obtaining it pure by phosphate of silver, to its conduct with the acids, to the rank of its acid in the order of affinities, and to its other properties. It is deliquescent; and more soluble in alcohol than muriate of strontia. It melts in the mouth immediately, and produces cold. Its crystals assume the shape of needles.

It is composed of,

Hyperoxygenized muriatic acid - -	46
Strontia - - - -	26
Water - - - -	28
	100.

5th Species. Hyperoxygenized Muriate of Lime.

This salt is obtained pure, in the same manner as the other earthy salts. It is extremely deliquescent; liquifies at a low heat, by means of its water of crystallization; and is very soluble in alcohol. It produces much cold, and a sharp bitter taste in the mouth.

It is composed of,

Hyperoxygenized muriatic acid - -	55,2
Lime - - - - -	28,3
Water - - - - -	16,5
	100,0.

6th Species. Hyperoxygenized Muriate of Ammonia.

From the property which oxygenized muriatic acid possesses of decomposing ammonia, this combination may be thought paradoxical. For, how can an acid much more active than oxygenized muriatic acid exist with ammonia, which is destroyed by the latter? But this argument may be opposed by the sum of affinities that act in either case. If the affinity of composition of oxygenized muriatic acid and of ammonia, together with the affinity of oxygenized muriatic acid for ammonia, to form oxygenized muriate of ammonia, be not more powerful than the affinity of oxygen for hydrogen, of azote for caloric, and of muriatic acid for ammonia, the divellent affinities will prevail; and this is what actually happens. But, although oxygen may be held with less force of attraction in oxygenized than in hyperoxygenized muriatic acid, yet the affinity of the latter acid for ammonia may increase in a much greater ratio, and favour the quiescent affinities. If carbonate of ammonia be poured into any earthy salt of this genus, a double decomposition takes place; and hyperoxygenized muriate of ammonia is formed. This salt is very soluble in water, and in alcohol. It is decomposed at a very low temperature, and gives out a quantity of gas, together with a smell of hyperoxygenized muriatic acid. Such a smell is doubtless owing to the great quantity of oxygen contained in the acid; it being more than is necessary to combine with the quantity of hydrogen contained in the alkali, and therefore some of the acid is disengaged, without decomposition. All the attempts I have made to ascertain the proportions of its principles, have been fruitless. The formation and existence of this salt, as I before said, are very strong proofs of what I have advanced respecting the state in which hyperoxygenized muriates at first exist; and very fully prove the different degree of affinity exercised by each acid toward the basis.

7th Species. Hyperoxygenized Muriate of Magnesia.

Its chemical and physical properties are nearly the same with those of the 5th species, only that, in addition to the other bases, lime and ammonia cause a precipitate in this salt.

Its proportions are,

Hyperoxygenized muriatic acid - -	60
Magnesia - - - -	25,7
Water - - - -	14,3
	100,0.

8th Species. Hyperoxygenized Muriate of Alumina.

I put some alumina, precipitated from muriate of alumina, and well washed, but still moist, into a WOULFE's apparatus, disposed as for the other earths, and sent a current of oxygenized muriatic acid gas through the liquor. The alumina shortly disappeared; and, upon pouring sulphuric acid into the liquor, a strong smell of hyperoxygenized muriatic acid was perceivable. When I attempted to obtain the salt pure, by phosphate of silver, in the usual way, I found nothing in solution but hyperoxygenized muriate of silver;* and all the hyperoxygenized muriate of alumina had been decomposed. This salt, however, appears to be very deliquescent, and is soluble in alcohol; but I could not ascertain the proportion of its principles, because I did not obtain it sufficiently free from the simple muriate.

9th Species. Hyperoxygenized Muriate of Silica.

I am inclined to think this salt does not really exist. A current of oxygenized muriatic acid, sent through some silica which had been precipitated from an acid by ammonia, and collected moist from the filter, did not seem to dissolve any portion of it. In all barytes and strontia, prepared according to Mr. VAUQUELIN's method, a portion of silica from the crucibles is attacked, and taken up, by whatever acid those earths may afterwards be dissolved in; and, in all potash of commerce, there is some silica; but I have never perceived that any portion of this earth had been dissolved by this acid.

The very small portion of earth which, in attempts to form the different species of this genus of salts, is taken up by acids, and the still smaller portion of the salt so formed, which is really in the state of hyperoxygenized muriate, render the operation so tedious, that I have confined myself to form what was necessary to determine their analysis, in such a manner as I believe to be nearly accurate. It cannot, therefore, be expected that I make myself responsible, without a right of appeal to further experiments, for the accuracy with which the crystalline forms, and other physical properties,

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* This salt shall be particularly mentioned and described in another part of this Paper. For the present, it is sufficient to say, that it is very soluble in water; and, in that property, as in many others, is totally different from muriate of silver.

may have been stated. It is impossible to obtain satisfactory crystals from a very small portion of salt; and I have attached myself more particularly to chemical than to physical characters, as being a much more important and certain mode of determination. For the same reason, I have not examined the combination of the new and rarer earths with this acid. But I do not doubt, that whatever chemist undertakes a further investigation of these extraordinary bodies, will be amply repaid for his labour.

I have mentioned, in a former part of this Paper, that all muriates lost a portion of their acid at a red heat. I exposed one hundred parts of muriate of potash, in a crucible, to a red heat, for some minutes, and found that they lost five. I dissolved them in water, and they manifested alkaline properties. Treated by nitrate of silver, they gave a precipitate, which shewed one per cent. less of muriatic acid, than 100 parts of the same salt that had not been exposed to fire. A violent heat may be necessary to expel the last portion of water of crystallization from certain salts, as we know particularly is the case with sulphate of lime. But, if any of the acid can be expelled at the same temperature, there is no longer any certainty. The quantity of water, as stated by different chemists, varies much; and, from some experiments I have made, I do not believe it to have been accurately determined. The method I used to ascertain this, was as follows: I exposed a given quantity of the salt to a violent heat, and noted its loss of weight. I then precipitated, by nitrate of silver; and thus knew, how much the quantity of muriatic acid which this salt contained, was less than that in a like portion which had not been exposed to heat. I subtracted the difference in this quantity, from the total loss of weight in the salt exposed to heat; and the remainder I considered as water. It was upon results obtained in this manner, that I founded many of the proportions I have given in this Paper.

It is stated in the tables of BERGMAN, corrected by Dr. PEARSON, that lime and strontia prefer acetous to arsenic acid. But arsenic acid can expel hyperoxygenized muriatic acid from its basis, although the acetous cannot act in the same manner; therefore, this order of affinities is erroneous. It was not till lately, that we had potash and soda so pure as to be relied upon in delicate experiments; and it is not surprising that we find mistakes with regard to their taking the acid from barytes, strontia, and lime. But real potash and soda both precipitate even barytes from hyperoxygenized muriatic acid. If ever it becomes easy to obtain hyperoxygenized muriate of barytes, we may prepare that earth from it in the humid way, and more near to purity, than in the method proposed by VAUQUELIN.

METALLIC COMBINATIONS OF MURIATIC ACID, IN ITS DIFFERENT STATES.

The action of hyperoxygenized muriatic acid upon metals, is, as may well be expected, rapid, and without disengagement of gas. It appears to dissolve every metal, not excepting gold and platina. If the metal be presented to the acid at the moment when it is disengaged from the salt, inflammation ensues; and the phenomena of light and heat vary according to the metal; but the salts thus produced are merely muriates. In order to form real hyperoxygenized muriates, it is necessary to take the metal in its fullest state of oxidizement, and combine it with the acid, either by double decomposition, or by passing a current of oxygenized muriatic acid gas through the oxide suspended in water. The acid is thus separated into muriatic and hyperoxygenized muriatic acid; and, in these states, combines with the metallic oxide. The metallic hyperoxygenized muriates are different, in every respect, from the metallic muriates. Red oxide of iron is dissolved with difficulty. Oxide of copper more easily. Red oxide of lead exhibits the same appearances, during its combination with this acid, as with nitric acid. When nitric acid is poured, even in excess, upon red oxide of lead, only a part of the oxide is dissolved, unless heat be applied; and what remains becomes a blackish brown powder. But, if metallic lead be added, in a just proportion, all the red oxide disappears, and none of the brown powder is formed; neither is there any disengagement of nitrous gas, when the metallic lead is dissolved. The precipitates caused in either case, by pouring an alkali into the nitric solution, are yellow. Hence it appears, that red oxide of lead contains too much oxygen to be dissolved by nitric acid. One part of the oxide takes up the excess of oxygen, and becomes brown; while the portion which loses oxygen, becomes yellow, and is soluble in nitric acid. The presence of metallic lead promotes the total solution of the red oxide, by taking up the superabundant oxygen. I found that a current of oxygenized muriatic acid gas, like the nitric acid, dissolved a part of the red oxide, and caused the brown powder to be formed, upon which it could not act. Hyperoxygenized muriate of lead is much more soluble than muriate of lead; and the acid is very slightly attracted by the basis.

But, of all the metallic salts formed by the combination of the muriatic acid, in any of its different states, none so much deserve attention as those which have for their bases, the oxides of mercury. The nature of the salts which result from the combination of common muriatic acid with the different oxides of this metal, has been stated in the most contradictory manner, by different chemists. But, as the knowledge of hyperoxygenized muriatic acid has thrown some light upon the true state of calomel and corrosive sublimate,* I must beg leave to dwell at some length upon this important part of my subject.

It would be useless to repeat the opinions of the old authors, who have treated of corrosive sublimate, and of calomel. They are to be found in the works of those respective chemists, and I must refer to them for particulars.

In the Memoirs of the Academy of Sciences of Paris, for 1780, we find a Paper of Mr. BERTHOLLET, upon the causticity of metallic salts; in which he appears to think, that the acid in corrosive sublimate is in the state of what was then called dephlogisticated marine acid. In 1785, when he had examined the oxygenized muriatic acid with more care, he renounced his former opinion; and gave the reasons why he no longer adhered to it. Some late experiments of Mr. PROUST shew, that this chemist thinks as Mr. BERTHOLLET now does. And these may be ranked among the first of modern authorities.

Notwithstanding those opinions, Mr. FOURCROY, in his *Système des Connoissances chimiques*, still considers corrosive sublimate as a hyperoxygenized muriate of mercury; and designs it

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* I regret very much, that I am under the necessity of using these unmeaning terms. But the French nomenclature has made no distinction between salts formed by metallic oxides in different states of oxidizement, except by the colour, which is an extremely defective and unmeaning method. At all events, this metal is so uncomplaisant as to retain the white colour, in its different oxides combined with muriatic acid. I prefer, however, using the old name, to proposing any provisional substitute that might be found defective. This will be farther explained in *Remarks upon chemical Nomenclature.*

throughout by that name.* This chemist, one of the founders of the methodical Nomenclature, is too well acquainted with its principles, to apply the term hyperoxygenized muriate to any thing but a combination of hyperoxygenized muriatic acid. It is evident, therefore, that he considers the portion of oxygen, which, in equal quantities of corrosive sublimate and calomel, is greater in the former, to be combined with the acid, and not with the oxide of mercury. As soon as I have stated some experiments that prove Mr. FOURCROY's opinion to be erroneous, and endeavoured to establish the analysis of corrosive sublimate and of calomel, I shall take notice of a salt hitherto unknown, which really is hyperoxygenized muriate of mercury.

I took a portion of corrosive sublimate, and precipitated by potash. The liquor was filtered; and, upon being tried, nothing but muriate of potash was found. No reagent could discover the smallest trace of hyperoxygenized muriatic acid.

Sulphuric, nitric, phosphoric, and many other acids, poured upon corrosive sublimate, did not disengage either muriatic, or hyperoxygenized muriatic acid. Nitrate of silver, poured into a solution of corrosive sublimate, gave an abundant white precipitate.

From these experiments it is evident, that muriatic acid, not hyperoxygenized muriatic acid, is combined with the oxide of mercury in corrosive sublimate.

To determine the proportions of this salt, I took one hundred parts, and precipitated by nitrate of silver. I then took another hundred, and precipitated by potash. The result of these two

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* I have said before, that this acid was talked of by many chemists, as if the existence of it had really been proved.

experiments was such as to establish the proportions of corrosive sublimate as follows:

Oxide of mercury - - -	82
Muriatic acid - - - -	18
	100.

But, the acid of this salt not being charged with a superabundance of oxygen, we must look for the excess in the metallic oxide. I took 100 grains of mercury, and dissolved them in nitric acid; then poured in muriatic acid; and, at a very gentle heat, evaporated to dryness. I afterwards sublimed, in a Florence flask, the salt that remained, and obtained 143,5 of corrosive sublimate. But, 143,5 of corrosive sublimate, contain 26 of acid; which will leave 117,5 for the mercurial oxide; and, if 117,5 contain 100 of mercury, 100 of the oxide will contain 85. Therefore, the oxide of mercury, in corrosive sublimate, is oxidized at the rate of 15 per cent.

To determine the proportions in calomel, I dissolved 100 grains of it in nitric acid. The phenomena of the solution have been so accurately described by Mr. BERTHOLLET, that I shall not repeat them. I precipitated by nitrate of silver; and obtained a quantity of muriate of silver, corresponding with 11,5 of muriatic acid. The oxide of mercury I obtained apart. Therefore, calomel is composed of,

Oxide of mercury - - -	88,5
Muriatic acid - - - -	11,5
	100,0.

To ascertain the state of oxidizement of the oxide in calomel, I took 100 grains, and boiled them with nitro-muriatic acid;

then evaporated very slowly, and sublimed as above. The calomel was totally converted into corrosive sublimate, and weighed 113. But 113 of corrosive sublimate contain 20,3 of muriatic acid, of which, 11,5 were originally in the calomel. The total addition of weight was 13. But the quantity of acid in these 13, amounts to 20,3 — 11,5 = 8,8. Therefore, 13 — 8,8 = 4,2, remain for that part of the additional weight which is oxygen. On the other hand, 100 of calomel contain the same quantity of mercury as 113 of corrosive sublimate, = 79. These 79, with 11,5 of acid, are equal to 90,5, and leave 9,5 for the quantity of oxygen contained in calomel. It would appear, from these experiments, that corrosive sublimate contains 6,5 per cent. more acid, and but 2,8 per cent. more oxygen, than calomel. But this quantity of oxygen is combined with a much greater proportion of mercury; and forms an oxide of a very different degree of oxidizement. For, 88,5 : 9,5 : : 100 : 10,7. Therefore, we may establish the following comparative table.

CALOMEL.		CORROSIVE SUBLIMATE.	
The oxide of mercury in calomel is composed of,		The oxide of mercury in corrosive sublimate is composed of,	
Mercury - -	89,3	Mercury - - -	85
Oxygen - -	10,7	Oxygen - -	15
	100,0.		100.
And calomel is composed of,		And corrosive sublimate is composed of,	
Mercury 79 Oxygen 9,5 } oxide of mercury	88,5	Mercury 69,7 Oxygen 12,3 } oxide of mercury	82
Muriatic acid -	11,5	Muriatic acid -	18
	100,0.		100.

These proportions are different from those given by LEMERY, GEOFFROY, BERGMAN, &c. But, without calling in question the accuracy and skill of these chemists, it is fair to assert, that the pure materials used by modern chemists, are more likely to lead to sure results, than the impure reagents of the ancients.

In these salts we find another instance, that, in proportion as metallic oxides contain a greater quantity of oxygen, they require a greater quantity of acid to enter into combination with them.

The method I have followed, to ascertain the proportions just stated, may appear, at first view, not to be the shortest that I might have adopted. But I have tried others, and have found none so accurate. It is impossible, synthetically, to convert a given quantity of mercury into calomel, in such a manner as to be certain that none of it is in a different state from that required. And, if we would attack calomel analytically, the action of the alkalis, without which we cannot proceed, is such as to alter the nature of the oxides. I have also made many comparative experiments, by dissolving calomel in nitro-muriatic acid, (which converted it into corrosive sublimate,) and then precipitating by ammonia; but I have not found these trials so successful as those I have described. The nature of the precipitate from corrosive sublimate by ammonia, certainly differs, according to the excess of acid that may be present; and mercury seems to have the power of existing in many degrees of combination with oxygen. The only precaution absolutely necessary, in this mode of operating, is, that while the mercurial salt is in an open vessel, it should not be exposed to a degree of heat capable of volatilizing any part of it.

The quantity of mercury ordered in the London Pharmacopoeia, to convert corrosive sublimate into calomel, is 9 pounds of mercury for every 12 pounds of corrosive sublimate. But, from the above experiments, it would appear, that a smaller quantity of mercury might strictly answer. However, from the results of minute investigation, we should not conclude too hastily upon preparations on the great scale; and, I rather think, that the excess of mercury ordered by the Pharmacopoeia is a useful precaution.

In my experiments, I attempted to reduce, by means of copper, iron, or zinc, the mercury contained in the mercurial salts. Iron did not answer the purpose: zinc precipitated the mercury a little better; and copper produced a change which I did not expect. If a bit of copper be put into a solution of corrosive sublimate, a white powder shortly falls to the bottom; and that powder is calomel. When washed, it does not contain an atom of copper, nor of corrosive sublimate.

Before I conclude these considerations, I must say, that whether calomel be prepared in the dry or in the humid way,* it does not seem to differ chemically; nor does it contain any

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* By the humid way, I do not mean precisely the method of Scheele. That chemist desires us to boil the acid with the mercury, after they have ceased to act upon each other at a low temperature. By this method, the nitric acid takes up an excess of mercurial oxide; and the nitrate of mercury thus formed, precipitates by water. Therefore, when this nitrate of mercury is poured into the dilute solution of muriate of soda, according to the formula of Scheele, the action, on the part of the solution, is twofold.

1st. The water acts upon one part, and precipitates an oxide, or rather an insoluble subnitrate of mercury. And,

2dly. A double decomposition takes place between the nitrate of mercury and the muriate of soda. It is with reason, that the medical world have supposed the calomel of Scheele to be different from that prepared in the humid way; for it is, in fact, calomel, plus an insoluble subnitrate of mercury. In the first part of Scheele's sensible portion of water of crystallization. The same may be said of corrosive sublimate.

It now remains to speak of the real hyperoxygenized muriate of mercury. I passed a current of oxygenized muriatic acid gas through some water, in which there was red oxide of mercury.* After a short time, the oxide became of a very dark brown colour; and a solution appeared to have taken place. The current was continued for some time; and, when I thought that a sufficient quantity of the oxide had been dissolved, I stopped the operation. The liquor was evaporated to dryness; and the salt was thus obtained. There evidently was in the mass a great proportion of corrosive sublimate, as might be expected, from what I had observed to take place in the formation of the other salts of this acid; but, by carefully separating

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process, there is disengagement of nitrous gas, together with oxidizement and solution of some of the mercury. When he boils the acid upon the remaining mercury, there is no further disengagement of gas; yet more mercury is dissolved. The nitrate of mercury, therefore, rather contains an oxide less oxidized after ebullition than before it. The true difference is in the subnitrate of mercury, precipitated, as I before said, by the water in which the muriate of soda was dissolved. And the orange coloured powder, which remains after an attempt to sublime SCHEELE's calomel, is to be attributed to the same cause. To prepare calomel in the humid way, uniform as to itself, and in all respects similar to that prepared in the dry way, it is necessary, either to use the nitric solution before it has boiled, or to pour some muriatic acid into the solution of muriate of soda, previously to mixing it with the boiled solution of nitrate of mercury. In the first case, no precaution is necessary; and, in the latter, the oxide of mercury, which the nitrate of mercury has, by boiling, taken up in excess, finds an acid which is ready to saturate it. All the mercurial oxide being thus converted into calomel, none of that subnitrate of mercury can be present.

The objections made by a medical gentleman against SCHEELE's calomel, when this Paper was read before the Royal Society, led me to reconsider the subject, and to undertake the investigation detailed in this note.

* I used either of the red oxides of mercury, indiscriminately.

the last formed crystals, I could pick out some hyperoxygenized muriate of mercury. I then crystallized it over again; and, in this manner, I obtained it nearly pure. This salt is more soluble than corrosive sublimate: about four parts of water retain it in solution. The shape of its crystals, I cannot well determine. When sulphuric, or even weaker acids, are poured upon it, it gives out the usual smell of hyperoxygenized muriatic acid; and the liquor becomes of an orange colour. This is a sufficient proof, that corrosive sublimate is not a hyperoxygenized muriate of mercury.

I have just mentioned that, in the formation of this salt, the oxide of mercury, which was not dissolved by the acid, became of a very dark brown colour. I procured a portion of this oxide, which seemed different from the red oxide. It however retained the form, and the crystalline appearance, of the latter. It was soluble in nitric acid, without disengagement of gas; and was precipitated from it, in a yellow oxide, by all the alkalis, except ammonia. It formed corrosive sublimate with muriatic acid; and the precipitate by the alkalis, was the same as that from corrosive sublimate, made with the red oxide. Yet I am inclined to think, that the dark brown oxide differs in some essential point from the red; but I have not yet made sufficient experiments to prove this opinion. At all events, the present object being to examine the mercurial oxides only as combined with muriatic acid, it would be foreign to the purpose, to enter upon too minute an investigation of the other states of the metal. This, and some other objects hinted at in this Paper, must be reserved for future inquiry.

In treating the earthy hyperoxygenized muriates with phosphate of silver, as I mentioned before, I observed that the liquor sometimes contained in solution oxide of silver; which, upon examination, I found to be combined with hyperoxygenized muriatic acid. As the salt which is thus formed is different, in every respect, from simple muriate of silver, it may be of some importance to consider it with attention. In the first place, it will afford the most convincing proof of the difference between muriatic and hyperoxygenized muriatic acid; and, in the next place, it particularly deserves to be remarked, for possessing, in the most eminent degree, one of the great characteristic features of the genus to which it belongs. Hyperoxygenized muriate of silver is soluble in about two parts of warm water; but, by cooling, it crystallizes in the shape of small rhomboids, opaque and dull, like nitrate of lead or of barytes. It is somewhat soluble in alcohol. Muriatic acid decomposes it; as does nitric, and even acetous acid: but the result of this decomposition is not, as might be expected, nitrate or acetite of silver. At the moment that the acid is expelled from hyperoxygenized muriate of silver, a reaction takes place among its elements: oxygen is disengaged; and the muriatic acid remains in combination with the oxide of silver. If this fact be compared with the manner in which nitric and acetous acids act upon hyperoxygenized muriate of potash, it will give a strong proof of the proportionate affinities of all these acids for oxide of silver, in comparison with that which they exercise towards the alkali.

Hyperoxygenized muriate of silver, when exposed to a very moderate heat, begins by melting, and then gives out a considerable quantity of oxygen gas, with effervescence; and muriate of silver remains behind. These phenomena however differ much, according to the degree of heat applied. When hyperoxygenized muriate of silver is mixed with about half its weight of sulphur, it detonates in the most violent manner; and does not, like hyperoxygenized muriate of potash, require the addition of charcoal, to possess a very great force of explosion. The slightest pressure is sufficient to cause this mixture to detonate; and I think I shall be within bounds, when I state, that half a grain of hyperoxygenized muriate of silver, with a quarter of a grain of sulphur, explodes with a violence at least equal to five grains of hyperoxygenized muriate of potash, with the due quantities of sulphur and charcoal. The flash is white and vivid, and is accompanied by a sharp and quick noise, like the fulminating silver so ably described by Mr. HOWARD; and the silver is reduced to the metallic state, and vaporized.

I think it right to add a few remarks, upon what I have termed the proportionate affinities of acids and of bases, one for the other. It is a law, not indeed universally, but frequently observed, and very well worthy of consideration, that the acids are attracted by metallic oxides, in a very different order from that in which they are disposed to unite to alkaline and earthy bases.

Nitric acid, which holds so high a place in the order of affinities for alkalis, is expelled from metallic oxides by most acids. Phosphoric, fluoric, all the vegetable acids, except two or three, and the animal acids, attract the latter bases more strongly. Nay, we shall find, upon an attentive examination, that acids commonly attract metallic oxides, in the inverse ratio of their action upon metals, or, in other words, in proportion to their own affinity of composition. Thus, the phosphoric and fluoric acids sometimes rank before the sulphuric; and the nitric, as I before said, is generally very low. Hyperoxygenized muriatic acid seems to follow the same rule; and takes its place, in the order of affinities for metallic oxides, after many of those acids which it can expel from earths and alkalis.

The other hyperoxygenized muriates, I have not yet sufficiently examined. I shall, however, mention at present, that I have ascertained the muriatic salts, formerly known by the strange name of *butters of the metals*, to be muriates, and not hyperoxygenized muriates; and the extraordinary proportion of oxygen, to be combined, not in the acid, but in the metallic oxide.

In the course of different experiments, I have known hyperoxygenized muriatic acid to be formed in two cases, where I could not have expected it.

In the analysis of some menachanite from Botany Bay, given to me last year by the President of the Royal Society, I observed, that while the oxide of titanium was precipitated from the muriatic acid in which it was dissolved, the excess of oxygen in the oxide passed over to the muriatic acid and the potash, already in the liquor, and that hyperoxygenized muriate of potash was formed. I have attempted the same experiment with black oxide of manganese, but could not succeed.

There is, however, a still more extraordinary formation of this acid, in the distillation of nitro-muriatic acid upon platina. Oxygen is absorbed by the metal; yet, not only oxygenized, but also hyperoxygenized muriatic acid is formed. I have repeated the experiment several times; and am well convinced of the fact, however contrary to theory it may appear. I have tried the action of oxygenized muriatic acid upon nitric acid, in the hopes of forming hyperoxygenized muriatic acid; but there was no action to this effect among their elements.

The fact of the production of a peculiar gas, by the distillation of nitro-muriatic acid upon platina, has been observed by Mr. Davy, in his *Researches.*[*] But, as hyperoxygenized muriatic acid was not known at that time, he could not say the real nature of that gas. Had Mr. Davy carried his ingenious experiments a little farther, we should have been much earlier acquainted with the last degree of oxygenizement of muriatic acid.

Mr. Berthollet terminates his Paper upon hyperoxygenized muriate of potash, by saying, that he will consider muriatic acid as the radical; oxygenized muriatic acid, as corresponding with sulphureous and nitrous acid; and the acid which he conjectured to exist in this salt, as corresponding with sulphuric and nitric acid. I shall now conclude, by stating the arguments in favour of each denomination, and the analogies upon which they are founded.

Muriatic acid is for us a simple body; but it has acid properties of the strongest kind; therefore, from analogy, we suppose it to contain oxygen. But may not this be too hasty a conclusion? Are we not very doubtful concerning the existence of oxygen in prussic acid? And are we not, on the contrary, certain that sulphurated hydrogen, which possesses many of the characteristics of acids, does not contain any? Of the oxygenizement of fluoric and boracic acids, we have no proof: but then we cannot affirm that any one of these acids exists in three states of combination with oxygen; and the muriatic is the only radical of which we admit this fact. We must not, however, pretend to limit the number or degrees of combinations between combustible bodies and oxygen; but we can

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[*] Dr. Priestley, also, mentions a peculiar gas, produced by distilling a solution of gold in *aqua regia.*

speak, with certainty, only of those things which are proved. Besides its acid properties, this substance has others, common to oxygenizable bodies. With 16 of oxygen, it forms an acid, which, in many of its properties, is to its radical what the sulphureous is to sulphur. Like the sulphureous, it is volatile; has little attraction for salifiable bases; destroys vegetable blues; and is capable of further oxygenizement. With 65 of oxygen, it becomes more fixed, like sulphuric acid; has a stronger affinity for salifiable bases; and acquires more truly acid properties. Upon these considerations, I submit to the chemical world, whether, in the present state of our knowledge, it be not more philosophical to say,

Muriatic radical, or some single word of the same import,	instead of	Muriatic acid;
Muriatous acid,		Oxygenized muriatic acid;
Muriatic acid,		Hyperoxygenised muriatic acid.

I am fully aware that, at first sight, this may appear extraordinary; and the more so, as we have no positive facts that prove muriatic acid to be a simple body. All we can, therefore, consider fairly, is, in favour of which appellation does the sum of analogies seem to preponderate. And, to give the cause a candid investigation, we should begin by considering, whether the presence of oxygen in all bodies that have acid properties, has been rigidly demonstrated; and not determine by this law of the French chemistry, till we are well convinced it has not been too generally assumed.

If a nomenclature be not subservient to the uses of science, and does not keep pace with its progress, the relation between substances and their names will become so relaxed, that confusion will be brought about, by the very means we take to avoid it; and if, while we continue to extend our acquaintance with chemical bodies, nomenclature remains confined within its former limits, the bonds that unite these two parts of the science must inevitably be broken.

VII. *Experiments and Observations on certain stony and metalline Substances, which at different Times are said to have fallen on the Earth; also on various Kinds of native Iron. By* Edward Howard, *Esq. F. R. S.*

Read February 25, 1802.

The concordance of a variety of facts seems to render it most indisputable, that certain stony and metalline substances have, at different periods, fallen on the earth. Whence their origin, or whence they came, is yet, in my judgment, involved in complete obscurity.

The accounts of these peculiar substances, in the early annals, even of the Royal Society, have unfortunately been blended with relations which we now consider as fabulous; and the more ancient histories of stones fallen from heaven, from Jupiter, or from the clouds, have evidently confounded such substances with what have been termed *Ceraunia*, *Bœtilia*, *Ombria*, *Brontia*, &c. names altogether unappropriate to substances fallen on our globe. Indeed some mislead, and others are inexpressive.

The term Ceraunia, by a misnomer, deduced from its supposed origin, seems, as well as Bœtilia,[*] to have been anciently used to denote many species of stones, which were polished and shaped into various forms, though mostly wedge-like or triangular, sometimes as instruments, sometimes as oracles, and sometimes as deities. The import of the names, Ombria, Brontia, &c. seems subject to the same uncertainty.

In very early ages, it was believed, that stones did in reality

[*] Mercati, Metallotheca Vaticana, page 241.

fall, as it was said, from heaven, or from the gods; these, either from ignorance, or perhaps from superstitious views, were confounded with other stones, which, by their compact aggregation, were better calculated to be shaped into different instruments, and to which it was convenient to attach a species of mysterious veneration. In modern days, because explosion and report have generally accompanied the descent of such substances, the name of thunderbolt, or thunderstone, has ignorantly attached itself to them; and, because a variety of substances accidentally present, near buildings and trees struck with lightning, have, with the same ignorance, been collected as thunderbolts, the thunderbolt and the fallen metalline substance have been ranked in the same class of absurdity. Certainly, since the phenomena of lightning and electricity have been so well identified, the idea of a thunderbolt is ridiculous. But the existence of peculiar substances fallen on the earth, I cannot hesitate to assert; and, on the concordance of remote and authenticated facts, I shall rest the assertion.

Mr. KING, the learned author of *Remarks concerning Stones said to have fallen from the Clouds, in these Days, and in ancient Times*, has adduced quotations of the greatest antiquity, descriptive of the descent of fallen stones; and, could it be thought necessary to add antique testimonies to those instanced by so profound an antiquarian, the quotations of Mons. FALCONET, in his papers upon Bœtilia, inserted in the *Histoire des Inscriptions et Belles-Lettres*;* the quotations in ZAHN's *Specula Physico-mathematica Historiana*;† the *Fisica Sotterranea* of GIACINTO GEMMA; the works of PLINY, and others, might be referred to.

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* Tom. VI. P. 519. et Tom. XXIII. P. 228.

† Fol. 1696. Vol. I. p. 385. where a long enumeration of stones fallen from the sky is given.

Dr. CHLADNI, in his *Observations on the Mass of Iron found in Siberia, and on other Masses of the like Kind*, as well as in his *Observations on Fire-balls and hard Bodies fallen from the Atmosphere*, has collected almost every modern instance of phenomena of this nature.

Mr. SOUTHEY relates an account, juridically authenticated, of a stone weighing 10lbs. which was heard to fall in Portugal, Feb. 19, 1796, and was taken, still warm, from the ground.*

The first of these peculiar substances with which chemistry has interfered, was the stone presented by the Abbé BACHELAY to the Royal French Academy. It was found on the 13th of September, 1768, yet hot, by persons who saw it fall. It is described as follows:

" La substance de cette pierre est d'un gris cendré pâle; " lorsqu'on en regarde le grain à la loupe, on apperçoit que " cette pierre est parsemée d'une infinité de petits points brillans métalliques, d'un jaune pâle; sa surface extérieure, celle " qui, suivant M. l'Abbé BACHELAY, n'étoit point engagée dans " la terre, étoit couverte d'une petite couche très-mince d'une " matière noire, boursoufflée dans des endroits, et qui paroissoit avoir été fondue. Cette pierre, frappée dans l'intérieur " avec l'acier, ne donnoit aucune étincelle; si on frappoit, au " contraire, sur la petite couche extérieure, qui paroissoit avoir " été attaquée par le feu, on parvenoit à en tirer quelques-unes." The specific gravity of this stone was as 3535 to 1000.

The academicians analyzed the stone, and found it to contain,

Sulphur - - -	8½
Iron - - -	36
Vitrifiable earth - - -	55½
	100.

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* Letters written during a short residence in Spain and Portugal. Page 239.

Of their mode of analysis, I shall have occasion to speak hereafter. They were induced to conclude, that the stone, presented to the Academy by the Abbé BACHELAY, did not owe its origin to thunder; that it did not fall from heaven; that it was not formed by mineral substances, fused by lightning; and that it was nothing but a species of pyrites, without peculiarity, except as to the hepatic smell disengaged from it by marine acid. "Que cette pierre, qui peut-être étoit couverte d'une "petite couche de terre ou de gazon, aura été frappée par la "foudre, et qu'elle aura été ainsi mise en évidence: la chaleur "aura été assez grande pour fondre la superficie de la partie "frappée, mais elle n'aura pas été assez long-tems continuée "pour pouvoir pénétrer dans l'intérieur; c'est ce qui fait que "la pierre n'a point été décomposée. La quantité de matières "métalliques qu'elle contenoit, en opposant moins de résistance "qu'un autre corps au courant de matière électrique, aura peut-"être pu contribuer même à déterminer la direction de la "foudre."

The Memoir is however concluded, by observing it to be sufficiently singular, that M. MORAND le Fils had presented a fragment of a stone, from the environs of Coutances, also said to have fallen from heaven, which only differed from that of the Abbé BACHELAY, because it did not exhale the hepatic smell with spirit of salt. Yet the academicians did not think any conclusion could be drawn from this resemblance, unless that the lightning had fallen by preference on pyritical matter.*

Mons. BARTHOLD, Professeur à l'Ecole centrale du Haut-Rhin, gave I believe the next, and last,† analytical account of

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* See *Journal de Physique*. Tom. II. page 251.

† A very interesting detail of a meteor, and of stones fallen in July, 1790, was given by Professeur BAUDIN, in the *Magazin für das Neueste aus der Physik*, by Professor VOIGT.

what he also denominates *Pierre de Tonnerre.* He describes it thus: "La masse de pierre connue sous le nom de Pierre de "Tonnerre d'Ensisheim, pesant environ deux quintaux, a la "forme extérieure arrondie, presque ovale, raboteuse, d'un "aspect terne et terreux.

"Le fond de la pierre est d'une couleur grise bleuâtre, par-"semée de cristaux de pyrites, isolés, d'une cristalisation "confuse, en quelques endroits écailleuses, ramassés, formant "des nœuds et des petites veines, qui le parcourent en tout "sens: la couleur des pyrites est dorée; le poli leur donne un "éclat d'acier, et, exposées à l'atmosphère, elles ternissent et "brunissent. On distingue de plus, à l'œil nud, de la mine "de fer grise, écailleuse, non sulfureuse, attirable à l'aimant, "dissoluble dans les acides, peu oxidé, ou s'approchant beau-"coup de l'état métallique.

"La cassure est irrégulière, grenue, d'un grain un peu "serré: dans l'intérieur on voit de très petites fentes. Elle ne "fait pas feu au briquet; sa contexture est si lâche qu'elle se "laisse entamer au couteau. En la pilant, elle se réduit assez "facilement en une poudre grise bleuâtre, d'une odeur terreuse. "Quelquefois il se trouve des petits cristaux de mine de fer, "qui résistent plus aux coups du pilon."

The specific gravity of the piece in Professor BARTHOLD's possession, was 3233, distilled water being taken at 1000.

The analysis of Mons. BARTHOLD, of which I shall also have occasion to speak hereafter, gave in the 100,

Sulphur	2
Iron	20
Magnesia	14
Alumina	17
Lime	2
Silica	42
	97.

From the external characters, and from his analysis, the Professor considers the stone of Ensisheim to be argillo-ferruginous; and is of opinion that ignorance and superstition have attributed to it a miraculous existence, at variance with the first notions of natural philosophy.*

The account next in succession is already printed in the Transactions of the Royal Society; but cannot be omitted, as it immediately relates to one of the substances I have examined. I allude to the letter received by Sir WILLIAM HAMILTON, from the Earl of BRISTOL, 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 persons, men, women, and children. The stones are "of a quality not found in any part of the Siennese territory; "they fell about eighteen hours after the enormous eruption of "Mount Vesuvius; which circumstance leaves a choice of difficulties in the solution of this extraordinary phenomenon. "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 philosophers 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." (Phil. Trans. for 1795. p. 103.) Sir WILLIAM HAMILTON, it seems, also received a piece of one of the largest stones, which weighed upwards of five pounds; and had seen another, which weighed about one. He likewise observed, that the outside of every stone which had been found, and had been ascertained to have fallen

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* See *Journal de Physique. Ventose, An* 8. p. 169.

from the clouds near Sienna, was evidently freshly vitrified, and was black, having every sign of having passed through an extreme heat; the inside was of a light gray colour, mixed with black spots and some shining particles, which the learned there had decided to be pyrites.

In 1796, a stone weighing 56lbs. was exhibited in London, with several attestations of persons who, on the 13th of December, 1795, saw it fall, near Wold Cottage, in Yorkshire, at about three o'clock in the afternoon. It had penetrated through 12 inches of soil and 6 inches of solid chalk rock; and, in burying itself, had thrown up an immense quantity of earth, to a great distance: as it fell, a number of explosions were heard, about as loud as pistols. In the adjacent villages, the sounds heard were taken for guns at sea; but, at two adjoining villages, were so distinct of something singular passing through the air, towards the habitation of Mr. TOPHAM, that five or six people came up, to see if any thing extraordinary had happened to his house or grounds. When the stone was extracted, it was warm, smoked, and smelt very strongly of sulphur. Its course, as far as could be collected from different accounts, was from the south-west. The day was mild and hazy, a sort of weather very frequent in the Wold hills, when there are no winds or storms; but there was not any thunder or lightning the whole day. No such stone is known in the country. There was no eruption in the earth; and, from its form, it could not come from any building; and, as the day was not tempestuous, it did not seem probable that it could have been forced from any rocks, the nearest of which are those of Hamborough Head, at a distance of twelve miles.* The nearest volcano, I believe to be Hecla, in Iceland.

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* Extracted from the printed paper delivered at the place of exhibition.

The exhibition of this stone, as a sort of show, did not tend to accredit the account of its descent, delivered in a hand-bill at the place of exhibition; much less could it contribute to remove the objections made to the fall of the stones presented to the Royal French Academy. But the Right Hon. President of the Royal Society, ever alive to the interest and promotion of science, observing the stone so exhibited to resemble a stone sent to him as one of those fallen at Sienna, could not be misled by prejudice: he obtained a piece of this extraordinary mass, and collected many references to descriptions of similar phenomena. At length, in 1799, an account of stones fallen in the East Indies was sent to the President, by JOHN LLOYD WILLIAMS, Esq. which, by its unquestionable authenticity, and by the striking resemblance it bears to other accounts of fallen stones, must remove all prejudice. Mr. WILLIAMS has since drawn up the following more detailed narrative of facts.

Account of the Explosion of a Meteor, near Benares, *in the* East Indies; *and of the falling of some Stones at the same Time, about 14 Miles from that City. By* John Lloyd Williams, *Esq. F. R. S.*

A circumstance of so extraordinary a nature as the fall of stones from the heavens, could not fail to excite the wonder, and attract the attention, of every inquisitive mind.

Among a superstitious people, any preternatural appearance is viewed with silent awe and reverence; attributing the causes to the will of the Supreme Being, they do not presume to judge the means by which they were produced, nor the purposes for which they were ordered; and we are naturally led to suspect the influence of prejudice and superstition, in their descriptions of such phenomena; my inquiries were therefore chiefly directed to the Europeans, who were but thinly dispersed about that part of the country.

The information I obtained was, that on the 19th of December, 1798, about eight o'clock in the evening, a very luminous meteor was observed in the heavens, by the inhabitants of Benares and the parts adjacent, in the form of a large ball of fire; that it was accompanied by a loud noise, resembling thunder; and that a number of stones were said to have fallen from it, near Krakhut, a village on the north side of the river Goomty, about 14 miles from the city of Benares.

The meteor appeared in the western part of the hemisphere, and was but a short time visible: it was observed by several Europeans, as well as natives, in different parts of the country.

In the neighbourhood of Juanpoor, about 12 miles from the spot where the stones are said to have fallen, it was very distinctly observed by several European gentlemen and ladies; who described it as a large ball of fire, accompanied with a loud rumbling noise, not unlike an ill discharged platoon of musquetry. It was also seen, and the noise heard, by various persons at Benares. Mr. DAVIS observed the light come into the room where he was, through a glass window, so strongly as to project shadows, from the bars between the panes, on a dark coloured carpet, very distinctly; and it appeared to him as luminous as the brightest moonlight.

When an account of the fall of the stones reached Benares, Mr. DAVIS, the judge and magistrate of the district, sent an intelligent person to make inquiry on the spot. When the person arrived at the village near which the stones were said to have fallen, the natives, in answer to his inquiries, told him, that they had either broken to pieces, or given away to the Tesseldar (native collector) and others, all that they had picked up; but that he might easily find some in the adjacent fields, where they would be readily discovered, (the crops being then not above two or three inches above the ground,) by observing where the earth appeared recently turned up. Following these directions, he found four, which he brought to Mr. DAVIS: most of these, the force of the fall had buried, according to a measure he produced, about six inches deep, in fields which seemed to have been recently watered; and it appeared, from the man's description, that they must have lain at the distance of about a hundred yards from each other.

What he further learnt from the inhabitants of the village, concerning the phenomenon, was, that about eight o'clock in the evening, when retired to their habitations, they observed a very bright light, proceeding as from the sky, accompanied with a loud clap of thunder, which was immediately followed by the noise of heavy bodies falling in the vicinity. Uncertain whether some of their deities might not have been concerned in this occurrence, they did not venture out to inquire into it until the next morning; when the first circumstance which attracted their attention was, the appearance of the earth being turned up in different parts of their fields, as before mentioned, where, on examining, they found the stones.

The assistant to the collector of the district, Mr. ERSKINE, a very intelligent young gentleman, on seeing one of the stones, brought to him by the native superintendant of the collections, was also induced to send a person to that part of the country, to make inquiry; who returned with several of the stones, and brought an account similar to that given by the person sent by Mr. DAVIS, together with a confirmation of it from the Cauzy, (who had been directed to make the inquiry,) under his hand and seal.

Mr. MACLANE, a gentleman who resided very near the village of Krakhut, gave me part of a stone that had been brought to him the morning after the appearance of the phenomenon, by the watchman who was on duty at his house; this, he said, had fallen through the top of his hut, which was close by, and buried itself several inches in the floor, which was of consolidated earth. The stone must, by his account, previous to its having been broken, have weighed upwards of two pounds.

At the time the meteor appeared, the sky was perfectly serene; not the smallest vestige of a cloud had been seen since the 11th of the month, nor were any observed for many days after.

Of these stones, I have seen eight, nearly perfect, besides parts of several others, which had been broken by the possessors, to distribute among their friends. The form of the more perfect ones, appeared to be that of an irregular cube, rounded off at the edges; but the angles were to be observed on most of them. They were of various sizes, from about three to upwards of four inches in their largest diameter; one of them, measuring four inches and a quarter, weighed two pounds twelve ounces. In appearance, they were exactly similar: externally, they were covered with a hard black coat or incrustation, which in some parts had the appearance of varnish, or bitumen; and, on most of them were fractures, which, from their being covered with a matter similar to that of the coat, seemed to have been made in the fall, by the stones striking against each other, and to have passed through some medium, probably an intense heat, previous to their reaching the earth. Internally, they consisted of a number of small spherical bodies, of a slate colour, embedded in a whitish gritty substance, interspersed with bright shining spiculæ, of a metallic or pyritical nature. The spherical bodies were much harder than the rest of the stone: the white gritty part readily crumbled, on being rubbed with a hard body; and, on being broken, a quantity of it attached itself to the magnet, but more particularly the outside coat or crust, which appeared almost wholly attractable by it.

As two of the more perfect stones which I had obtained, as well as parts of some others, have been examined by several gentlemen well versed in mineralogy and chemistry, I shall not attempt any further description of their constituent parts; nor shall I offer any conjecture respecting the formation of such singular productions, or even record those which I have heard of others, but leave the world to draw their own inferences from the facts above related. I shall only observe, that it is well known there are no volcanos on the continent of India; and, as far as I can learn, no stones have been met with in the earth, in that part of the world, which bear the smallest resemblance to those above described.

It remains for me to speak of a substance mentioned in the *Lithophylacium Bornianum*, Part I. page 125, described thus: "Ferrum retractorium, granulis nitentibus, matrice virescenti "immixtis, (*Ferrum virens* LINN.) cujus fragmenta, ab unius "ad vigenti usque librarum pondus, cortice nigro scoriaceo "circumdata, ad Plann, prope Tabor, circuli Bechinensis Bohe-"miæ, passim reperiuntur."

The iron thus described, is moreover made remarkable by a note,* which observes, that credulous people assert it to have fallen from heaven, during a thunder storm, on the 3d of July, 1753.

The collection of Baron BORN, it is well known, has a place in the cabinet of the Right Hon. CHARLES GREVILLE, who, from the effect produced by comparing the histories and structure of the Italian and Yorkshire stones with the description of this iron, was induced to search the collection of BORN, where he discovered the very substance asserted to have fallen on the 3d of July, 1753. How far these four substances have resemblance to each other, it will soon appear not to be my province to anticipate.

The President having done me the honour to submit his specimens of the Yorkshire and Italian stones to my examination, I became indebted to Mr. GREVILLE and Mr. WILLIAMS for a similar distinction: and, being thus possessed of four substances, to all of which the same origin had been attributed, the necessity of describing them mineralogically did not fail to present itself. To execute this task, no one could be more eager, and certainly no one better qualified, than the Count de BOURNON. He has very obligingly favoured me with the following descriptions.

Mineralogical Description of the various Stones said to have fallen upon the Earth. By the Count de Bournon, *F. R. S.*

The stones I am about to describe, are not of any regular shape; and those which were found in an entire state, that is, those which had not been broken, either by their fall or other-

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* Quæ (fragmenta) 3 Julii, anni 1753, inter tonitrua, e cœlo pluisse creduliores quidam asserunt.

wise, were entirely covered with a black crust, the thickness of which was very inconsiderable.

The stones which fell at Benares, are those of which the mineralogical characters are the most striking: I shall therefore begin the following description with them; and shall afterwards make use of them, as objects of comparison, in describing the others.

STONES FROM BENARES.

These stones, as well as the others described in this Paper, whatever may be their size, are covered over the whole extent of their surface, with a thin crust, of a deep black colour: they have not the smallest gloss; and their surface is sprinkled over with small asperities, which cause it to feel, in some measure, like shagreen, or fish skin.

When these stones are broken, so as to shew their internal appearance, they are found to be of a grayish ash colour; and of a granulated texture, very similar to that of a coarse grit-stone: they appear evidently to be composed of four different substances, which may be easily distinguished, by making use of a lens.

One of these substances, which is in great abundance, appears in the form of small bodies, some of which are perfectly globular, others rather elongated or elliptical. They are of various sizes, from that of a small pin's head to that of a pea, or nearly so: some of them, however, but very few, are of a larger size. The colour of these small globules is gray, sometimes inclining very much to brown: and they are completely opaque. They may, with great ease, be broken in all directions: their fracture is conchoid, and shews a fine, smooth, compact grain, having a small degree of lustre, resembling in some measure that of enamel. Their hardness is such, that, being rubbed upon glass, they act upon it in a slight degree; this action is sufficient to take off its polish, but not to cut it: they give faint sparks, when struck with steel.

Another of these substances, is a martial pyrites, of an indeterminate form: its colour is a reddish yellow, slightly inclining to the colour of nickel, or to that of artificial pyrites. The texture of this substance is granulated, and not very strongly connected: when powdered, it is of a black colour. This pyrites is not attractable by the magnet; and is irregularly distributed through the substance of the stone.

The third of these substances consists in small particles of iron, in a perfectly metallic state, so that they may easily be flattened or extended, by means of a hammer. These particles give to the whole mass of the stone, the property of being attractable by the magnet; they are, however, in less proportion than those of pyrites just mentioned. When a piece of the stone was powdered, and the particles of iron separated from it, as accurately as possible, by means of a magnet, they appeared to compose about $\frac{2}{100}$ of the whole weight of the stone.

The three substances just described, are united together by means of a fourth, which is nearly of an earthy consistence. For this reason, it is easy to separate, with the point of a knife, or even with the nail, the little globular bodies above mentioned, or any other of the constituent parts of the stone we may wish to obtain. Indeed the stone itself may readily be broken, merely by the action of the fingers. The colour of this fourth substance, which serves as a kind of cement to unite the others, is a whitish gray.

The black crust with which the surface of the stone is coated, although it is of no great thickness, emits bright sparks, when struck with steel: it may be broken by a stroke with a hammer; and seems to possess the same properties as the very attractable black oxide of iron. This crust is, however, like the substance of the stone, here and there mixed with small particles of iron in the metallic state: they may easily be made visible, by passing a file over the crust, as they then become evident, on account of their metallic lustre. This is more particularly the case with respect to the crust of those stones which remain to be mentioned, they being much more rich in iron than that I have just described; a circumstance I think it needless to repeat, in the following descriptions of them. The stone now treated of, does not, when breathed upon, emit an argillaceous smell: the same remark may be applied to all the others.

The specific gravity of this stone is 3352.

STONE FROM YORKSHIRE.

This stone, the constituent parts of which are exactly the same as those of the stones from Benares, differs from them, however,

First. In having a finer grain.

Secondly. That the substance described as being in the form of small globular or elliptical bodies, is not so constantly in those forms, but is also found in particles of an irregular shape; a circumstance that is not met with in the other stones: these bodies are likewise, in general, of a smaller size.

Thirdly. The proportion of martial pyrites, which has precisely the same characters as that in the stones from Benares, is less; on the contrary, that of the iron in a metallic state, is much greater. The quantity I was able to separate by means of the magnet, appeared to me to compose about eight or nine parts, in one hundred, of the weight of the whole mass. I observed many pieces of this iron, of a pretty considerable size; one of them, taken from a portion of the stone I had powdered, in order to separate the iron, weighed several grains.

The part of the stone which is in an earthy state, and which serves to connect the other parts together, has rather more consistence than that of the preceding stones; and its appearance does not differ much from that of decomposed felspar or kaolin. The stone itself, therefore, although by no means hard, is rather more difficult to break with the fingers.

The specific gravity of this stone is 3508.

STONE FROM ITALY.

This stone was in a perfectly entire state; consequently, its whole surface was covered over with the black crust peculiar to all stones of this kind. As the stone was of a very small size, it became necessary to sacrifice the whole of it to the investigation of its nature. Its grain was coarse, similar to that of the stones from Benares: in it might be perceived the same gray globular bodies, the same kind of martial pyrites, and the same particles of iron in the metallic state. The proportion of these last was much less than in the stone from Yorkshire; but rather greater than in the stones from Benares. The same kind of gray earthy substance served to connect the different parts together; and nothing more could be perceived, except a few globules, which consisted wholly of black oxide of iron, attractable by the magnet, and one single globule of another substance, which appeared to differ from all those we have already described. This last substance had a perfectly vitreous lustre, and was completely transparent: it was of a pale yellow colour, slightly inclining to green; and its hardness was rather inferior to that of calcareous spar. The quantity of it, however, was too small to be submitted to such an investigation as might have determined its nature. The black crust which covered the stone, was rather thinner than that of the stones already described; and seemed to have undergone a kind of contraction, which had produced in it a number of fissures or furrows, thereby tracing upon the surface the appearance of compartments, similar in some measure to what is observed in the stones called Septaria.

The specific gravity of this stone was 3418.

STONE FROM BOHEMIA.

The internal structure of this stone is very similar to that of the stone from Yorkshire: Its grain is finer than that of the stones from Benares: in it may be observed the same gray substance, both in small globules and in particles of an irregular shape; also the same particles of metallic iron. The same kind of earthy substance likewise served to connect the other parts together.

This stone, however, differs materially from the others.

First. The particles of pyrites cannot be seen without a lens.

Secondly. It contains a much larger quantity of iron in the metallic state; insomuch, that the proportion of that metal, separated from it by means of the magnet, amounted to about $\frac{25}{100}$ of the weight of the whole.

This stone has also (owing perhaps to its having remained a much longer time in the earth than the preceding ones, all of which were taken up nearly at the very instant of their fall,) another difference, viz. many of the particles of iron in a metallic state, have undergone an oxidizement at their surface; a circumstance that has produced a great number of spots, of a yellowish brown colour, and very near to each other, over a part of its internal substance. This oxidizement, by adding to the bulk, and to the force of action, of the part we have described as serving by way of cement to the other constituent parts of the stone, has occasioned a greater degree of adhesion between these parts, and has rendered the substance of the stone more compact.

The great quantity of iron in a metallic state which this stone contains, added to its greater compactness, makes it capable of receiving a slight degree of polish; whereas it is impossible to give any polish to the others. When polished, the iron becomes very evident, in the polished part; appearing in the form of small specks, almost close to each other, which have the colour and lustre peculiar to that metal: these specks are, in general, nearly of an equal size.

The black crust of this stone is similar to that of the others.

The specific gravity of the stone is 4,281.

It is easy to perceive, from the foregoing description, that these stones, although they have not the smallest analogy with any of the mineral substances already known, either of a volcanic or any other nature, have a very peculiar and striking analogy with each other. This circumstance renders them truly worthy to engage the attention of philosophers; and naturally excites a desire of knowing to what causes they owe their existence.

I proceed to consider the assistance to be derived from chemistry, in distinguishing these stones from all other known substances, and in establishing the assertion, that they have fallen on the earth.

The analysis made by the French Academicians, of the stone presented to them by the Abbé BACHELAY, was, in part, conducted by the ever to be deplored LAVOISIER; but it was performed before that celebrated author had enriched chemistry with his last discoveries, and before he had given birth to the system under which it flourishes. The result of this analysis might well induce the conclusion, that the subject of it was common pyritical matter. It was unfortunately made of an aggregate portion of the stone, and not of each distinct substance, irregularly disseminated through it. The proportions obtained were, consequently, as accidental as the arrangement of every substance in the mass.

The analysis of M. BARTHOLD, of the stone of Ensisheim, is subject to the same objections: but, after having the advantage of the foregoing descriptions, the researches which follow cannot be supposed altogether liable to a similar fatality.

EXAMINATION OF THE STONE FROM BENARES.

This stone, as the Count de BOURNON has already remarked, has the most distinguished characters. Indeed it is the only one of the four, sufficiently perfect (if I be allowed that expression) to be subjected to any thing approaching to a regular analysis.

The crust, or external black covering, is the first substance to which the attention is naturally directed. When a portion of this crust had been detached with a knife, or a file, and finely pulverized, I separated the particles attractable by a magnet;

and digested the unattractable portion with nitric acid, which was presently decomposed; but, owing to a strong adherence of some of the interior and earthy parts of the stone, it did not disentangle the coating or metalline part without some difficulty. The acid being sufficiently neutralized, the solution was passed through a filtre, and saturated to excess with ammonia. An abundant precipitate of oxide of iron was produced; and, when this oxide was separated, I observed the saline liquor to have a greenish colour. I evaporated it to dryness; and redissolved the dry salt in distilled water. No precipitate was formed during the evaporation, nor was the colour of the solution entirely destroyed. It appeared to me like a triple salt, described by Mr. HERMSTADT* as an ammoniacal nitrate of nickel. By examination with prussiate of ammonia, it yielded a whitish precipitate, inclining to a violet colour; and, by various properties, I was soon confirmed in the opinion, that nickel was present. Since I shall have occasion more than once to treat of the triple compound, and since it has been only mentioned by Mr. HERMSTADT, it is necessary now to detail some of its distinctive characters. The same chemist informs us, that the three mineral acids, with ammonia, enter into similar combinations with nickel; and I have observed, that oxide of nickel can be dissolved by nitrate and muriate of ammonia. The muriate seems to take up the largest quantity. The colour of this salt is by no means uniform: it is sometimes grass green, violet, rose colour, inclining to purple, and I have seen it almost colourless. It seems to be purple, and to incline to rose colour and violet, when all the oxide of nickel is not united to both acid and alkali, but, from the deficiency of salt, is held in solution by an

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* *Annales de Chimie.* Tom. XXII. p. 108.

excess of ammonia. In this case, evaporation, of course, precipitates the nickel in the state of oxide, which is of a whitish green colour.

The nickel cannot be *precipitated* from a perfectly formed triple salt, by any reagent I have tried, except by a prussiate, or a hydrogenized sulphuret of ammonia. Potash and lime, as well as, I presume, other bodies, standing in the order of affinities before ammonia, decompose the salt; but the nickel is then continued in solution by the disengaged ammonia.

As it may be imagined that I have occasionally met with copper, when I describe a violet or purple ammoniacal solution, it is right to observe, that to avoid this error, I have either reduced the liquor to a neutral state, and endeavoured, without success, to obtain from it a precipitate, with a solution of sulphureted hydrogen gas; or, by adding an acid to slight excess, and immersing a piece of iron, I have not been able to detect a trace of copper. These, and many other trials, when they do not appear to be made before the estimation of the quantities of nickel, have been constantly made afterwards.

But, to return to the incrustation or coating of the stone, the decomposition of the nitric acid shewed the presence of matter at least nearly metallic, although not attractable; and the examinations made of the liquor, from which the iron was precipitated, ascertained the presence of nickel beyond dispute. The difficulty of obtaining the coating of the stone, either distinct from matter not belonging to it, or in sufficient quantity, induced me to relinquish the idea of attempting to give the proportions of its constituent parts.

The stone being deprived of its covering, the shining particles irregularly disseminated, next demand examination. I first examined the pyrites. Their very loose texture made it exceedingly difficult to collect the weight of 16 grains, which was however effected by the dexterity of the Count de BOURNON.

I digested these, at a low heat, with weak muriatic acid; which acted gradually, and disengaged a trifling but sensible quantity of sulphureted hydrogen gas. After several hours, I found the acid discontinued its action. The whole metalline part appeared in solution; but sulphur and earthy particles were observable. The sulphur, from its small specific gravity, was suspended through the solution; whilst the earthy matter, which could not be separated by mechanical means, was fortunately left at the bottom of the digesting vessel. I decanted off the solution, holding suspended the sulphur; and, by repeated washing, separated every thing belonging to the pyrites from the insoluble earthy matter, the subtraction of which reduced the weight of real pyrites to 14 grains. I next obtained the sulphur, by filtration. When it was as dry as I could make it, without fear of its being sublimed, its weight was two grains. To the filtrated liquor I added nitrate of barytes, by way of detecting any sulphuric acid which might have been present; but no cloudiness ensued. I then separated, by sulphate of ammonia, the barytes thus added, and precipitated the iron with ammonia. The liquor, on the subsidence of oxide of iron, appeared of a violet purple colour: it contained nickel, which I threw down with sulphureted hydrogen gas, there being already a sufficient excess of ammonia in the saline liquor to form an alkaline hydrogenized sulphuret. The oxide of iron, after ignition, weighed 15 grains; and the sulphuret of nickel, reduced to an oxide, weighed, after the same treatment, something more than one grain. The proportions of the substances contained in the pyrites of the stone from Benares, may therefore be considered nearly thus:

	Grains.
Sulphur	2
Iron	10½

Since 15 grains of the oxide represent about that quantity of iron,

Nickel, nearly	1
Extraneous earthy matter	2
	15½.

It is observable that, notwithstanding the loss appears to be only half a grain, it was probably more, because the sulphur could not be reduced to the same state of dryness in which it existed when in combination with the iron; not to say that it was, in a small degree, volatilized with the hydrogen gas disengaged during the solution.

The weight of nickel is a mere estimation. We are not yet sufficiently acquainted with that metal to speak of it with accuracy, except as to its presence. Upon the whole, however, it may be concluded, that these pyrites are of a very particular nature; for, although Henkel has observed that sulphur may be separated from pyrites by muriatic acid, it is by no means the usual habitude of pyrites to be of such easy decomposition.

The other shining particles immediately seen, when the internal structure of the stone is exposed, are the malleable iron. Before I state the examination of this iron, I must remark, that preliminary experiments having shewn me it contained nickel, I treated several kinds of the most pure irons I could obtain, with nitric acid; and precipitated the oxide from the metallic salt by ammonia. The quantity of oxide I obtained from 100 grains of iron, was from 144 to 146. I may consequently infer, that 100 grains of pure iron acquires, by such a process, 45 grains of oxygen; and that, whenever a metallic substance, supposed to be iron, does not, under the same circumstances, acquire the same proportionate weight, something is either volatilized, or left in solution. Hence, when a metallic alloy of nickel and iron presents itself, a judgment may, at least, be formed of the quantity of nickel, by the deficiency of weight in the precipitated oxide of iron.

This mode of treatment was not allowed me in the examination of the coating of the stone, because it was impossible to know in what state of oxidizement the iron existed. But, as the particles disseminated through the whole mass, are clearly metallic, a very tolerable idea of the quantities of nickel contained in them will be obtained, by noting the quantity of oxide of iron separated, as above described. 25 grains of these metallic particles were therefore heated with a quantity of nitric acid, much more than sufficient to dissolve the whole. Some earthy matter, which, as in a former case, was not separable by mechanical means, remained after a complete solution of the metal had been effected. This earthy matter, after being ignited, weighed two grains. The real matter of the present examination, was therefore reduced to 23 grains, and was in complete solution. I added ammonia to a very sensible excess. The oxide of iron was thereby precipitated, and, being collected and ignited, it weighed 24 grains; whereas, according to my experiments, 33½ grains should have been produced from the solution, had it contained nothing but iron. I examined the saline liquor, when free from ferruginous particles, and discovered it to be the triple salt of nickel. Hence, allowing for loss, the quantity of nickel may be estimated, by calculating the quantity of iron contained in 24 grains of oxide. Thus, if 145 grains of oxide contain 100 of iron, about 16½ are contained in 24 of oxide. This would suppose the 23 grains of alloy to consist of 16½ iron and 6½ nickel; which, if the usual loss be added to the 16½ grains of iron, and deducted from the nickel, may not be very remote from the truth.

I shall next examine the globular bodies, also irregularly dispersed throughout the stone. A number of them were reduced to fine powder; but nothing metallic could be separated by the magnet. As a preliminary experiment, I sought for pyrites, by digestion with muriatic acid; but no hepatic smell was in the least perceivable, nor was white carbonate of lead at all altered by being held over the mixture. I therefore conclude these globular bodies do not envelope either iron or pyrites. By way of analysis, I treated 100 grains with potash, in a silver crucible; and, after the usual application of a red heat, separated as much silica as possible, by muriatic acid and evaporation. The silica being collected on a filtre, carbonate of potash was added to the filtrated liquor; by which, a precipitate, almost wholly ferruginous, was produced. This precipitate was collected in the common way; then boiled with potash, to extract alumina; and, by supersaturating the alkaline liquor with muriatic acid, and precipitating by carbonate of ammonia, an earth was gathered, which I afterwards found to be partly, if not entirely, siliceous. After redissolving, in muriatic acid, the portion of the ferruginous matter rejected by the potash, I precipitated by ammonia, what I took to be entirely oxide of iron; but, after igniting it, and again attempting to redissolve the whole in muriatic acid, more silica was left. The non-existence of lime was proved, by the addition of carbonate of ammonia, immediately after the same alkali, pure, had thrown down what I took wholly for oxide of iron. I had now obtained every thing in the subject of my analysis, except magnesia and nickel. The former, and a trace of the latter, were held by carbonic acid in the liquor, from which the ferruginous precipitate was, in the first instance, thrown down by carbonate of potash; and the latter was found in the last named muriate of ammonia. I disengaged the magnesia, by the assistance of potash, and by evaporating to dryness. The oxide of nickel was precipitated by hydrogenized sulphuret of ammonia.

Under all circumstances, I am induced to state the proportions of constituent parts thus:

Silica - - - -	50
Magnesia - - -	15
Oxide of iron - - -	34
Oxide of nickel - - -	$2\frac{1}{2}$
	$101\frac{1}{2}$.

The excess of weight, instead of the usual loss, is owing to the difference of oxidizement of the iron, in the stone and in the result of the analysis; which will be found to be the case in all analyses of these substances; indeed it is always necessary to reduce the oxide to the red state, as being the only one to be depended upon. To avoid future repetition, I shall also observe, first, that by preliminary experiments, I could not detect any other substance than those mentioned. Secondly, that the earth obtained as alumina, appeared to me to be mostly, if not entirely, siliceous; because, after it had been ignited, and again treated with potash and muriatic acid, I found it was very nearly all precipitated by evaporation. Thirdly, I examined, and judged of, the silica collected from the oxide of iron, in the same way. Fourthly, the weight of the magnesia is given, not immediately, as obtained by evaporation, but after a subsequent solution in an acid, and precipitation by potash. And, fifthly, the proportions are taken from the mean of two analyses.

Nothing remains to be examined, of the stone from Benares, except the earthy matter, forming a cement or matrix for the substances already examined. 100 grains of this matter were, by mechanical means, separated as perfectly as possible, from the pyrites, iron, and globular bodies, and analysed as above. The mean result of two analyses gave,

Silica - - - -	48
Magnesia - - - -	18
Oxide of iron - - -	34
Oxide of nickel - - -	2½
	102½.

EXAMINATION OF THE STONE FROM SIENNA.

The external coating of this stone appeared to have the same characters as that of the stone from Benares.

The pyrites, although certainly present, were not crystallized in such groups as in the preceding stone; nor could they be separated by mechanical means.

The attractable metal was easily separated by the magnet; but 8½ grains only were collected. I treated them with nitric acid and ammonia, as in a preceding case. Nearly one grain of earthy matter was insoluble; the weight was therefore reduced to rather less than 8 grains. The oxide of iron, precipitated by ammonia, weighed 8 grains; and the saline liquor gave abundant indications of nickel. As 8 grains of this oxide of iron contain nearly 6 of metal, the quantity of nickel, in the bare 8 grains, may be estimated between 1 and 2 grains. Some globular bodies were extracted, but too few to analyze.

Since the pyrites could not be separated, I collected 150 grains of the stone, freed from iron by the magnet, and as exempt as possible from globular bodies. These 150 grains, I first digested with muriatic acid, that the pyrites might be decomposed, and every thing taken up which could be dissolved by that menstruum. A very decided disengagement of sulphureted hydrogen gas was occasioned. When the acid could produce no further action, I collected the undissolved matter on a filtre, and boiled it with the most concentrate nitric acid, in hopes of being able to convert the sulphur, previously liberated, into sulphuric acid; but my endeavours were fruitless; for, upon the addition of nitrate of barytes to the nitric solution, rendered previously transparent, a very insignificant quantity of sulphate of barytes was obtained. The surplus of barytic nitrate was removed by sulphate of potash. I next completely edulcorated the mass which remained insoluble, after the action of the muriatic and nitric acids; and, adding the water of edulcoration to the muriatic and nitric liquors, evaporated the whole for silica. I then submitted the mass, undissolved by the acids and the water, to the treatment with potash, muriatic acid, and evaporation, which was, in the first instance, applied to the stone from Benares. The first precipitation was, as in that analysis, also effected with carbonate of potash; but, instead of endeavouring immediately to extract alumina, I ignited the precipitate, that the alumina or silica remaining might be rendered insoluble. After the ignition, I separated the oxide of iron with very concentrate muriatic acid; and the earths, which were left perfectly white, I heated with potash, until they were again capable of being taken up by the same acid. The solution so made, was slowly evaporated; and, as very nearly every thing was deposited during the evaporation, I conclude all was silica. The proportions resulting from this single analysis, without the weight of sulphur contained in the pyrites irregularly disseminated through the whole, were,

Silica - - - -	70
Magnesia - - - -	34
Oxide of iron - - -	52
Oxide of nickel - - -	3
	159.

EXAMINATION OF THE STONE FROM YORKSHIRE.

The mechanical separation of the substances in this stone being as difficult as in the preceding case, I was necessarily satisfied with submitting it to the same treatment. I collected, however, 34 grains of malleable particles; which, by the process already more than once mentioned, left 4 grains of earthy matter; and, by yielding 37½ of oxide of iron, indicated about 4 grains of nickel.

150 grains of the earthy part of the stone were, by analysis, resolved into,

Silica - - -	75
Magnesia - - -	37
Oxide of iron - - -	48
Oxide of nickel - - -	2
	162.

EXAMINATION OF THE STONE FROM BOHEMIA.

The probability of never being able to obtain another specimen of the very remarkable fragment of this substance, did not allow me to trespass more on the liberality of Mr. Greville, than to detach a small portion. I found it of similar composition to that of the three preceding stones; and the Count de Bournon has already shewn the proportionate quantity of the attractable metal to be very considerable. 16½ grains, left 2½ of extraneous earthy matter; and yielded, by the treatment with nitric acid and ammonia, 17½ grains of oxide of iron. This would seem to induce an estimation of 1½ of nickel in 14 grains, or about 9 per cent.

55 grains of the earthy part of the stone, by the analytical treatment of the two former, afforded,

Silica	25
Magnesia	9½
Oxide of iron	23½
Oxide of nickel	1½
	59½.

The unusual increase of weight in the result of the three last analyses, notwithstanding the entire loss of the sulphur in the pyrites, is obviously owing to the metallic state of the iron combined with the sulphur, as was shewn in a former instance.

I have now concluded the chemical examination of these four extraordinary substances. It unfortunately differs from the analysis made by the French Academicians, of the stone presented to them by the Abbé Bachelay, as well as from that made by Professor Barthold, of the stone of Ensisheim. It is at variance with that of the Academicians, inasmuch as they found neither magnesia nor nickel. It differs from that of Mr. Barthold, as he did not find nickel, but discovered some lime, with 17 per cent. of alumina. With regard to these differences, I have to submit to the chemical world, whether magnesia might not have eluded the action of an acid, when the aggregation of the integrant parts of the stone was not destroyed by treatment with potash. As to the existence of alumina, I do not absolutely deny it; yet I must observe, that the whole of the earth which seemed to have any resemblance, however small, to alumina, was at most 3 per cent. and there seems good reason to consider it as silica. Respecting the existence of lime in the stone of Ensisheim, I must appeal to Professor BARTHOLD, whether, supposing lime a constituent part, sulphate of lime should not have been formed, as well as sulphate of magnesia, when sulphuric acid was generated by igniting the earths and pyrites. And, as to the proportion of alumina, in the same stone, I would ask, at least, whether it would have been so considerable, if the solutions formed by acids, after the treatment with potash, had been evaporated to the requisite dryness: not to observe, that no mention is made of any examination of the properties of the earth called alumina. In the proportion of magnesia, I have the satisfaction to find my analysis correspond very nearly with that of Professor BARTHOLD; and, if what he considered alumina were supposed silica, the stone presented to the French Academy, the stone of Ensisheim, and the four I have examined, would agree very nearly in siliceous proportions. With respect to the nickel, I am confident it would have been found in all, had the metallic particles been separately examined. But, whatever be these variations, the mineralogical description of the French Academicians, of Mr. BARTHOLD, and of the Count de BOURNON, all exhibit a striking conformity of character, common to each of these stones; and I doubt not but the similarity of component parts, especially of the malleable alloy, together with the near approach of the constituent proportions of the earths contained in each of the four stones, the immediate subject of this Paper, will establish very strong evidence in favour of the assertion, that they have fallen on our globe. They have been found at places very remote from each other, and at periods also sufficiently distant. The mineralogists who have examined them, agree that they have no resemblance to mineral substances, properly so called; nor have they been described by mineralogical authors. I would further urge the authenticity of accounts of fallen stones, and the similarity of circumstances attendant on such phenomena; but, to the impartial it would be superfluous, and, to those who disbelieve whatever they cannot explain, it would be fruitless. Attempts to reconcile occurrences of this nature with known principles of philosophy, it is true, are already abundant; but (as the Earl of BRISTOL has well expressed) they leave us a choice of difficulties equally perplexing. It is however remarkable, that Dr. CHLADNI, who seems to have indulged in these speculations with most success, should have connected the descent of fallen stones with meteors; and that, in the narrative of Mr. WILLIAMS, the descent of the stones near Benares, should have been immediately accompanied with a meteor.

No luminous appearance having been perceived during the day on which the stone fell in Yorkshire, it must be admitted, rather militates against the idea, that these stones are the substances which produce or convey the light of a meteor, or that a meteor must necessarily accompany them.* Yet the stones from Sienna fell amidst what was imagined lightning, but what might in reality have been a meteor. Stones were also found,

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* In the account of the stone which fell in Portugal, no mention is made, either of a meteor or lightning.

after the meteor seen in Gascony, in July, 1790. And Mr. FALCONET, in the memoir I have already quoted, relates, that the stone which was adored as the mother of the gods, was a Bœtilia; and that it fell at the feet of the poet PINDAR, enveloped in a ball of fire. He also observes, that all the Bœtilia had the same origin.

I ought not perhaps to suppress, that in endeavouring to form an artificial black coating on the interior surface of one of the stones from Benares, by sending over it the electrical charge of about 37 square feet of glass, it was observed to become luminous, in the dark, for nearly a quarter of an hour; and that the tract of the electrical fluid was rendered black. I by no means wish to lay any stress upon this circumstance; for I am well aware, that many substances become luminous by electricity.

But, should it ever be discovered that fallen stones are actually the bodies of meteors, it would not appear so problematical, that such masses as these stones are sometimes represented, do not penetrate further into the earth: for meteors move more in a horizontal than in a perpendicular direction; and we are as absolutely unacquainted with the force which impels the meteor, as with the origin of the fallen stone.

Before I close this subject, I may be particularly expected to notice the meteor which, a few months ago, traversed the county of Suffolk. It was said, that part of it fell near Saint Edmundsbury, and even that it set fire to a cottage in that vicinity. It appeared, from inquiries made on the spot, that something, seemingly from the meteor, was, with a degree of reason, believed to have fallen in the adjacent meadows; but the time of the combustion of the house did not correspond with the moment of the meteor's transition. A phenomenon much more worthy of attention, has since been described in the Philosophical Magazine. On the night of the 5th of April, 1800, a body wholly luminous, was seen, in America, to move with prodigious velocity. Its apparent size was that of a large house, 70 feet long; and its elevation above the surface of the earth, about 200 yards. The light produced effects little short of sunbeams; and a considerable degree of heat was felt by those who saw it, but no electric sensation. Immediately after it disappeared in the north-west, a violent rushing noise was heard, as if the phenomenon were bearing down the forest before it; and, in a few seconds after, there was a tremendous crash, causing a very sensible earthquake. Search being afterwards made in the place where the burning body fell, every vegetable was found burnt, or greatly scorched, and a considerable portion of the surface of the earth broken up. We have to lament, that the authors of this account did not search deeper than the surface of the ground. Such an immense body, though moving in a horizontal direction, could not but be buried to a considerable depth. Should it have been more than the semblance of a body of a peculiar nature, the lapse of ages may perhaps effect what has now been neglected; and its magnitude and solitary situation become the astonishment of future philosophers.

This leads me to speak of the solitary mass of what has been called native iron, which was discovered in South America, and has been described by Don RUBIN DE CELIS. Its weight was about 15 tons. The same author mentions another insulated mass of the same nature. The whole account is exceedingly interesting; but, being already published in the Philosophical Transactions for the year 1788, it needs not be here repeated.

Mr. PROUST has shewn the mass particularly described, not to be wholly iron, but a mixture of nickel and iron. The Trustees of the British Museum, who are in possession of some fragments of this mass, sent to the Royal Society by Don RUBIN DE CELIS, have done me the honour to permit me to examine them; and I have great satisfaction in agreeing with a chemist so justly celebrated as Mr. PROUST.

The connexion which naturally exists between one mass of native iron and another, immediately turns our attention to the native iron in Siberia, described by PALLAS; and this, we are told, the Tartars considered as a sacred relic, which had dropped from heaven. The nickel found in the one mass, and the traditional history of the other, not to compare the globular bodies of the stone from Benares with the globular concavities and the earthy matter of the Siberian iron, tend to the formation of a chain between fallen stones and all kinds of native iron. How far any real affinity exists between these several substances, very obliging friends have afforded me an opportunity to form some judgment. I am indebted to Mr. GREVILLE and Mr. HATCHETT for portions of almost every known native iron: and the Count de BOURNON has done me the favour particularly to describe them as follows.

Description of various Kinds of native Iron. By the Count de Bournon.

The great number of particles of iron, in a perfectly metallic state, contained in the stone from Bohemia, and the said particles being so near each other, naturally lead to some reflections respecting the existence of native iron, which, by many mineralogists, is still considered as problematical. Let us suppose for a moment, that these particles of iron were to approach still more nearly to each other, so as absolutely to come into contact, and in that manner to form a kind of chain, folded upon itself in the interior part of the substance, and leaving a great number of cavities between the links of the chain so folded. Let us then suppose, that the earthy substance with which these cavities are filled, being very porous, and having but a small degree of consistence, should (as may happen by a variety of causes) be destroyed. It is plain, that if such a destruction were to take place, the iron alone would remain; and, being thus left bare, it would appear in the form of a mass, more or less considerable, of a cellular texture, and as it were ramified; such a form, in short, as that in which most of the native irons we are acquainted with have been found. May it not be fair to attribute to such an origin, the native iron found in Bohemia, a specimen of which was presented by the Academy of Freyberg to Baron BORN, and which came, with the rest of his collection, into the hands of Mr. GREVILLE? May not such also, notwithstanding the enormity of its bulk, be the origin of the mass of native iron found in Siberia, near Mount Kemirs, by the celebrated PALLAS?

We have already seen, in the results of the analyses made by Mr. HOWARD, of the various stones above described, that he constantly found a certain proportion of nickel mixed with the iron they contained. This circumstance recalls to our notice the observations that were made by Mr. PROUST, some time ago, respecting the mixture of nickel in the native iron of South America; and tends to give some additional support to the opinion hinted at in the foregoing paragraph.

The circumstances just mentioned, naturally gave to Mr. HOWARD, as well as to me, a desire to know whether the native iron from Siberia, and that from Bohemia, were also mixed with nickel. Mr. HOWARD, consequently, lost no time in proceeding upon this important investigation. The native iron of Siberia presents some very interesting peculiarities, and has often been referred to, but has not yet been properly described; it is therefore with great pleasure that I add the following description of it, and of some other kinds of native iron, to the description I have already given of the various stones said to have fallen on the earth.

I feel the greater satisfaction in doing this, as the noble collection of Mr. GREVILLE contains two specimens of this iron, in perfect condition; one of which weighs several pounds, and was sent to Mr. GREVILLE by Mr. PALLAS himself: on this account, therefore, I enjoy an advantage that many of the authors who have spoken of this iron probably wanted.

One of these pieces has a cellular and ramified texture, analogous to that of some very porous and light volcanic scoria: this is the usual texture of the specimens of this kind of iron, which are preserved in the various mineralogical collections in Europe. When it is attentively examined, there may be perceived in it, not only empty cells, but also impressions or cavities, of greater or less depth, and sometimes perfectly round, which appear evidently to be the result of the compression of hard bodies, which were situated there, and which, when they came away, left the surface of these cavities quite smooth, and having the lustre of polished metal. Here and there, in some of these cavities, there remains a transparent substance, of a yellowish green colour, of which I shall treat more particularly, when I come to the description of the second of the specimens above mentioned. It is very clear, that the cavities here spoken of owe their existence to this transparent substance; and that the polish of the cavities arises merely from the compression of the said substance, and is the natural consequence of its surface having been in perfect contact with that of the iron.

This iron is very malleable: it may be easily cut with a knife; and may be as easily flattened or extended by means of a hammer. Its specific gravity is 6487; which, however, is very much under that of iron which has been merely melted, and has not been forged. The specific gravity of the native iron of Bohemia, which is nearly as malleable and as easy to be cut, is still less: I found it not to exceed 6146. This low degree of gravity, appears to be owing partly to the oxidizement of the surface of the iron, and partly to there being, in the interior part of its substance, a number of small cavities, which are often rendered visible by fracture, and which have their surfaces also oxidized. The fracture of this iron, presents the same shining and silvery white colour as the common cast iron, known by the name of white cast iron; but its grain is much smoother and finer: it is also much more malleable when cold. BERGMAN says that this iron is brittle, when heated to a red heat. I have frequently tried it in that state, and have constantly found it to be malleable. The same remark may be applied to the native iron from South America; and also to that from Senegal.

The second of the two specimens mentioned above, and which weighs several pounds, presents an aspect that differs, in some respects, from that of the preceding specimen. The most considerable part of it forms a solid compact mass, in which there is not to be perceived the smallest appearance of pores or cavities; but there arises upon its surface, a kind of ramified or cellular part, similar, in every respect, to the specimen already described, and every where completely connected with the substance of the mass itself.

If the compact part of this piece is examined with attention, it will be perceived, that it is not entirely composed of iron in the metallic state, but that it is mixed with nearly an equal quantity of the transparent substance of a yellowish green colour, (sometimes also of a greenish yellow,) already spoken of in the description of the other specimen. This substance is mixed with the iron, in such a manner, that if the whole of the former could be removed, the remaining part would consist merely of iron in the metallic state, and would present the same cellular appearance as the preceding specimen, and the ramified or cellular part of the specimen now described.

This stony part, separated from the iron, appears in the form of small nodules, generally of an irregular shape, but sometimes nearly globular: they have a perfectly smooth and shining surface, so as very often to present the appearance of small balls of glass; a circumstance that has led many persons to suppose them the result of a real vitrification. Some of these nodules have several irregular facets, produced by the compression of the iron in which they were inclosed; but I have never observed in them, any appearances that could lead me to suspect they had the slightest tendency whatever to assume a determined crystalline form.

This substance is always more or less transparent. It is sufficiently hard to cut glass; but has no effect upon quartz. It is very brittle: its fracture is usually conchoid; but I could not perceive that it broke in any particular direction, in such a way that I could consider the fracture as a natural one. It becomes electric by friction. Its specific gravity is from 3263 to 3300. It is very refractory: I kept it, for some time, exposed to a degree of heat sufficiently strong to oxidize, to a considerable depth, the iron crucible in which it was placed, without its having undergone any alteration, except that of having acquired a greater degree of intensity in its colour. Its transparency was not at all diminished. I think, therefore, there is not the smallest reason to allow any probability to the opinion that it ought to be considered as a kind of glass.

Of all substances hitherto known, that with which it seems to have the greatest analogy, is the peridot, (the chrysolite of WERNER,) to which some mineralogists have referred it. The result of Mr. HOWARD's analysis of it, is nearly the same as that of the analysis of the peridot, made by Mr. KLAPROTH.

The hardness and infusibility of this substance are nearly the same as those of the peridot; but it seems to have a rather less degree of specific gravity: that of two very perfect crystals of peridot, I found to be from 3340 to 3375. The crystalline forms of the substance here described, if ever we should be able to determine them, would clear up our doubts respecting the analogy between the two substances. If we consider the compact part of the specimen now treated of, particularly the strong connexion that appears to exist between the iron and the transparent substance, and the great resistance we experience when we attempt to separate them, we cannot help being surprised, that almost all the specimens of this mass of metallic iron that have been brought to Europe, are in the cellular state already described, owing apparently to the total, or almost total, destruction of the transparent substance. But, besides the fragility of this substance, the specimen in question helps very much to explain the above circumstance, inasmuch as many of the nodules of the transparent substance belonging to it, are in a state of real decomposition. In that state, they are changed into a white opaque substance, which, upon being lightly pressed or squeezed between the fingers, crumbles into a gritty dry powder. This decomposition may be observed to have taken place in various degrees: in many of the nodules, the substance is merely become friable, without being much altered in its appearance; whereas, some of those which are in a state of complete decomposition, are of an ochreous reddish yellow colour; it is, however, easy to distinguish that this colour does not belong to them, but is owing only to the oxidizement of the adjacent particles of iron.

From the above observations, it will not be difficult to conceive the possibility of the total, or nearly total, destruction of the transparent substance; and also, the appearance the pieces of iron must naturally present, when deprived of it. I cannot help observing likewise, that there appears to exist a very interesting analogy, between these transparent nodules and the globules I described as making part of the stones said to have fallen on the earth. This analogy, though not a very strong one, may lead us to suppose that the two substances are similar in their nature, but that the globules are less pure, and contain a greater quantity of iron.

The native iron from Bohemia is a compact mass, similar to the compact part of the large specimen of iron from Siberia, which has just been described: like that, also, it contains a number of globular bodies or nodules; but they are not in such great proportion as in the Siberian iron. They are besides perfectly opaque, and very much resemble the most compact of the globules belonging to the stones said to have fallen on the earth.

EXAMINATION OF THE IRON FROM SOUTH AMERICA.

I have already observed, that my experiments coincided with those of Mr. PROUST. He obtained 50 grains of sulphate of nickel, from 100 of this mass. The process I have so frequently mentioned, yielded me 80 grains of oxide of iron from 62 of the metal; which indicates about 7½ of nickel, or about 10 per cent.

EXAMINATION OF THE SIBERIAN IRON.

100 grains of this iron, gave 127 of oxide of iron: hence, it should contain about 17 per cent. of nickel.

The yellow substance belonging to this iron, was analyzed in the same way as the globular bodies, and the earthy parts, of the stone from Benares.

The proportions, resulting from the analysis of 50 grains, and from some previous experiments on other particles, were,

Silica - - - -	27
Magnesia - - - -	13½
Oxide of iron - - -	8½
Oxide of nickel - - -	½
	49½.

EXAMINATION OF THE BOHEMIAN IRON.

26½ grains of this metal, left about 1½ grain of earthy matter, insoluble in nitric acid; and, by ammonia, afforded 30 grains of oxide of iron, inducing an estimation of nearly 5 of nickel.

EXAMINATION OF IRON FROM SENEGAL, BROUGHT BY GENERAL O'HARA, AND GIVEN TO ME BY MR. HATCHETT.

In this experiment, 199 grains of oxide were produced from 145 grains of metal: hence, there may be an estimation of 8 grains in 145, or between 5 and 6 per cent. of nickel.

It will appear, from a collected view of the preceding pages and authorities, that a number of stones asserted to have fallen under similar circumstances, have precisely the same characters. The stones from Benares, the stone from Yorkshire, that from Sienna, and a fragment of one from Bohemia, have a relation to each other not to be questioned.

1st. They have all pyrites of a peculiar character.

2dly. They have all a coating of black oxide of iron.

3dly. They all contain an alloy of iron and nickel. And,

4thly. The earths which serve to them as a sort of connecting medium, correspond in their nature, and nearly in their proportions.

Moreover, in the stones from Benares, pyrites and globular bodies are exceedingly distinct. In the others they are more or less definite; and that from Sienna had one of its globules transparent. Meteors, or lightning, attended the descent of the stones at Benares, and at Sienna. Such coincidence of circumstances, and the unquestionable authorities I have adduced, must, I imagine, remove all doubt as to the descent of these stony substances; for, to disbelieve on the mere ground of incomprehensibility, would be to dispute most of the works of nature.

Respecting the kinds of iron called native, they all contain nickel. The mass in South America is hollow, has concavities, and appears to have been in a soft or welding state, because it has received various impressions.

The Siberian iron has globular concavities, in part filled with a transparent substance, which, the proportional quantity of oxide of iron excepted, has nearly the composition of the globules in the stone from Benares.

The iron from Bohemia adheres to earthy matter studded with globular bodies.

The Senegal iron had been completely mutilated before it came under my examination.

From these facts, I shall draw no conclusion, but submit the following queries.

1st. Have not all fallen stones, and what are called native irons, the same origin?

2dly. Are all, or any, the produce or the bodies of meteors?

And, lastly, Might not the stone from Yorkshire have formed a meteor in regions too elevated to be discovered?

Specimens of the Benares and Yorkshire stones have been deposited, by the President, in the British Museum.

METEOROLOGICAL JOURNAL,

KEPT AT THE APARTMENTS

OF THE

ROYAL SOCIETY,

BY ORDER OF THE

PRESIDENT AND COUNCIL.

METEOROLOGICAL JOURNAL
for January, 1801.

1801	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
Jan. 1	28	8	0	39	50	29,88	85		S	2	Cloudy.
	50	2	0	47	53	29,70	90		S	2	Rain.
2	41	8	0	41	51	30,00	81	0,350	SSW	1	Fair.
	49	2	0	48	53	30,04	77		SW	1	Cloudy.
3	44	8	0	48	52	29,80	78		SSW	2	Cloudy.
	52	2	0	52	54	29,78	83		W	2	Cloudy.
4	39	8	0	41	52	29,70	78	0,030	SSW	1	Fine.
	45	2	0	45	55	29,76	72		SW	1	Fine.
5	43	8	0	52	53	29,45	78	0,042	S	2	Cloudy. [Much wind last night.
	54	2	0	49	56	29,58	75		WSW	2	Cloudy.
6	38	8	0	38	53	29,90	77	0,080	SSW	1	Fine.
	44	2	0	44	56	30,04	70		SSW	1	Fine.
7	38	8	0	38	53	30,20	78		SSW	1	Cloudy.
	45	2	0	45	56	30,20	77		E	1	Cloudy.
8	38	8	0	39	53	30,14	81		E	1	Cloudy.
	41	2	0	41	56	30,14	83		E	1	Cloudy.
9	39	8	0	41	53	30,14	84		E	1	Cloudy.
	44	2	0	44	56	30,17	82		E	1	Cloudy.
10	40	8	0	42	54	30,18	84		E	1	Foggy.
	47	2	0	47	56	30,18	84		SE	1	Cloudy.
11	41	8	0	41	54	30,21	82		E	1	Cloudy.
	42	2	0	42	56	30,11	81		ESE	1	Cloudy.
12	36	8	0	36	53	30,16	80		ESE	1	Cloudy.
	39	2	0	39	54	30,09	80		E	1	Cloudy.
13	38	8	0	40	53	30,01	84		E	1	Cloudy.
	44	2	0	44	55	29,95	82		SE	1	Cloudy.
14	42	8	0	45	53	29,89	86	0,022	S	1	Rain.
	46	2	0	45	56	29,93	75		NW	1	Fair.
15	32	8	0	32	53	29,93	82				Foggy.
	43	2	0	43	56	29,86	73		SSE	2	Fine.
16	38	8	0	42	54	29,68	83	0,225	ESE	1	Rain.
	46	2	0	46	57	29,68	82		S	1	Cloudy.

METEOROLOGICAL JOURNAL
for January, 1801.

1801	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. 17	41	8	0	47	56	29,56	87	0,100	S	2	Rain.
	49	2	0	49	58	29,59	79		SW	1	Cloudy.
18	35	8	0	35	55	29,79	77		SSW	1	Fine.
	46	2	0	46	58	29,76	75		S	1	Fair.
19	38	8	0	39	54	29,77	85	0,352	N	1	Cloudy.
	44	2	0	43	57	29,93	74		N	1	Fair.
20	36	8	0	48	54	29,87	87		S	2	Cloudy.
	54	2	0	54	56	29,80	81		WSW	2	Cloudy.
21	39	8	0	41	54	29,99	79		SW	1	Cloudy.
	47	2	0	46	56	29,94	73		NW	1	Cloudy.
22	38	8	0	38	53	29,73	77		SW	1	Cloudy.
	43	2	0	43	55	29,56	70		WNW	1	Cloudy.
23	32	8	0	34	53	29,30	73		NW	1	Snow.
	38	2	0	38	54	29,38	72		N	2	Cloudy.
24	29	8	0	30	51	29 59	78	0,025	NNE	2	Fair.
	36	2	0	36	53	29,71	69		NNE	2	Cloudy.
25	28	8	0	28	50	29,77	70		NNE	1	Fine.
	33	2	0	33	52	29,78	66		NE	1	Fine.
26	24	8	0	25	49	29,78	73		S	1	Cloudy.
	36	2	0	36	52	29,66	79		SSW	1	Cloudy.
27	36	8	0	38	50	29,69	84		SW	1	Cloudy.
	44	2	0	41	52	29,76	80		NNE	1	Cloudy.
28	36	8	0	42	50	29,63	81		SW	1	Cloudy.
	48	2	0	48	52	29,67	77		WSW	1	Cloudy.
29	41	8	0	42	51	29,62	78		WSW	2	Cloudy.
	52	2	0	52	54	29,58	67		W	2	Cloudy.
30	39	8	0	39	52	29,94	76		WSW	1	Fair.
	44	2	0	44	54	29,92	74		SW	1	Fair.
31	44	8	0	45	53	29,78	75		SW	1	Cloudy.
	49	2	0	49	55	29,83	71		W	1	Cloudy.

METEOROLOGICAL JOURNAL

for February, 1801.

1801	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
Feb. 1	44	7	0	44	54	29,82	76		SSW	1	Cloudy.
	49	2	0	49	55	29,81	74		SSW	1	Cloudy.
2	45	7	0	45	54	29,88	77		S	2	Cloudy.
	50	2	0	49	56	29,90	75		S	2	Cloudy.
3	48	7	0	48	54	29,88	84		S	2	Cloudy.
	54	2	0	54	57	29,88	83		S	2	Cloudy.
4	50	7	0	50	56	29,94	75		S	2	Cloudy.
	57	2	0	56	59	29,92	66		S	2	Fair.
5	48	7	0	49	57	29,98	82	0,054	SSW	1	Cloudy.
	55	2	0	54	60	30,02	72		SSW	1	Cloudy.
6	48	7	0	48	58	29,85	77	0,170	S	2	Cloudy.
	53	2	0	53	60	29,85	70		W	2	Cloudy.
7	39	7	0	39	58	30,20	76		NNE	1	Fair.
	44	2	0	44	59	30,24	70		NE	2	Fair.
8	35	7	0	35	56	30,26	75		E	1	Cloudy.
	43	2	0	43	57	30,23	71		S	1	Cloudy.
9	41	7	0	41	56	30,16	77		E	1	Cloudy.
	40	2	0	40	56	30,16	67		E	1	Cloudy.
10	33	7	0	33	54	30,16	69		NE	1	Cloudy.
	37	2	0	37	56	30,11	72		NE	2	Cloudy.
11	33	7	0	34	53	29,90	75		NE	2	Cloudy.
	35	2	0	34	54	29,86	72		NE	2	Snow.
12	25	7	0	27	50	29,78	70		NE	2	Cloudy.
	33	2	0	33	51	29,64	67		NE	2	Cloudy.
13	27	7	0	27	49	29,61	74		NE	2	Snow.
	30	2	0	30	51	29,66	74		NE	2	Snow.
14	27	7	0	28	48	29,72	74		NE	2	Cloudy.
	31	2	0	31	51	29,68	70		NE	2	Cloudy.
15	28	7	0	31	47	29,52	73		NE	2	Cloudy.
	35	2	0	34	50	29,51	73		NE	2	Cloudy.
16	32	7	0	33	47	29,56	73		NE	1	Cloudy.
	35	2	0	35	50	29,58	70		NE	1	Cloudy.

METEOROLOGICAL JOURNAL

for February, 1801.

1801	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.	
Feb. 17	33	7	0	34	47	29,60	73		ENE	1	Cloudy.
	41	2	0	41	50	29,66	69		ENE	1	Cloudy.
18	30	7	0	30	47	29,81	78		NNE	1	Fine.
	39	2	0	38	51	29,84	69		NNE	1	Fair.
19	32	7	0	33	48	29,74	73		S	1	Cloudy.
	39	2	0	39	50	29,50	73		SSE	2	Cloudy.
20	34	7	0	38	48	29,37	73	0,025	W	2	Fair.
	45	2	0	45	52	29,59	63		W	2	Fair.
21	40	7	0	42	49	29,46	82	0,115	ESE	2	Rain.
	50	2	0	50	53	29,31	85		SSE	1	Rain.
22	39	7	0	39	50	29,20	81	0,033	SSE	1	Cloudy.
	44	2	0	43	53	29,16	78		S	1	Cloudy.
23	36	7	0	36	51	29,27	79	0,092	SSE	1	Fair.
	46	2	0	46	54	29,27	68		SW	1	Fine.
24	31	7	0	33	51	29,45	78		W	1	Cloudy.
	44	2	0	44	54	29,62	67		W	1	Fair.
25	38	7	0	43	52	29,68	81	0,022	S	2	Rain.
	51	2	0	51	54	29,55	84		S	2	Cloudy.
26	42	7	0	42	53	29,79	75	0,033	WSW	1	Fair.
	51	2	0	51	56	29,94	62		W	1	Fair.
27	36	7	0	36	53	30,08	76		W	1	Fair.
	49	2	0	49	56	30,01	68		SSW	2	Cloudy.
28	38	7	0	41	54	29,93	79		SSW	1	Cloudy.
	50	2	0	50	56	29,90	72		S	1	Cloudy.

METEOROLOGICAL JOURNAL

for March, 1801.

1801	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
Mar. 1	44	7	0	49	55	29,82	83	0,135	SSW	2	Cloudy.
	55	2	0	55	57	29,89	79		SSW	2	Cloudy.
2	50	7	0	50	56	30,11	84		S	2	Cloudy.
	58	2	0	58	58	30,22	78		S	1	Cloudy.
3	50	7	0	50	57	30,37	84		SW	1	Cloudy.
	59	2	0	59	60	30,38	71		SW	1	Fair.
4	50	7	0	50	58	30,34	81		WSW	1	Cloudy.
	53	2	0	53	59	30,43	63		NW	1	Cloudy.
5	38	7	0	41	57	30,42	74		SW	1	Cloudy.
	51	2	0	51	59	30,36	63		W	1	Fair.
6	45	7	0	45	58	30,24	74		SW	1	Cloudy.
	49	2	0	49	60	30,15	72		E	1	Cloudy.
7	37	7	0	40	57	30,34	69	0,062	E	1	Cloudy.
	44	2	0	44	61	30,40	65		E	1	Fair.
8	31	7	0	31	56	30,28	75		SW	1	Fair.
	43	2	0	43	58	30,20	72		NW	1	Fair.
9	33	7	0	33	55	29,96	78		WNW	1	Cloudy.
	50	2	0	50	58	29,91	73		WNW	1	Fair.
10	42	7	0	42	56	30,00	80	0,055	E	1	Rain.
	45	2	0	45	57	30,01	80		E	1	Cloudy.
11	43	7	0	44	56	29,72	80	0,056	ESE	1	Cloudy.
	50	2	0	50	58	29,56	80		ESE	2	Cloudy.
12	46	7	0	47	56	29,32	82	0,067	SW	1	Cloudy.
	55	2	0	53	58	29,48	68		NW	1	Cloudy.
13	40	7	0	41	57	29,58	77		WSW	1	Fair.
	52	2	0	52	59	29,63	63		W	1	Fair.
14	45	7	0	49	57	29,38	80		SSW	2	Cloudy.
	54	2	0	52	58	29,22	78		S	2	Cloudy.
15	37	7	0	37	56	29,38	76	0,128	NW	2	Cloudy.
	44	2	0	44	57	29,64	67		NW	2	Cloudy.
16	31	7	0	32	54	29,86	73		SW	1	Fine.
	49	2	0	49	58	29,80	68		SW	2	Cloudy.

METEOROLOGICAL JOURNAL
for March, 1801.

1801	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.	
Mar. 17	43	7	0	44	55	29,72	79	0,084	SW	2	Fine.
	53	2	0	51	58	29,57	73		SSW	2	Cloudy.
18	35	7	0	36	55	29,50	77	0,218	WSW	1	Cloudy.
	51	2	0	47	57	29,50	68		WNW	2	Cloudy.
19	35	7	0	36	54	29,71	73		NW	1	Fine.
	48	2	0	47	56	29,83	63		NW	2	Fair.
20	40	7	0	42	54	29,74	76		SW	1	Cloudy.
	48	2	0	45	55	29,29	76		S	2	Rain.
21	36	7	0	38	52	29,18	73	0,160	W	2	Fair.
	47	2	0	45	56	29,14	75		SW	2	Fair.
22	37	7	0	39	52	29,12	70	0,058	W	2	Fair.
	48	2	0	47	56	29,22	62		W	2	Fair.
23	37	7	0	37	52	29,15	75		WSW	1	Fine.
	49	2	0	47	55	29,30	69		WNW	2	Cloudy.
24	34	7	0	36	52	29,78	75		SW	1	Fair.
	50	2	0	50	54	29,77	62		S	2	Cloudy.
25	34	7	0	35	53	30,04	77	0,085	W	1	Cloudy.
	50	2	0	50	56	30,14	63		SW	1	Fair.
26	41	7	0	45	54	30,18	75		SW	1	Cloudy.
	55	2	0	55	56	30,18	70		SSW	1	Cloudy.
27	48	7	0	48	55	30,05	81		SSW	1	Cloudy.
	56	2	0	56	57	30,01	73		W	1	Cloudy.
28	45	7	0	47	56	30,01	76		SW	1	Cloudy.
	59	2	0	58	58	30,03	65		SSW	1	Cloudy.
29	48	7	0	50	56	30,03	73		SSW	1	Fair.
	59	2	0	59	57	30,03	66		SW	1	Cloudy.
30	50	7	0	51	57	30,21	81		N	1	Cloudy.
	54	2	0	51	59	30,32	79		E	1	Cloudy.
31	40	7	0	43	56	30,40	78		NE	1	Cloudy.
	54	2	0	54	60	30,33	66		ENE	1	Fine.

METEOROLOGICAL JOURNAL

for April, 1801.

1801	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
April 1	38	7	0	40	57	30,32	77		NE	1	Fine.
	57	2	0	57	60	30,31	65		ENE	1	Fine.
2	38	7	0	43	58	30,27	73		NE	1	Fine.
	62	2	0	61	62	30,25	60		E	1	Fine.
3	42	7	0	44	58	30,14	72		NE	1	Hazy.
	65	2	0	64	61	30,04	61		E	1	Fine.
4	45	7	0	47	60	29,86	70		W	1	Hazy.
	65	2	0	65	62	29,87	64		NW	1	Fine.
5	41	7	0	41	60	29,80	64		N	2	Cloudy.
	46	2	0	46	60	29,90	53		NNE	2	Fair.
6	33	7	0	36	57	30,07	70		W	1	Hazy.
	51	2	0	50	58	30,01	57		W	1	Fair.
7	40	7	0	44	56	29,53	66		SSW	2	Cloudy.
	48	2	0	47	57	29,38	69		S	2	Rain.
8	38	7	0	41	55	29,40	76	0,248	W	1	Cloudy.
	49	2	0	49	56	29,54	65		NW	2	Cloudy.
9	32	7	0	34	54	29,78	72		W	1	Fair.
	52	2	0	52	55	29,73	57		SW	2	Fair.
10	42	7	0	43	55	29,48	70	0,038	WNW	2	Fair.
	53	2	0	51	56	29,54	65		NW	2	Fair.
11	34	7	0	37	54	29,63	71	0,052	WNW	2	Fine.
	49	2	0	48	56	29,66	63		WNW	2	Fair.
12	32	7	0	32	54	29,91	78		N	2	Snow.
	39	2	0	39	55	29,97	59		N	2	Fair.
13	30	7	0	33	52	30,28	70		NE	1	Fine.
	45	2	0	44	56	30,28	70		NE	2	Fair.
14	36	7	0	40	53	30,25	73	0,022	NNE	1	Cloudy.
	48	2	0	48	55	30,15	77		NNE	1	Cloudy.
15	39	7	0	39	53	30,18	76	0,016	NE	1	Cloudy.
	49	2	0	49	54	30,12	70		ENE	1	Cloudy.
16	39	7	0	42	53	29,86	77		ENE	1	Cloudy.
	51	2	0	48	54	28,87	70		ESE	2	Cloudy.

METEOROLOGICAL JOURNAL
for April, 1801.

1801	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
Apr. 17	42	7	0	44	54	29,96	75		NE	1	Cloudy.
	57	2	0	57	57	29,89	64		ESE	1	Cloudy.
18	40	7	0	41	55	29,98	73		S	1	Hazy.
	60	2	0	59	59	30,01	58		WNW	1	Fair.
19	42	7	0	44	57	30,16	70		S	1	Fine.
	63	2	0	62	59	30,13	61		N	1	Fine.
20	45	7	0	49	58	30,16	69		W	1	Hazy.
	65	2	0	65	60	30,11	61		NW	1	Fair.
21	48	7	0	50	60	30,16	71		N	1	Cloudy.
	60	2	0	60	60	30,21	60		NE	1	Cloudy.
22	43	7	0	44	58	30,38	65		ENE	1	Cloudy.
	52	2	0	52	60	30,37	62		NE	1	Fair.
23	42	7	0	45	58	30,37	64		NE	2	Cloudy.
	51	2	0	51	60	30,37	60		E	2	Fine.
24	38	7	0	44	58	30,33	67		E	1	Hazy.
	55	2	0	54	60	30,30	55		E	2	Fine.
25	40	7	0	46	58	30,27	72		E	1	Hazy.
	62	2	0	62	62	30,24	56		E	2	Fine.
26	42	7	0	47	59	30,23	69		E	1	Fine.
	63	2	0	63	61	30,22	55		E	2	Fine.
27	43	7	0	48	59	30,27	62		NE	1	Hazy.
	64	2	0	64	61	30,27	56		E	2	Fine.
28	44	7	0	49	59	30,26	68		NE	1	Hazy.
	61	2	0	61	61	30,23	50		E	2	Fine.
29	44	7	0	48	59	30,19	69		NE	1	Hazy.
	60	2	0	60	61	30,15	57		ENE	2	Fine.
30	43	7	0	48	59	30,10	69		NE	1	Hazy.
	62	2	0	62	61	30,04	55		E	2	Fine.

METEOROLOGICAL JOURNAL
for May, 1801.

1801	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	40	7	0	45	58	30,01	73		NE	1	Hazy.
	54	2	0	54	59	29,94	61		NNE	2	Cloudy.
2	43	7	0	48	58	29,68	73		NE	2	Cloudy.
	53	2	0	52	58	29,65	66		NE	2	Cloudy.
3	39	7	0	44	57	29,72	73		NE	1	Cloudy.
	58	2	0	57	58	29,77	58		WNW	1	Fair.
4	48	7	0	52	57	29,96	67		NW	1	Hazy.
	65	2	0	64	59	29,99	56		N	1	Fair.
5	45	7	0	48	58	30,12	72		NE	1	Hazy.
	64	2	0	62	60	30,14	57		E	1	Fine.
6	43	7	0	46	58	30,16	70		NE	1	Cloudy.
	61	2	0	61	60	30,11	59		NNE	1	Fine.
7	43	7	0	47	58	30,10	71		NNE	2	Cloudy.
	62	2	0	62	59	30,05	60		NNE	2	Cloudy.
8	43	7	0	47	58	30,02	67		N	1	Fine.
	63	2	0	61	60	29,95	57		N	1	Fair.
9	46	7	0	50	60	29,95	65		WNW	1	Fair.
	59	2	0	59	60	29,97	57		WNW	1	Cloudy.
10	42	7	0	47	58	30,11	66		SW	1	Fine.
	63	2	0	63	59	30,11	56		NW	1	Fair.
11	45	7	0	48	58	30,15	68		WSW	1	Fine.
	67	2	0	66	60	30,12	60		WSW	1	Fair.
12	46	7	0	50	59	29,95	70		SW	1	Fine.
	64	2	0	57	59	29,88	72		W	1	Rain.
13	43	7	0	47	59	29,85	70	0,235	N	1	Fair.
	59	2	0	59	59	29,85	58		NW	1	Cloudy.
14	40	7	0	44	58	29,88	73	0,031	SE	1	Cloudy.
	60	2	0	58	58	29,84	60		S	2	Cloudy.
15	49	7	0	50	57	29,64	76	0,133	S	2	Rain.
	60	2	0	60	58	29,58	67		S	2	Cloudy.
16	46	7	0	52	58	29,73	74	0,048	SSW	1	Fine.
	65	2	0	65	59	29,75	62		SSW	2	Cloudy.

METEOROLOGICAL JOURNAL
for May, 1801.

1801	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
May 17	51	7	0	54	59	29,68	72		SE	2	Cloudy.
	64	2	0	64	60	29,64	63		SSW	2	Cloudy.
18	51	7	0	52	59	29,73	73	0,195	S	1	Rain.
	61	2	0	60	60	29,75	67		SE	1	Cloudy.
19	51	7	0	53	59	29,87	70	0,050	NNE	1	Cloudy.
	66	2	0	64	60	29,89	60		N	1	Cloudy.
20	46	7	0	50	60	30,01	52		W	1	Hazy.
	66	2	0	66	61	30,00	55		S	1	Fair.
21	48	7	0	50	60	29,92	70		S	1	Fine.
	70	2	0	70	62	29,82	56		S	1	Fine.
22	48	7	0	55	61	29,74	64		S	1	Fine.
	70	2	0	70	62	29,68	59		SE	2	Fair.
23	55	7	0	58	62	29,62	67		SE	1	Hazy.
	70	2	0	68	63	29,62	61		SSE	2	Fair.
24	54	7	0	55	63	29,77	70		S	2	Fine.
	71	2	0	71	64	29,79	54		SSW	2	Fair.
25	50	7	0	56	63	29,78	56		E	1	Cloudy.
	71	2	0	69	64	29,71	54		ESE	2	Fair.
26	52	7	0	57	63	29,62	75		E	1	Cloudy.
	65	2	0	65	63	29,62	71		SE	1	Cloudy.
27	50	7	0	54	62	29,65	73	0,223	SW	1	Cloudy.
	60	2	0	57	62	29,67	70		SSW	1	Rain.
28	50	7	0	54	61	29,59	73	0,185	S	2	Cloudy.
	63	2	0	63	62	29,63	66		S	2	Cloudy.
29	50	7	0	54	61	29,44	72	0,032	SSE	2	Cloudy.
	66	2	0	66	62	29,44	66		SSE	2	Cloudy.
30	54	7	0	57	61	29,54	76		SE	1	Rain.
	64	2	0	64	62	29,47	78		E	1	Rain.
31	54	7	0	56	62	29,53	86	0,377	NE	1	Rain.
	60	2	0	59	62	29,61	82		NE	1	Rain.

METEOROLOGICAL JOURNAL

for June, 1801.

1801	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
June 1	52	7	0	54	61	29,76	80	0,048	NE	1	Cloudy.
	60	2	0	59	62	29,85	77		N	1	Cloudy.
2	53	7	0	55	61	29,90	80		E	1	Cloudy.
	62	2	0	62	62	29,90	78		SE	1	Rain.
3	53	7	0	56	61	29,85	78	0,125	SSE	1	Cloudy.
	66	2	0	65	62	29,86	62		E	1	Cloudy.
4	55	7	0	59	62	29,97	77		NE	1	Cloudy.
	68	2	0	68	63	30,00	66		NE	1	Fair.
5	53	7	0	58	63	30,17	74		E	1	Cloudy.
	68	2	0	66	63	30,18	72		NE	1	Cloudy.
6	55	7	0	58	63	30,19	78	0,070	E	1	Cloudy.
	76	2	0	76	66	30,16	56		SE	1	Fine.
7	56	7	0	58	64	30,28	80	0,130	NE	1	Cloudy.
	68	2	0	67	65	30,21	70		NE	1	Hazy.
8	59	7	0	61	64	30,33	69		SW	1	Cloudy.
	77	2	0	76	67	30,29	63		NW	1	Fair.
9	63	7	0	67	66	30,30	72		NW	1	Fine.
	80	2	0	79	68	30,30	62		NE	1	Fine.
10	62	7	0	66	68	30,28	69		N	1	Fair.
	80	2	0	80	69	30,12	58		W	2	Fine.
11	57	7	0	59	69	30,03	62		NW	2	Fine.
	66	2	0	66	69	30,08	53		N	2	Fine.
12	50	7	0	56	67	30,13	62		NW	1	Fair.
	66	2	0	66	67	29,98	55		NW	2	Fair.
13	44	7	0	51	65	29,81	65	0,058	N	2	Fair.
	56	2	0	48	64	29,80	64		N	2	Cloudy.
14	43	7	0	50	60	30,01	66		NE	2	Fine.
	63	2	0	62	62	30,05	55		N	1	Fair.
15	47	7	0	54	61	30,11	70		W	1	Cloudy.
	67	2	0	65	62	30,08	62		W	1	Cloudy.
16	51	7	0	54	62	30,02	63		W	2	Cloudy.
	64	2	0	64	62	30,04	58		NW	2	Cloudy.

METEOROLOGICAL JOURNAL

for June, 1801.

1801	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
June 17	47	7	0	52	61	30,08	64		N	1	Fair.
	65	2	0	64	62	30,08	55		N	1	Fair.
18	48	7	0	53	61	30,08	64		NE	1	Hazy.
	70	2	0	70	63	30,05	54		NE	1	Fine.
19	53	7	0	56	62	30,08	68		E	1	Cloudy.
	70	2	0	70	63	30,08	58		E	1	Hazy.
20	54	7	0	57	62	30,08	64		W	1	Fair.
	73	2	0	73	64	30,04	53		NE	1	Hazy.
21	51	7	0	56	63	30,04	68		E	1	Fine.
	66	2	0	66	63	30,02	61		ENE	1	Fair.
22	46	7	0	54	62	29,96	65		NE	1	Cloudy.
	63	2	0	63	63	29,92	58		E	2	Fine.
23	45	7	0	52	62	29,89	65		NE	1	Cloudy.
	59	2	0	59	62	29,89	64		NE	2	Cloudy.
24	52	7	0	54	61	29,83	67		E	2	Cloudy.
	65	2	0	65	62	29,86	62		E	2	Fair.
25	53	7	0	56	62	29,82	67		ENE	1	Hazy.
	70	2	0	69	63	29,82	60		E	1	Fine.
26	55	7	0	59	63	29,95	65		NE	1	Cloudy.
	73	2	0	73	63	30,03	57		NW	1	Cloudy.
27	57	7	0	59	63	30,20	67		NE	1	Hazy.
	78	2	0	78	65	30,18	55		SW	1	Hazy.
28	54	7	0	57	64	30,26	67		W	1	Fine.
	78	2	0	78	67	30,24	54		W	1	Fine.
29	55	7	0	58	66	30,24	68		SSW	1	Fine.
	80	2	0	80	67	30,14	54		SSW	1	Fine.
30	61	7	0	62	67	29,91	64		SSE	1	Cloudy.
	70	2	0	70	67	29,81	63		E	1	Rain.

METEOROLOGICAL JOURNAL

for July, 1801.

1801	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.	
July 1	54	7	0	57	66	29,69	73	0,960	SW	1	Fair.
	68	2	0	68	67	29,62	60		SSE	2	Fair.
2	54	7	0	57	66	29,60	74	0,056	E	1	Cloudy.
	70	2	0	70	67	29,60	61		SSW	2	Fair.
3	55	7	0	55	66	29,65	92	0,780	N	1	Rain.
	66	2	0	64	67	29,67	65		NE	1	Rain.
4	55	7	0	57	66	29,77	77	0,207	WSW	1	Fair.
	70	2	0	70	66	29,78	61		SSW	1	Fair.
5	57	7	0	58	66	29,71	75	0,108	SSW	2	Fair.
	70	2	0	70	66	29,71	59		SSW	2	Cloudy.
6	56	7	0	58	65	29,67	78	0,042	E	1	Rain.
	69	2	0	69	66	29,63	71		SE	1	Cloudy.
7	57	7	0	60	66	29,72	76	0,026	S	2	Fair.
	73	2	0	73	67	29,71	63		S	2	Fair.
8	59	7	0	59	66	29,60	73		S	2	Fair.
	70	2	0	70	67	29,53	67		S	2	Rain.
9	56	7	0	58	65	29,48	75	0,115	SW	2	Cloudy.
	66	2	0	66	66	29,58	63		W	2	Cloudy.
10	47	7	0	52	64	29,84	68	0,063	NW	1	Fine.
	66	2	0	66	66	29,87	60		NW	1	Cloudy.
11	50	7	0	53	63	29,89	67		SSW	1	Cloudy.
	69	2	0	67	63	29,77	71		S	1	Cloudy.
12	58	7	0	58	64	29,57	83	0,180	SSW	1	Rain.
	68	2	0	66	65	29,56	58		W	2	Fair.
13	53	7	0	56	64	29,82	74	0,035	W	1	Fair.
	70	2	0	70	64	29,76	64		SSW	1	Cloudy.
14	55	7	0	57	64	29,76	80	0,050	SW	1	Cloudy.
	74	2	0	73	65	29,74	61		W	1	Fair.
15	56	7	0	57	64	29,56	77	0,105	S	1	Rain.
	69	2	0	68	64	29,42	68		S	1	Cloudy.
16	51	7	0	53	64	29,39	75	0,240	W	1	Fair.
	64	2	0	60	63	29,38	67		WNW	1	Cloudy.

METEOROLOGICAL JOURNAL

for July, 1801.

1801	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	51	7	0	53	63	29,47	76	0,177	S	1	Cloudy.
	67	2	0	66	63	29,54	63		SSE	1	Cloudy.
18	50	7	0	55	61	29,78	77		N	1	Cloudy.
	72	2	0	69	64	29,84	63		W	1	Fair.
19	53	7	0	55	63	29,95	79		SW	1	Cloudy.
	77	2	0	77	65	29,98	60		W	1	Fine.
20	55	7	0	57	64	30,10	80		NE	1	Cloudy.
	75	2	0	73	67	30,12	62		NE	1	Fine.
21	56	7	0	62	66	30,12	67		SW	1	Hazy.
	79	2	0	79	69	30,10	57		N	1	Fine.
22	61	7	0	66	68	30,10	65		N	1	Hazy.
	79	2	0	79	70	30,12	63		N	1	Hazy.
23	63	7	0	63	69	30,14	73	0,052	NE	1	Cloudy.
	72	2	0	72	69	30,12	66		E	1	Cloudy.
24	54	7	0	58	68	30,02	72		NE	1	Fine.
	69	2	0	69	68	29,95	60		ENE	2	Fair.
25	54	7	0	57	67	29,90	72	0,135	NE	1	Cloudy.
	71	2	0	71	68	29,88	62		NE	1	Cloudy.
26	55	7	0	56	67	29,88	70		W	1	Hazy.
	76	2	0	76	68	29,89	57		SW	1	Fine.
27	57	7	0	60	67	29,93	76		E	1	Cloudy.
	72	2	0	72	68	29,91	66		E	1	Fair.
28	57	7	0	62	67	29,91	75		E	1	Cloudy.
	70	2	0	68	68	29,91	63		E	1	Cloudy.
29	55	7	0	58	67	29,91	78	0,048	ENE	1	Cloudy.
	66	2	0	66	67	29,87	71		E	1	Cloudy.
30	56	7	0	61	66	29,59	94	0,145	E	1	Rain.
	76	2	0	74	68	29,56	68		S	1	Fair.
31	58	7	0	60	67	29,59	77		SE	1	Fair.
	74	2	0	72	68	29,57	67		SE	2	Fair.

METEOROLOGICAL JOURNAL

for August, 1801.

1801	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.	
Aug. 1	60	7	0	62	67	29,67	75	0,030	S	2	Cloudy.
	75	2	0	74	68	29,69	63		SW	2	Cloudy.
2	59	7	0	60	67	29,90	75	0,022	SW	2	Fair.
	78	2	0	74	68	29,93	68		SW	1	Rain.
3	59	7	0	60	67	30,01	81	0,153	SW	1	Cloudy.
	69	2	0	63	68	30,05	87		NE	1	Cloudy.
4	52	7	0	55	67	30,12	85	0,607	NE	1	Cloudy.
	74	2	0	71	68	30,10	62		N	1	Fine.
5	58	7	0	61	67	30,09	75		WNW	1	Cloudy.
	70	2	0	68	67	30,09	68		NW	1	Cloudy.
6	58	7	0	61	67	30,22	70		NE	1	Cloudy.
	74	2	0	72	68	30,22	61		NE	1	Cloudy.
7	59	7	0	61	67	30,34	74		E	1	Cloudy.
	76	2	0	76	68	30,34	56		E	1	Fair.
8	54	7	0	60	67	30,34	71		E	1	Fair.
	77	2	0	73	69	30,26	61		ENE	1	Fair.
9	58	7	0	60	68	30,22	68		NE	1	Cloudy.
	68	2	0	68	68	30,18	63		NNE	1	Cloudy.
10	56	7	0	60	67	30,14	76		NE	1	Cloudy.
	76	2	0	74	69	30,11	58		E	1	Fine.
11	57	7	0	61	68	30,07	75		NE	1	Fine.
	76	2	0	76	70	30,03	56		NE	1	Fine.
12	57	7	0	63	68	30,00	82		NE	1	Cloudy.
	76	2	0	76	71	29,94	61		NE	2	Fine.
13	61	7	0	61	69	29,83	86	0,207	NE	1	Rain.
	71	2	0	70	69	29,78	79		NW	1	Cloudy.
14	61	7	0	61	69	29,85	90	0,510	NE	1	Cloudy.
	71	2	0	71	69	29,89	65		NE	1	Cloudy.
15	61	7	0	61	69	30,07	77		NE	1	Cloudy.
	73	2	0	73	69	30,13	62		NE	1	Cloudy.
16	62	7	0	62	69	30,24	73		E	1	Cloudy.
	71	2	0	70	69	30,27	67		E	1	Cloudy.

METEOROLOGICAL JOURNAL
for August, 1801.

1801	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
Aug. 17	57	7	0	58	68	30,32	72		E	1	Fair.
	76	2	0	75	70	30,27	62		SE	1	Fine.
18	55	7	0	60	68	30,22	80		E	1	Cloudy.
	74	2	0	73	70	30,18	62		E	1	Fine.
19	56	7	0	58	68	30,17	78		ENE	1	Hazy.
	74	2	0	74	72	30,11	60		E	1	Fine.
20	57	7	0	61	69	30,04	80		E	1	Cloudy.
	79	2	0	79	72	29,97	60		E	1	Fine.
21	59	7	0	61	70	30,04	78		NNE	1	Cloudy.
	71	2	0	71	70	30,05	65		E	1	Cloudy.
22	58	7	0	61	69	30,12	80		NE	1	Fine.
	71	2	0	71	70	30,14	55		E	1	Fine.
23	52	7	0	56	68	30,20	70		NE	1	Fine.
	69	2	0	69	68	30,19	55		E	2	Fine.
24	51	7	0	54	67	30,15	70		SE	1	Fine.
	74	2	0	73	68	30,10	57		NE	1	Fair.
25	55	7	0	58	66	30,17	70		NE	1	Fair.
	72	2	0	71	67	30,16	57		NE	1	Fair.
26	60	7	0	63	66	30,14	65		SW	1	Cloudy.
	71	2	0	69	67	30,10	61		E	1	Cloudy.
27	58	7	0	62	66	30,06	69		ENE	1	Hazy.
	73	2	0	73	67	30,01	55		E	1	Fair.
28	51	7	0	54	65	29,95	69		NNE	1	Fair.
	74	2	0	74	67	29,90	56		N	1	Fair.
29	55	7	0	58	66	29,88	68		W	1	Hazy.
	76	2	0	76	69	29,86	57		W	1	Hazy.
30	60	7	0	61	68	29,85	76		SW	1	Cloudy.
	77	2	0	76	70	29,81	56		WSW	2	Fair.
31	62	7	0	62	68	29,61	78	0,040	SSW	1	Rain.
	69	2	0	68	68	29,57	70		NW	1	Cloudy.

METEOROLOGICAL JOURNAL

for September, 1801.

1801	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
Sept. 1	50	7	0	53	66	29,78	71	0,057	W	1	Fine.
	69	2	0	68	66	29,83	57		W	1	Fair.
2	56	7	0	58	64	29,72	73		SW	1	Cloudy.
	67	2	0	66	66	29,66	57		SW	2	Fair.
3	54	7	0	55	59	29,72	70	0,045	SW	1	Cloudy.
	69	2	0	69	66	29,71	59		WSW	1	Cloudy.
4	58	7	0	60	62	29,50	81	0,150	SSW	2	Fair.
	67	2	0	66	65	29,40	77		SSW	2	Rain.
5	60	7	0	62	63	29,41	75	0,038	SW	2	Cloudy.
	73	2	0	73	64	29,54	62		WSW	2	Cloudy.
6	60	7	0	60	63	29,44	81	0,325	E	1	Cloudy.
	71	2	0	69	64	29,38	73		E	1	Cloudy.
7	61	7	0	62	62	29,54	78	0,018	WNW	1	Cloudy.
	66	2	0	66	64	29,72	73		NW	2	Cloudy.
8	53	7	0	55	62	30,10	78		NW	1	Cloudy.
	69	2	0	68	64	30,14	64		NNW	1	Cloudy.
9	60	7	0	60	62	30,20	74		E	1	Cloudy.
	70	2	0	70	64	30,20	65		E	1	Fair.
10	51	7	0	52	62	30,18	70		ENE	1	Fair.
	66	2	0	66	64	30,18	60		NE	1	Cloudy.
11	56	7	0	56	62	30,16	67		NE	1	Cloudy.
	66	2	0	65	65	30,11	63		NE	1	Cloudy.
12	57	7	0	57	62	30,00	84		NE	1	Cloudy.
	64	2	0	62	64	29,97	77		NE	1	Cloudy.
13	57	7	0	58	62	29,94	82	0,028	NE	1	Cloudy.
	65	2	0	65	63	29,94	78		NE	1	Cloudy.
14	59	7	0	59	62	30,02	83		SE	1	Cloudy.
	71	2	0	66	64	30,05	74		NW	1	Rain.
15	60	7	0	60	63	30,24	84		NE	1	Cloudy.
	70	2	0	70	64	30,30	65		NE	1	Fair.
16	54	7	0	55	62	30,33	80		ENE	1	Cloudy.
	69	2	0	68	64	30,25	65		ENE	1	Fine.

METEOROLOGICAL JOURNAL

for September, 1801.

1801	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
Sep. 17	56	7	0	59	63	30,04	84		NE	1	Cloudy.
	69	2	0	68	65	29,87	73		E	1	Cloudy.
18	62	7	0	62	62	29,60	86		E	1	Cloudy.
	71	2	0	71	65	29,57	71		S	2	Fair.
19	55	7	0	55	63	29,72	77	0,205	WSW	1	Fine.
	69	2	0	69	66	29,83	60		WSW	1	Fair.
20	55	7	0	57	63	29,90	78		SSW	1	Fair.
	67	2	0	67	65	29,89	67		SSW	1	Cloudy.
21	53	7	0	54	61	29,81	73		WNW	1	Cloudy.
	64	2	0	64	63	29,81	65		NW	1	Fair.
22	54	7	0	54	60	29,86	80		N	1	Rain.
	56	2	0	56	61	29,86	76		NW	1	Rain.
23	53	7	0	54	60	29,75	84	0,086	S	1	Cloudy.
	63	2	0	63	61	29,88	75		E	1	Cloudy.
24	48	7	0	48	59	29,98	79	0,197	NE	1	Fair.
	62	2	0	61	60	30,00	69		NE	1	Fair.
25	46	7	0	47	58	30,04	77		SW	1	Fine.
	59	2	0	59	61	30,00	63		NE	1	Fine.
26	51	7	0	51	59	29,90	75		E	1	Fair.
	61	2	0	61	59	29,84	75		E	1	Cloudy.
27	58	7	0	59	60	29,90	92	0,115	SW	1	Cloudy.
	71	2	0	70	63	29,97	68		SW	1	Cloudy.
28	57	7	0	59	62	30,05	87		E	1	Fine.
	67	2	0	67	63	30,04	76		SSE	2	Cloudy.
29	59	7	0	59	63	29,94	87		SSE	2	Fair.
	70	2	0	69	65	29,85	69		S	2	Cloudy.
30	55	7	0	55	63	29,85	81		SW	1	Fair.
	68	2	0	64	64	29,91	73		W	1	Rain.

METEOROLOGICAL JOURNAL

for October, 1801.

1801	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
Oct. 1	59	7	0	59	61	30,16	71	0,046	NW	1	Fine.
	59	2	0	59	63	30,20	63		NW	1	Fine.
2	48	7	0	52	61	30,22	72		W	1	Fair.
	65	2	0	65	63	30,18	63		S	1	Fair.
3	51	7	0	52	61	30,07	77		E	1	Fine.
	65	2	0	65	63	30,05	68		WSW	1	Fair.
4	55	7	0	56	62	29,92	78		S	1	Cloudy.
	61	2	0	61	63	29,81	74		SW	1	Fair.
5	51	7	0	52	61	29,67	78	0,180	SW	1	Rain.
	61	2	0	61	62	29,62	71		SW	1	Fair.
6	47	7	0	47	61	29,66	78		SW	1	Cloudy.
	57	2	0	57	61	29,72	68		NW	1	Cloudy.
7	42	7	0	42	58	29,77	77		SW	1	Fair.
	58	2	0	58	60	29,70	70		SSW	1	Fair.
8	50	7	0	54	59	29,42	74		ESE	2	Cloudy.
	58	2	0	58	60	29,38	77		ESE	2	Rain.
9	49	7	0	50	59	29,55	80	0,130	SSE	1	Fair.
	61	2	0	61	62	29,65	67		SE	1	Fair.
10	49	7	0	50	61	29,68	83				Foggy.
	65	2	0	65	63	29,61	68		E	1	Fair.
11	54	7	0	55	62	29,52	87	0,537	E	1	Cloudy. Much thunder & lightning last night.
	61	2	0	61	63	29,53	76		WNW	1	Cloudy.
12	47	7	0	47	62	29,77	85		WNW	1	Fair.
	58	2	0	58	64	29,84	83		SW	1	Fine.
13	48	7	0	51	62	29,92	82		E	1	Fair.
	63	2	0	63	64	29,92	83		S	1	Fair.
14	55	7	0	59	63	29,98	90	0,020	S	2	Cloudy.
	66	2	0	63	64	29,96	67		S	2	Cloudy.
15	53	7	0	54	63	29,94	86	0,067	S	1	Cloudy.
	60	2	0	60	64	29,80	77		S	2	Rain.
16	47	7	0	47	60	29,71	85	0,090	SW	1	Fair.
	60	2	0	60	63	29,73	70		SSW	1	Fine.

METEOROLOGICAL JOURNAL
for October, 1801.

1801	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.	o	o	Inches.		Inches.	Points.	Str.	
Oct. 17	50	7	0	52	62	29,66	80		SW	1	Cloudy.
	60	2	0	58	62	29,51	80		SSE	1	Rain.
18	55	7	0	55	62	29,16	74	0,140	SW	2	Fine. [Much wind last night.
	59	2	0	59	64	29,04	66		SSW	2	Cloudy.
19	46	7	0	47	61	29,48	79	0,065	SW	1	Fair.
	55	2	0	54	62	29,51	69		N	1	Cloudy.
20	39	7	0	39	60	29,69	77	0,014	NW	1	Fine.
	52	2	0	50	62	29,74	63		NW	1	Fine.
21	40	7	0	45	58	29,48	77		WSW	1	Cloudy.
	47	2	0	45	61	29,46	68		NNE	1	Fine.
22	34	7	0	34	57	29,51	72		W	1	Fine.
	47	2	0	47	58	29,57	73		NW	1	Cloudy.
23	45	7	0	46	56	29,97	77		N	1	Cloudy.
	53	2	0	53	59	30,06	69		N	1	Fair.
24	40	7	0	40	57	30,15	76		NNE	1	Fine.
	50	2	0	50	60	30,09	67		NNE	1	Fine.
25	42	7	0	42	57	30,04	82		NNE	1	Fine.
	53	2	0	53	58	30,07	75		N	1	Cloudy.
26	45	7	0	47	56	30,28	79		N	1	Cloudy.
	56	2	0	56	59	30,34	75		NE	1	Cloudy.
27	47	7	0	47	57	30,34	80		W	1	Cloudy.
	56	2	0	56	59	30,25	69		SW	1	Cloudy.
28	46	7	0	47	57	30,08	77	0,185	SW	1	Cloudy.
	49	2	0	49	60	30,16	03		NNE	1	Fine.
29	38	7	0	41	57	30,21	72		SSE	1	Cloudy.
	54	2	0	54	58	30,10	77		S	1	Cloudy.
30	52	7	0	53	58	29,99	82		SW	2	Cloudy.
	59	2	0	59	60	29,92	73		SW	1	Cloudy.
31	55	7	0	56	59	29,98	87		SW	1	Cloudy.
	62	2	0	62	62	29,98	70		WSW	1	Cloudy.

METEOROLOGICAL JOURNAL

for November, 1801.

1801	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. 1	56	7	0	56	60	29,70	83	0,060	S	2	Cloudy.
	60	2	0	60	62	29,68	73		SW	2	Fair.
2	59	7	0	59	60	29,36	82	0,395	S	2	Rain.
	56	2	0	56	62	28,85	77		S	2	Rain.
3	37	7	0	37	58	29,88	69	0,381	NW	1	Cloudy.
	42	2	0	42	58	29,80	71		E	1	Cloudy.
4	41	7	0	46	57	29,09	91	1,105	NE	1	Rain.
	50	2	0	48	58	29,12	95		NE	1	Rain.
5	31	7	0	31	53	29,91	67	0,200	NW	1	Fine.
	38	2	0	38	56	30,10	61		NW	1	Fine.
6	30	7	0	30	53	30,33	76		WSW	1	Fine.
	42	2	0	42	55	30,31	72		S	1	Fine.
7	32	7	0	34	52	30,02	75	0,235	NE	1	Cloudy.
	40	2	0	40	54	29,81	73		E	1	Rain.
8	35	7	0	35	52	29,80	84		SW	1	Fine.
	48	2	0	47	54	29,92	80		SSE	1	Fair.
9	39	7	0	39	52	30,07	88		E	1	Cloudy.
	48	2	0	48	52	30,08	87		NE	1	Cloudy.
10	41	7	0	43	53	30,00	88		E	1	Cloudy.
	48	2	0	48	55	29,96	83		E	1	Cloudy.
11	44	7	0	46	53	29,81	92				Foggy.
	53	2	0	53	55	29,75	85		S	1	Cloudy.
12	44	7	0	44	54	29,77	90		WSW	1	Fine.
	49	2	0	48	57	29,84	76		SW	1	Fair.
13	35	7	0	37	55	29,88	82		NE	1	Foggy.
	44	2	0	44	56	29,91	84		NE	1	Rain.
14	43	7	0	43	55	30,05	85	0,073	NW	1	Cloudy.
	44	2	0	43	56	30,11	82		SW	1	Cloudy.
15	37	7	0	41	55	30,18	85		SSW	1	Cloudy.
	53	2	0	53	57	30,16	86		S	1	Cloudy.
16	41	7	0	47	56	30,18	85		S	1	Cloudy.
	55	2	0	55	57	30,18	78		SW	1	Cloudy.

METEOROLOGICAL JOURNAL

for November, 1801.

1801	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
Nov. 17	51	7	0	51	56	30,15	91		SSW	1	Cloudy.
	54	2	0	54	58	30,08	85		SW	1	Cloudy.
18	49	7	0	49	57	29,80	87		S	1	Cloudy.
	51	2	0	50	58	29,70	78		N	1	Cloudy.
19	38	7	0	38	56	29,57	76		W	1	Cloudy.
	42	2	0	42	58	29,61	67		NW	1	Fair.
20	33	7	0	35	56	29,86	78		SW	1	Cloudy.
	44	2	0	41	57	29,81	77		W	1	Cloudy.
21	39	7	0	42	55	29,34	78	0,180	SW	2	Rain.
	44	2	0	44	56	29,41	70		NW	2	Cloudy.
22	33	7	0	34	53	29,47	71		WNW	2	Fine.
	41	2	0	41	56	29,54	66		NW	2	Fine.
23	30	7	0	30	51	29,78	68		NW	1	Fine.
	37	2	0	37	53	29,82	66		W	1	Hazy.
24	36	7	0	43	52	29,55	88		SW	1	Foggy.
	49	2	0	48	54	29,47	90		WNW	1	Cloudy.
25	37	7	0	39	52	29,71	77	0,200	W	1	Cloudy.
	47	2	0	46	54	29,68	72		W	1	Fair.
26	42	7	0	42	53	29,33	73		W	2	Fair.
	45	2	0	45	55	29,40	65		W	2	Fine.
27	34	7	0	36	52	29,18	75		E	1	Cloudy.
	36	2	0	36	53	28,83	89		E	1	Snow.
28	33	7	0	34	50	29,29	85	0,385	NW	2	Fair.
	37	2	0	37	53	29,47	81		NW	1	Fine.
29	26	7	0	26	50	29,61	83		WSW	1	Fine.
	31	2	0	29	50	29,58	84		SW	1	Foggy.
30	28	7	0	34	49	29,05	88	0,080	NE	2	Rain.
	36	2	0	36	51	29,00	85		NE	2	Snow.

METEOROLOGICAL JOURNAL
for December, 1801.

1801	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
Dec. 1	33	8	0	37	49	28,83	81	0,040	SSW	2	Cloudy.
	39	2	0	38	51	28.92	81		NW	1	Cloudy.
2	31	8	0	33	49	29,18	84	0,045	E	1	Rain.
	43	2	0	43	53	29.21	80		W	1	Fair.
3	32	8	0	33	49	29.58	83		W	1	Fine.
	39	2	0	39	53	29,64	77		WNW	1	Fine.
4	30	8	0	31	48	29,80	82		SW	1	Cloudy.
	40	2	0	40	52	29,64	80		ESE	2	Cloudy.
5	40	8	0	50	51	29.03	94	0,335	WSW	2	Cloudy.
	52	2	0	50	53	28.96	90		S	1	Rain.
6	42	8	0	42	51	28,95	83	0,140	WNW	1	Cloudy.
	46	2	0	45	54	29.17	76		W	1	Fair.
7	32	8	0	33	51	29,49	84		WSW	1	Cloudy.
	39	2	0	39	53	29,53	77		SW	1	Cloudy.
8	33	8	0	35	50	29,63	83		S	1	Cloudy.
	51	2	0	45	53	29,50	82		SE	1	Cloudy.
9	41	8	0	48	53	28,87	91	0,180	S	2	Cloudy.
	50	2	0	50	54	28,65	87		S	2	Rain. [Much wind this forenoon.
10	43	8	0	43	52	29,27	84	0,192	SW	2	Fair.
	48	2	0	47	55	29,40	72		WNW	2	Fine.
11	34	8	0	34	52	29,73	82		SW	1	Fair.
	44	2	0	44	54	29,65	78		SSW	1	Cloudy.
12	31	8	0	31	51	29,62	75		SW	2	Fine.
	38	2	0	38	54	29,57	70		W	2	Fair.
13	31	8	0	31	51	29,67	78		W	1	Fair.
	35	2	0	35	54	29,69	74		W	1	Fine.
14	27	8	0	28	50	29,61	76		NW	1	Fair.
	32	2	0	32	52	29,64	73		NW	1	Fair.
15	25	8	0	26	48	29,70	77		SW	1	Fair.
	35	2	0	35	52	29,57	77		SW	1	Hazy.
16	33	8	0	33	48	29,29	82		W	1	Cloudy.
	31	2	0	31	50	29,29	67		NNW	1	Fair.

METEOROLOGICAL JOURNAL
for December, 1801.

1801	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
Dec. 17	27	8	0	28	47	29,28	81		W	1	Snow.
	32	2	0	32	50	29,39	80		N	1	Cloudy.
18	24	8	0	25	46	29,66	83		W	1	Cloudy.
	31	2	0	31	49	29,68	81		SW	1	Fair.
19	25	8	0	25	45	30,00	80		NW	1	Cloudy.
	31	2	0	30	49	30,13	77		NW	1	Fair.
20	23	8	0	24	45	30,22	80		WNW	1	Cloudy.
	42	2	0	36	47	30,05	80		S	1	Cloudy.
21	38	8	0	42	46	29,70	94	0,102	WSW	1	Cloudy.
	43	2	0	43	49	29,70	87		SW	1	Fair.
22	35	8	0	36	48	29,85	91		NE	1	Cloudy.
	38	2	0	38	51	29,90	80		NE	1	Hazy.
23	32	8	0	41	48	29,63	88		S	2	Cloudy.
	47	2	0	47	50	29,24	91		S	2	Rain.
24	43	8	0	43	50	29,49	82	0,438	SW	1	Cloudy.
	45	2	0	45	52	29,51	79		SW	1	Cloudy.
25	43	8	0	46	52	29,31	93	0,618	E	1	Rain.
	49	2	0	49	55	29,21	94		SSE	1	Rain.
26	44	8	0	45	52	29,05	85	0,248	S	2	Rain. [Much wind last night.]
	48	2	0	48	54	29,04	78		S	2	Cloudy.
27	40	8	0	40	51	29,22	80	0,180	S	2	Fair.
	47	2	0	47	54	29,25	75		S	2	Cloudy.
28	36	8	0	36	51	29,66	83		S	1	Cloudy.
	42	2	0	42	53	29,68	81		S	1	Fair.
29	39	8	0	42	52	29,55	87		S	1	Cloudy.
	43	2	0	42	54	29,60	72		W	1	Cloudy.
30	35	8	0	36	52	29,32	80		W	1	Cloudy.
	39	2	0	39	53	29,25	75		NW	1	Cloudy.
31	30	8	0	31	50	29,76	77		NE	1	Fine.
	34	2	0	33	53	29,87	79		NE	2	Cloudy.

1801.	Six's Therm. without.			Thermometer without.			Thermometer within.			Barometer.*			Hygrometer.			Rain.
	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	
	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Inches.	Inches.	Inches.	Deg.	Deg.	Deg.	Inches.
January	54	24	41,1	52	25	42,0	58	49	53,7	30,21	29,30	29,84	90	66	78,3	1,226
February	57	25	40,4	56	27	40,7	60	47	53,1	30,26	29,16	29,77	85	62	73,9	0,544
March	59	31	46,1	59	31	46,4	61	52	56,4	30,43	29,12	29,87	84	62	73,5	1,108
April	65	30	47,7	65	32	48,9	62	52	57,6	30,38	29,38	30,04	78	50	65,9	0,376
May	71	39	57,0	71	44	56,7	64	57	60,0	30,16	29,44	29,82	86	52	66,2	1,509
June	80	43	60,9	80	50	62,3	69	61	63,7	30,20	29,76	30,04	80	53	64,7	0,791
July	79	47	63,0	79	52	63,9	70	61	65,9	30,12	29,38	29,77	94	57	69,6	3,524
August	79	51	65,4	79	54	66,1	72	65	68,2	30,34	29,57	30,06	90	55	68,7	1,569
September	73	46	61,3	73	47	61,3	66	58	62,8	30,30	29,38	29,89	92	57	73,7	1,264
October	66	34	53,2	65	34	53,2	64	56	60,6	30,34	29,04	29,83	90	63	75,1	1,474
November	60	26	42,0	60	26	42,5	62	49	54,8	30,33	28,83	29,71	95	61	79,6	3,294
December	52	23	37,5	50	24	38,1	55	45	50,9	30,22	28,65	29,48	94	70	81,2	2,518
Whole year			51,3			51,8			59,0			29,84			72,5	19,197

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* The quicksilver in the bason of the barometer, is 81 feet above the level of low water spring tides at Somerset-house.

PHILOSOPHICAL
TRANSACTIONS,
OF THE
ROYAL SOCIETY
OF
LONDON.

FOR THE YEAR MDCCCII.

PART II.

LONDON,

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CONTENTS.

PHILOSOPHICAL TRANSACTIONS.

VIII. *Observations on the two lately discovered celestial Bodies.*

By William Herschel, *LL. D. F. R. S.*

Read May 6, 1802.

In my early account of the moving star discovered by Mr. Piazzi, I have already shewn that it is of a remarkably small size, deviating much from that of all the primary planets.*

It was not my intention to rest satisfied with an estimation of the diameter of this curious object, obtained by comparing it with the Georgian planet, and, having now been very successful in the application of the lucid disk micrometer, I shall relate the result of my investigations.

But the very interesting discovery of Dr. Olbers having introduced another moving star to our knowledge, I have extended my researches to the magnitude, and physical construction, of that also. Its very particular nature, which, from the observations I shall relate, appears to be rather cometary

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* By comparing its apparent disk with that of the Georgian planet, it was estimated, that the real diameter of this new star could not amount to $\frac{3}{5}$ths of that of our moon.

than planetary, will possibly throw also considerable light upon the circumstances belonging to the other celestial body; and, by that means, enable us to form some judgment of the nature of both the two last-discovered phenomena.

As the measures I have taken will oblige me to give a result which must appear extraordinary, it will be highly necessary to be particular in the circumstances of these measures, and to mention the condition and powers of the telescopes that were used to obtain them.

Magnitude of the new Stars.

April 1, 1802. Having placed a lucid disk at a considerable distance from the eye, but so that I might view it with perfect distinctness, I threw the image of Mr. PIAZZI'S star, seen in a 7-feet reflector, very near it, in order to have the projected picture of the star and the lucid disk side by side, that I might ascertain their comparative magnitudes. I soon perceived that the length of my garden would not allow me to remove the disk-micrometer, which must be placed at right angles to the telescope, far enough to make it appear no larger than the star; and, not having disks of a less diameter prepared, I placed the smallest I had, as far from me as the situation of the star would allow. Then, bringing its image again by the side of the disk, and viewing, at the same time, with one eye the magnified star, while the other eye saw the lucid disk, I perceived that Ceres, which is the name the discoverer has given to the star, was hardly more than one third of the diameter of the disk, and certainly less than one half of it.

This being repeated, and always appearing the same, we shall not under-rate the size of the star, by admitting its diameter to have been 45 hundredths of the lucid disk.

The power of the telescope, very precisely ascertained, by terrestrial geometrical measures properly reduced to the focus of the mirror on the stars, was 370,42. The distance of the lucid disk from the eye, was 2131 inches; and its diameter 3,4 inches. Hence we compute, that the disk was seen under an angle of 5′ 29″,09; and Ceres, when magnified 370 times, appearing, as we have shewn, 45 hundredths of that magnitude, its real diameter could not exceed 0″,40. Had this diameter amounted to as much as was formerly estimated, the power of 370 would have made it appear of 6′ 10″, which is more than the whole lucid disk.

This extraordinary result, raised in me a suspicion, that the power 370 of a 7-feet telescope, and its aperture of 6,3 inches, might not be sufficient to shew the planet's feeble light properly. I therefore adapted my 10-feet instrument to observations with lucid disks; which require a different arrangement of the head of the telescope and finder: I also made some small transparencies, to represent the object I intended to measure.

April 21. The night being pretty clear, though perhaps not quite so proper for delicate vision as I could have wished, I directed my 10-feet reflector, with a magnifying power of 516,54, also ascertained by geometrical terrestrial measures reduced to the focus of the instrument on celestial objects, to Mr. Piazzi's star, and compared it with a lucid disk, placed at 1486 inches from the eye, and of 1,4 inch in diameter. I varied the distance of the lucid disk many times; and fixed at last on the above-mentioned one, as the best I could find. There was, however, a haziness about the star, which resembled a faint coma; and this, it may be supposed, must render the measure less satisfactory than it would otherwise have been.

From these data we compute, that the disk appeared to the natural eye under an angle of 3′ 14″,33; while Ceres, when magnified $516\frac{1}{2}$ times, was seen by the other eye of an equal magnitude; and that consequently its real diameter, by measurement, was only 0″,38.

April 22. 11^h 38′, sidereal time. I used now a more perfect small mirror; the former one having been injured by long continued solar observations. This gave me the apparent diameters of the stars uncommonly well defined; to which, perhaps, the very favourable and undisturbed clearness of the atmosphere might contribute considerably.

With a magnifying power of 881,51, properly ascertained, like those which have been mentioned before, I viewed Dr. Olbers's star, and compared it with a lucid disk of 1,4 inch in diameter, placed at 1514 inches from the eye, measured, like the rest of the distances, with long deal rods. The star appeared to me so ill defined, that, ascribing it to the eye-glass, I thought it not adviseable to compare the object, as it then appeared, with a well defined lucid disk. Exchanging the glass for that which gives the telescope a magnifying power of $516\frac{1}{2}$, I found Pallas, as the discoverer wishes to have it called, better defined; and saw, when brought together, that it was considerably less in diameter than the lucid disk.

In order to produce an equality, I removed the disk to 1942 inches; and still found Pallas considerably less than the disk.

Before I changed the distance again, I wished to ascertain whether Ceres or Pallas would appear under the largest angle, especially as the air was now more pure than last night. On comparing the diameter of Ceres with that of the lucid disk, I found it certainly less than the disk. By proper attention, and continued examination, for at least an hour, I judged it to be nearly $\frac{3}{4}$ of the lucid disk.

Then, if we calculate as before, it appears by this observation, in which there is great reason to place confidence, that the angle under which this star appeared, was only 0″.22. For, a lucid disk of 1,4 inch diameter, at the distance of 1942 inches, would be seen under an angle of 2′ 28″,7; three quarters of which are 1′ 51″,52. This quantity, divided by the power 516,54, gives 0″,2159, or, as we have given it abridged, 0″,22.

13^h 7′. I removed the micrometer to the greatest convenient distance, namely, 2136 inches, and compared Dr. OLBERS's star, which, on account of its great altitude, I saw now in high perfection, with the lucid disk. It was, even at this distance, less than the diameter of the disk, in the proportion of 2 to 3.

When, by long continued attention, the appearance of Pallas was reduced to its smallest size, I judged it to bear no greater proportion to the diameter of the lucid disk of the micrometer, than as 1 to 2.

In consequence of these measures, it appears that the diameter of Pallas, according to the first of them, is 0″,17; and, according to the last, where the greatest possible distinctness was obtained, only 0″,13.

If it should appear almost incredible that these curious objects could give so small an image, had they been so much magnified as has been reported, I can say, that curiosity led me to throw the picture of Jupiter, given by the same telescope and magnifying power, on a wall at the distance of 1318 inches, of which it covered a space that measured 12 feet 11 inches. I do not mention this as a measure of Jupiter, for the wall was not perfectly at right angles to the telescope, on which account the projected image would be a little larger than it should have been, nor was I very attentive to other necessary minute circumstances, which would be required for an accurate measure; but we see at once, from the size of this picture, that the power of the telescope exerted itself to the full of what has been stated.

As we generally can judge best of comparative magnitudes, when the measures are, as it were, brought home to us; it will not be amiss to reduce them to miles. This, however, cannot be done with great precision, till we are more perfectly acquainted with the elements of the orbits of these stars. But, for our present purpose, it will be sufficiently accurate, if we admit their mean distances from the sun, as the most recent information at present states them; for Ceres 2,6024; and for Pallas 2,8. The geocentric longitudes and north latitudes, at the time of observation, were, for Ceres, about ♍ 20° 4′, 15° 20′; and for Pallas, ♍ 23° 40′, 17° 30′. With these data, I have calculated the distances of the stars from the earth at the time of observation, partly by the usual method, and, where the elements were wanting, by a graphical process, which is sufficiently accurate for our purpose. My computed distances were 1,634 for Ceres, and 1,8333 for Pallas; and, by them we find, that the diameter of Ceres, at the mean distance of the earth from the sun, would subtend an angle of 0″,35127; and that, consequently, its real diameter is 161,6 miles.

It also follows, that Pallas would be seen, at the same distance from the sun, under an angle of 0″,3199; and that its real diameter, if the largest measure be taken, is 147 miles; but, if we take the most distinct observation, which gives the smallest measure, the angle under which it would be seen from the sun, will be only 0″,2399; and its diameter, no more than 110⅓ miles.

Of Satellites.

After what has just now been shewn, with regard to the size of these new stars, there can be no great reason to expect that they should have any satellites. The little quantity of matter they contain, would hardly be adequate to the retention of a secondary body; but, as I have made many observations with a view to ascertain this point, it will not be amiss to relate them.

Feb. 25. 20-feet reflector. There is no small star near Ceres, that could be supposed to be a satellite.

Feb. 28. There is no small star within 3 or 4 minutes of Ceres, that might be taken for a satellite.

March 4. 9^h 45′, sidereal time. A very small star, south-preceding Ceres, may be a satellite. See Plate V. Fig. 1. where C is Ceres, S the supposed satellite, *a b c d e f*, are delineation stars, *c* and *d* are very small. S makes nearly a right angle with them; *e* is larger than either *c* or *d*. There is an extremely faint star *f*, between *e* and *d*.

14^h 16′. Ceres has left the supposed satellite behind.

March 5. There are two very small stars, which may be satellites; see Fig. 2. where they are marked, 1st S, 2d S. The rest, as before, are delineation stars.

March 6. The two supposed satellites of last night remain in their situation, Ceres having left them far behind.

10^h 16′. There is a very small star, like a satellite, about 75° south-following Ceres. See Fig. 3. It is in a line from C to *b* of last night.

11^{h} 20′. Ceres has advanced in its orbit; but has left the supposed satellite behind.

March 30. 9^{h} 35′. A supposed 1st satellite is directly following Ceres: it is extremely faint. A 2d supposed satellite is north-following. See Fig. 4. The supposed satellites are so small, that, with a 20-feet telescope, they require a power of 300 to be seen; and the planet should be hidden behind a thick wire, placed a little out of the middle of the field of view, which must be left open to look for the supposed satellites.

12^{h} 17′. Ceres has changed its place, and left both the supposed satellites behind.

March 31. 9^{h} 20′. There is a very small star, on the north-preceding side of Ceres, which may be a satellite.

11^{h} 50′. Ceres has moved forwards in its path; but the supposed satellite remains in its former situation. The nearest star is 20″ of time from Ceres; so that, within a circle of 40″ of time, there certainly is no satellite that can be seen with the space-penetrating power of this instrument.

It is evident, that when the motion of a celestial body is so considerable, we need never be long in doubt whether a small star be a satellite belonging to it, since a few hours must decide it.

May 1. 12^{h} 51′. I viewed Pallas with the 20-feet reflector, power 300; there was no star within 3′, that could be taken for a satellite.

Of the Colour of the new Stars.

Feb. 13. The colour of Ceres is ruddy, but not very deep.

April 21. Ceres is much more ruddy than Pallas.

April 22. Pallas is of a dusky whitish colour.

Of the Appearances of the new Stars, with regard to a Disk.

Feb. 7. Ceres, with a magnifying power of 516½, shews an ill defined planetary disk, hardly to be distinguished from the surrounding haziness.

Feb. 13. Ceres has a visible disk.

April 22. In viewing Pallas, I cannot, with the utmost attention, and under the most favourable present circumstances, perceive any sharp termination which might denote a disk; it is rather what I would call a nucleus.

April 28. In the finder, Pallas is less than Ceres. It is also rather less than when I first saw it.

Of the Appearances of the new Stars, with regard to an Atmosphere, or Coma.

April 21. I viewed Ceres for nearly an hour together. There was a haziness about it, resembling a faint coma, which was, however, easily to be distinguished from the body.

April 22. I see the disk of Ceres better defined, and smaller, than I did last night. There does not seem to be any coma; and I am inclined to ascribe the appearance of last night to a deception, as I now and then, with long attention, saw it without; at which times, it was always best defined, and smallest.

April 28. Ceres is surrounded with a strong haziness. Power 550.

With 516½, which is a better glass, the breadth of the coma beyond the disk may amount to the extent of a diameter of the disk, which is not very sharply defined. Were the whole coma and star taken together, they would be at least three times as large as my measure of the star. The coma is very dense near the nucleus; but loses itself pretty abruptly on the outside, though a gradual diminution is still very perceptible.

April 30. Ceres has a visible, but very small coma about it. This cannot be seen with low powers; as the whole of it together is not large enough, unless much magnified, to make up a visible quantity.

May 1. The diameter of the coma of Ceres, is about 5 times as large as the disk, or extends nearly 2 diameters beyond it.

13^h 19′. 20-feet reflector; power 477. The disk of Ceres is much better defined than that of Pallas. The coma about it is considerable, but not quite so extended as that of Pallas.

May 2. 13^h 20′. Ceres is better defined than I have generally seen it. Its disk is strongly marked; and, when I see it best, the haziness about it hardly exceeds that of the stars of an equal size.

Memorandum. This may be owing to a particular disposition of the atmosphere, which shews all the stars without twinkling, but not quite so bright as they appear at other times. Jupiter likewise has an extremely faint scattered light about it, which extends to nearly 4 or 5 degrees in diameter.

April 22. Pallas, with a power of 881½, appears to be very ill defined. The glass is not in fault; for, in the day time, I can read with it the smallest letters on a message card, fixed up at a great distance.

13^h 17′. The appearance of Pallas is cometary; the disk, if it has any, being ill defined. When I see it to the best advantage, it appears like a much compressed, extremely small, but ill defined, planetary nebula.

April 28. Pallas is very ill defined: no determined disk can be seen. The coma about it, or rather the coma itself, for no star appears within it, would certainly measure, at first sight, 4 or 5 times as much as it will do after it has been properly kept in view, in order to distinguish between the haziness which surrounds it, and that part which may be called the body.

May 1. Pallas has a very ill defined appearance; but the whole coma is compressed into a very small compass.

$13^h\ 5'$. 20-feet reflector; power 477. I see Pallas well, and perceive a very small disk, with a coma of some extent about it, the whole diameter of which may amount to 6 or 7 times that of the disk alone.

May 2. $13^h\ 0'$. 10-feet reflector. A star of exactly the same size, in the finder, with Pallas, viewed with $516\frac{1}{2}$, has a different appearance. In the centre of it is a round lucid point, which is not visible in Pallas. The evening is uncommonly calm and beautiful. I see Pallas better defined than I have seen it before. The coma is contracted into a very narrow compass; so that perhaps it is little more than the common aberration of light of every small star. See the memorandum to the observation of Ceres, May 2.

On the Nature of the new Stars.

From the account which we have now before us, a very important question will arise, which is, What are these new stars, are they planets, or are they comets? And, before we can enter into a proper examination of the subject, it will be necessary to lay down some definition of the meaning we have hitherto affixed to the term planet. This cannot be difficult, since we have seven patterns to adjust our definition by. I should, for instance, say of planets,

1. They are celestial bodies, of a certain very considerable size.

2. They move in not very excentric ellipses round the sun.

3. The planes of their orbits do not deviate many degrees from the plane of the earth's orbit.

4. Their motion is direct.

5. They may have satellites, or rings.

6. They have an atmosphere of considerable extent, which however bears hardly any sensible proportion to their diameters.

7. Their orbits are at certain considerable distances from each other.

Now, if we may judge of these new stars by our first criterion, which is their size, we certainly cannot class them in the list of planets: for, to conclude from the measures I have taken, Mercury, which is the smallest, if divided, would make up more than 135 thousand such bodies as that of Pallas, in bulk.

In the second article, their motion, they agree perhaps sufficiently well.

The third, which relates to the situation of their orbits, seems again to point out a considerable difference. The geocentric latitude of Pallas, at present, is not less than between 17 and 18 degrees; and that of Ceres between 15 and 16; whereas, that of the planets does not amount to one half of that quantity. If bodies of this kind were to be admitted into the order of planets, we should be obliged to give up the zodiac; for, by extending it to them, should a few more of these stars be discovered, still farther and farther deviating from the path of the earth, which is not unlikely, we might soon be obliged to convert the whole firmament into zodiac; that is to say, we should have none left.

In the fourth article, which points out the direction of the motion, these stars agree with the planets.

With regard to the fifth, concerning satellites, it may not be easy to prove a negative; though even that, as far as it can be done, has been shewn. But the retention of a satellite in its orbit, it is well known, requires a proper mass of matter in the central body, which it is evident these stars do not contain.

The sixth article seems to exclude these stars from the condition of planets. The small comas which they shew, give them so far the resemblance of comets, that in this respect we should be rather inclined to rank them in that order, did other circumstances permit us to assent to this idea.

In the seventh article, they are again unlike planets; for it appears, that their orbits are too near each other to agree with the general harmony that takes place among the rest; perhaps one of them might be brought in, to fill up a seeming vacancy between Mars and Jupiter. There is a certain regularity in the arrangement of planetary orbits, which has been pointed out by a very intelligent astronomer, so long ago as the year 1772; but this, by the admission of the two new stars into the order of planets, would be completely overturned; whereas, if they are of a different species, it may still remain established.

As we have now sufficiently shewn that our new stars cannot be called planets, we proceed to compare them also with the other proposed species of celestial bodies, namely, comets. The criteria by which we have hitherto distinguished these from planets, may be enumerated as follows.

1. They are celestial bodies, generally of a very small size, though how far this may be limited, is yet unknown.

2. They move in very excentric ellipses, or apparently parabolic arches, round the sun.

3. The planes of their motion admit of the greatest variety in their situation.

4. The direction of their motion also is totally undetermined.

5. They have atmospheres of very great extent, which shew themselves in various forms of tails, coma, haziness, &c.

On casting our eye over these distinguishing marks, it appears, that in the first point, relating to size, our new stars agree sufficiently well; for the magnitude of comets is not only small, but very unlimited. Mr. PIGOTT's comet, for instance, of the year 1781, seemed to have some kind of nucleus; though its magnitude was so ill defined, that I probably over-rated it much, when, November 22, I guessed it might amount to 3 or 4″ in diameter. But, even this, considering its nearness to the earth, proves it to have been very small.

That of the year 1783, also discovered by Mr. PIGOTT, I saw to more advantage, in the meridian, with a 20-feet reflector. It had a small nucleus, which, November 29, was coarsely estimated to be of perhaps 3″ diameter. In all my other pretty numerous observations of comets, it is expressly remarked, that they had none that could be seen. Besides, what I have called a nucleus, would still be far from what I now should have measured as a disk; to constitute which, a more determined outline is required.

In the second article, their motions differ much from that of comets; for, so far as we have at present an account of the orbits of these new stars, they move in ellipses which are not very excentric.

Nor are the situations of the planes of their orbits so much unlike those of the planets, that we should think it necessary to bring them under the third article of comets, which leaves them quite unlimited.

In the fourth article, relating to the direction of their motion, these stars agree with planets, rather than with comets.

The fifth article, which refers to the atmosphere of comets, seems to point out these stars as belonging to that class; it will, however, on a more particular examination, appear that the difference is far too considerable to allow us to call them comets.

The following account of the size of the comas of the smallest comets I have observed, will shew that they are beyond comparison larger than those of our new stars.

Nov. 22, 1781. Mr. PIGOTT's comet had a coma of 5 or 6′ in diameter.

Nov. 29, 1783. Another of Mr. PIGOTT's comets had a coma of 8′ in diameter.

Dec. 22, 1788. My sister's comet had a coma of 5 or 6′ in diameter.

Jan. 9, 1790. Another of her comets was surrounded by haziness of 5 or 6′ in diameter.

Jan. 18, 1790. Mr. MECHAIN's comet had a coma of 5 or 6′ in diameter.

Nov. 7, 1795. My sister's comet had a coma of 5 or 6′ in diameter.

Sept. 8, 1799. Mr. STEPHEN LEE's comet had a coma of not less than 10′ in diameter, and also a small tail of 15′ in length.

From these observations, which give us the dimensions of the comas of the smallest comets that have been observed with good instruments, we conclude, that the comas of these new stars, which at most amount only to a few times the diameter of the bodies to which they belong, bear no resemblance to the comas of comets, which, even when smallest, exceed theirs above a hundred times. Not to mention the extensive atmospheres, and astonishing length of the tails, of some comets that have been observed, to which these new stars have nothing in the least similar.

Since, therefore, neither the appellation of planets, nor that of comets, can with any propriety of language be given to these two stars, we ought to distinguish them by a new name, denoting a species of celestial bodies hitherto unknown to us, but which the interesting discoveries of Mr. PIAZZI and Dr. OLBERS have brought to light.

With this intention, therefore, I have endeavoured to find out a leading feature in the character of these new stars; and, as planets are distinguished from the fixed stars by their visible change of situation in the zodiac, and comets by their remarkable comas, so the quality in which these objects differ considerably from the two former species, is that they resemble small stars so much as hardly to be distinguished from them, even by very good telescopes. It is owing to this very circumstance, that they have been so long concealed from our view. From this, their asteroidical appearance, if I may use that expression, therefore, I shall take my name, and call them *Asteroids;* reserving to myself, however, the liberty of changing that name, if another, more expressive of their nature, should occur. These bodies will hold a middle rank, between the two species that were known before; so that planets, asteroids, and comets, will in future comprehend all the primary celestial bodies that either remain with, or only occasionally visit, our solar system.

I shall now give a definition of our new astronomical term, which ought to be considerably extensive, that it may not only take in the asteroid Ceres, as well as the asteroid Pallas, but that any other asteroid which may hereafter be discovered, let its motion or situation be whatever it may, shall also be fully delineated by it. This will stand as follows.

Asteroids are celestial bodies, which move in orbits either of little or of considerable excentricity round the sun, the plane of which may be inclined to the ecliptic in any angle whatsoever. Their motion may be direct, or retrograde; and they may or may not have considerable atmospheres, very small comas, disks, or nuclei.

As I have given a definition which is sufficiently extensive to take in future discoveries, it may be proper to state the reasons we have for expecting that additional asteroids may probably be soon found out. From the appearance of Ceres and Pallas it is evident, that the discovery of asteroids requires a particular method of examining the heavens, which hitherto astronomers have not been in the habit of using. I have already made five reviews of the zodiac, without detecting any of these concealed objects. Had they been less resembling the small stars of the heavens, I must have discovered them. But the method which will now be put in practice, will completely obviate all difficulty arising from the asteroidical appearance of these objects; as their motion, and not their appearance, will in future be the mark to which the attention of observers will be directed.

A laudable zeal has induced a set of gentlemen on the

Continent, to form an association for the examination of the zodiac. I hope they will extend their attention, by degrees, to every part of the heavens; and that the honourable distinction which is justly due to the successful investigators of nature, will induce many to join in the meritorious pursuit. As the new method of observing the zodiac has already produced such interesting discoveries, we have reason to believe that a number of asteroids may remain concealed; for, how improbable it would be, that if there were but two, they should have been so near together as almost to force themselves to our notice. But a more extended consideration adds to the probability that many of them may soon be discovered. It is well known that the comas and tails of comets gradually increase in their approach to the sun, and contract again when they retire into the distant regions of space. Hence we have reason to expect, that when comets have been a considerable time in retirement, their comas may subside, if not intirely, at least sufficiently to make them assume the resemblance of stars; that is, to become asteroids, in which state we have a good chance to detect them. It is true that comets soon grow so faint, in retiring from their perihelia, that we lose sight of them; but, if their comas, which are generally of great extent, should be compressed into a space so small as the diameters of our two asteroids, we can hardly entertain a doubt but that they would again become visible with good telescopes. Now, should we see a comet in its aphelion, under the conditions here pointed out, and that there are many which may be in such situations, we have the greatest inducements to believe, it would be a favourable circumstance to lead us to a more perfect knowledge of the nature of comets and their orbits; for instance, the comet of the year 1770, which

Mr. Lexell has shewn to have moved in an elliptical orbit, such as would make the time of its periodical return only about 5½ years: if this should still remain in our system, which is however doubtful, we ought to look for it under the form of an asteroid.

If these considerations should be admitted, it might be objected, that asteroids were only comets in disguise; but, if we were to allow that comets, asteroids, and even planets, might possibly be the same sort of celestial bodies under different circumstances, the necessary distinction arising from such difference, would fully authorise us to call them by different names.

It is to be hoped that time will soon throw a greater light upon this subject; for which reason, it would be premature to add any other remarks, though many extensive views relating to the solar system might certainly be hinted at.

Additional Observations relating to the Appearances of the Asteroids Ceres and Pallas.

May 4, 12^h 40′. 10-feet reflector; power 516½. I compared Ceres with two fixed stars, which, in the finder, appeared to be of very nearly the same magnitude with the asteroid, and found that its coma exceeds their aberration but in a very small degree.

12^h 50′. 20-feet reflector; power 477. I viewed Ceres, in order to compare its appearance with regard to haziness, aberration, atmosphere, or coma, whatever we may call it, to the same phenomena of the fixed stars; and found that the coma of the asteroid did not much exceed that of the stars.

I also found, that even the fixed stars differ considerably in this respect among themselves. The smaller they are, the larger in proportion will the attendant haziness shew itself. A star that is scarcely perceptible, becomes a small nebulosity.

10-feet reflector. 13^h 10′. I compared the appearance of Pallas with two equal fixed stars; and found that the coma of this asteroid but very little exceeds the aberration of the stars.

14^h 5′, 20-feet reflector. I viewed Pallas; and, with a magnifying power of 477, its disk was visible. The coma of this asteroid is a little stronger than that which fixed stars of the same size generally have.

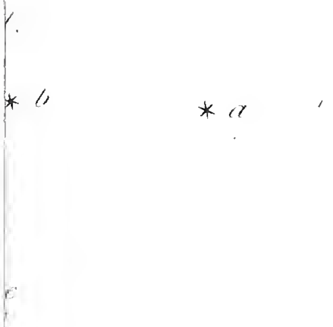

Fig. 3.

Fig. 4.

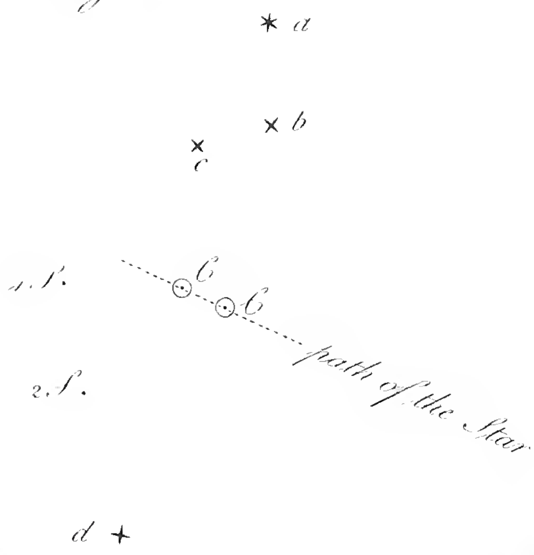

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Fig. 1.

Fig. 3.

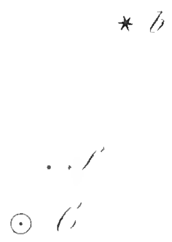
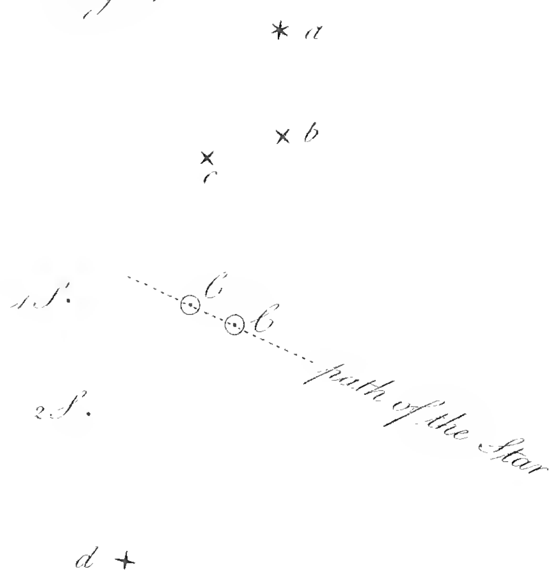


Fig. 2.

Fig. 4.



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IX. *Description of the Corundum Stone, and its Varieties, commonly known by the Names of Oriental Ruby, Sapphire,* &c.; *with Observations on some other mineral Substances. By the Count de* Bournon, *F. R. S.*

Read March 25, 1802.

When, in the year 1798, I presented to the Royal Society, in conjunction with Mr. Greville, a Paper on the Corundum Stone,* I gave some hints of an opinion which I, as well as Mr. Greville, had already formed, namely, that the said stone was absolutely of the same nature with those stones or gems which mineralogists, following the example of the jewellers, had hitherto distinguished by the epithet *oriental.* This opinion was founded upon circumstances which appeared to me perfectly satisfactory; but these circumstances had not yet been sufficiently examined, nor were they sufficiently striking, to

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* See Phil. Trans. for 1798. p. 428. My principal intention, in the Paper here referred to, was, to bring together the various observations which had been then made respecting the stone here treated of. The great number of specimens which have since been successively sent from different parts of the East Indies, have enabled me to form a more correct, and, in some respects, a different opinion of it. I therefore thought it would be of more advantage to science, instead of presenting to the Royal Society a supplement to my former Paper, to collect into one point of view, every information I could obtain upon the subject. I have consequently endeavoured, in the following Paper, to give, as far as I am able, a complete mineralogical history of this stone; my former account being, when compared with this, a very imperfect one.

obviate every possible objection; and, consequently, my opinion was not yet in a state fit to be presented to the Royal Society, as an established truth. Since that time, I have never lost sight of this object, nor have I neglected any means in my power, which could conduce to the end I had in view; and I may say, that my success has far surpassed my expectations. The specimens of corundum that have been lately sent from India, joined to the very considerable collection of oriental gems, in their perfect crystalline forms, which I have been able to procure, have afforded me the most satisfactory demonstration that a mineralogist can wish for; and nothing was now wanting to fix, in a complete and decisive manner, the general opinion respecting this stone, except to give it that additional support which is furnished by chemical investigation. Mr. KLAPROTH indeed had already published an analysis of the corundum stone, and of the sapphire; but he had not submitted to the same scrutiny, the perfect red corundum or oriental ruby; it is possible also, that the specimens of corundum he made use of in his analysis, which had been taken from among the first specimens of this stone sent from India, were not so pure as might have been wished, and that this impurity was the cause of the difference, (which however was very trifling,) between the result of their analysis and that of the sapphire. I therefore chose, from among the specimens of corundum which had been sent from China, from the kingdom of Ava, from the Carnatic, and from the coast of Malabar, such pieces as appeared to me the most pure; and, after having added to them a quantity of oriental rubies and sapphires, sufficient for many repeated analyses, I requested Mr. CHENEVIX, whose chemical labours are so useful to mineralogy, by his constant application of them to that science, to have the kindness to join with me in the investigation I had undertaken. The Royal Society will perceive, in the detail given by Mr. CHENEVIX himself, of the analyses which he has made, not only of the different varieties of corundum, but also of the substances which accompany this stone in its matrix, how very satisfactory to science are the results of those analyses; insomuch, that I can now offer to the Society, as one of the best established truths, what, in the year 1798, I mentioned merely as a suspicion which had great probability in its favour; and can also, in consequence of the particular study I have made of all the varieties of stones that I have here joined together, under the general denomination of corundum, present to the Society a collection of facts, for the most part unknown, which, altogether, may be considered as forming a mineralogical history of this substance.

Although the epithet oriental has been for a long time used by the lapidaries, to express, in gems or precious stones, a degree of hardness superior to that of other stones, (the diamond excepted,) which made them capable of taking a more brilliant polish; and although, following the example of the lapidaries, naturalists had employed the same term by way of distinguishing them, there still remained a great uncertainty, respecting the nature of the analogy which really existed between the various stones to which the above epithet was applied.

The nomenclature here spoken of was not, at its origin, the result of any mineralogical knowledge; in consequence of which, a number of stones, of a totally different nature, were united together, for no other reason but because, among those of the same colour, some were found to be of a much superior degree of hardness to others; and, as those which were the hardest most commonly came from the East Indies, all hard gems were called oriental, as a general mark of discrimination. The chief distinguishing character of gems was then derived from their colour, which had caused them to be denominated sapphire, ruby, amethyst, topaz, emerald, chrysolite, &c. and it was thought sufficient to add to these names the epithet oriental, to distinguish those among them whose hardness was superior to that of the others.

ROME' DE LISLE was the first mineralogist who threw a gleam of light, into the obscurity which existed in this confused assemblage of stones. His classification of gems, although it had not yet attained the degree of perfection to which the science of crystallography (of which he had just laid the foundation) may hereafter carry it, was undoubtedly one of the greatest steps mineralogy had made, at the time when the second edition of his work, upon this new character of stones, was published. After having fixed, according to their different characters, and particularly according to that which was derived from their crystalline forms, the place which each of the species composing this particular class of lithology ought to occupy, he placed at the head of them, under the title of *oriental ruby*, all those stones which, being possessed of a degree of hardness superior to that of all others, (except the diamond,) admitted a more brilliant polish, and appeared under the form of a hexaedral pyramid, or of two, joined base to base, the solid angle of whose summit, taken upon two of the opposite faces, varied, according to him, from 20° to 30°. He added also, that this stone presented all sorts of colours, either separately, or united together in the same stone. Nearly at the same time, Mr. Werner, following the system his genius had just then formed in mineralogy, was conducted to exactly the same results.

The very small number of perfectly defined crystals of this stone which existed in the cabinets of Europe, (they being much more rich in cut and polished specimens,) did not permit either of the above-mentioned mineralogists to obtain a clear idea of the whole of its characters, so as to enable him to give a proper description of it. Romé de Lisle, indeed, may be said to have made a step backwards, by excluding from the number of its crystalline forms, the rhomboid, which, in the first edition of his Crystallography, he had assigned to it, on account of a crystal of that form, which was among the stones preserved in the *Garde Meuble* of the King of France. This stone, which was of a blue colour inclining to purple, and of a very considerable size, (since it weighed no less than 132 carats,) had been polished; a circumstance which had necessarily altered its form in some measure, although there is reason to believe that it had been polished only upon its natural surfaces. Romé de Lisle, however, who had, merely for the above reason, excluded the rhomboid from the forms of the sapphire, being induced afterwards to recur to his former opinion, made another mistake, by assigning to this substance, the rhomboid of sulphate of iron or martial vitriol, (the measures of which are very nearly from 82° to 98°,) as that which properly belonged to it.

Our mineralogical knowledge with respect to corundum, was therefore very little advanced, when we became acquainted with that which was sent from India. Mr. Greville, in the Paper to which I have already referred, has given a very interesting and instructive account, not only respecting the introduction of this stone into Europe, but also respecting the information which, in consequence of his repeated inquiries, he had been able to obtain with regard to its local situation; and it is chiefly to him that we are indebted, for nearly all the specimens of this stone which exist in the various collections, as well as for the attention which has been paid to it.

From the moment when this stone became known, the opinions which were formed, respecting the place it ought to occupy in mineralogy, were very various; indeed, it was natural they should be so, with regard to a stone which, as yet, was only known by means of a few specimens, (by no means sufficiently numerous to supply every collection,) and whose local situation, as well as every thing else relating to it, was totally unknown. It has suffered, in this respect, the fate usually attendant on things so circumstanced; yet, whatever erroneous notions have hitherto been entertained respecting it, it has at last, I trust, found the place assigned to it by nature and truth.

The progress of chemistry, with respect to this stone, has not been more certain than that of mineralogy. It was first placed among those substances which were considered as composed of new earths; afterwards it was classed among those which were found by analysis to be chiefly, and indeed almost exclusively, composed of argill. This was already a great step towards the knowledge of its real nature; since it was thereby placed, if not by the side of, at least at a very inconsiderable distance from, the oriental gems, then known chiefly by the name of sapphire.

It is, in fact, among those gems or stones, now known by the names of sapphire, oriental ruby, &c. that corundum ought to be placed; but the progress by which we have arrived at this degree of knowledge was necessarily very slow, and was impeded by continual obstacles: for the scarcity and smallness of the crystals of corundum, and the impression naturally made upon our minds by the various appearances it exhibited to us, were by no means likely to lead us to form a true judgment respecting it. So that Mr. Werner, whose great and acknowledged talents have justly caused his opinion to be considered, nearly throughout all Germany, as of the highest importance in all mineralogical decisions, has hitherto continued to place corundum between pitchstone and felspar; consequently, he has removed it to a considerable distance from the sapphire, since there exists, according to his classification, nearly thirty intermediate substances.

Crystallography also offers some difficulties with respect to this stone; and these difficulties are only to be guarded against by a very particular study of it, and especially by an accurate examination of all its varieties, as objects of comparison. The Abbé Hauy, to whose great knowledge of crystallography all Europe is eager to do justice, although he gave some indications that he began to waver in his opinion, did not think there were reasons sufficiently strong to adopt that which I had, without satisfactory evidence, advanced in 1798; and has continued to separate the corundum from the sapphire, giving to the latter the name of *Telesie*. In the new System of Mineralogy, which the Abbé Hauy has just published, he places corundum immediately after felspar, and before ceylonite, the name of which he has changed into *Pleonaste*. One cannot help being astonished that the very great hardness of this stone, as well as its great gravity, did not lead him to place it nearer those stones with which, from their possessing those two qualities, it seemed to have some analogy. Perhaps he was not in possession of specimens of sapphire, or of oriental ruby, or of corundum, sufficiently characterised to serve as objects of comparison; and I cannot help expressing great regret, that the crystals of corundum which were sent to him by Mr. GREVILLE, selected by myself from his superb collection, and to which I had the pleasure of adding an almost equal number from my own, were not sufficient to carry conviction to Mr. HAUY's mind; as it would have given me great satisfaction to find that my observations, upon this interesting substance, perfectly coincided with his. The opinion of a naturalist so justly celebrated as Mr. HAUY, will naturally have great weight in the minds of those who pursue the study of mineralogy; for which reason, after giving a particular description of corundum, comprehending all the characters which are peculiar to it, I shall endeavour to remove every objection which this mineralogist still thinks it right to offer, against its union with the sapphire, oriental ruby, &c.

The substance here treated of, has hitherto presented itself to our notice under two appearances, which differ so much from each other, in the greater number of those characters which most forcibly affect our senses, particularly those which concern the organ of sight, that we cannot be much surprised to find that mineralogists feel some reluctance, at the idea of uniting together substances which appear so very dissimilar.

Under one of these appearances, in which it is known by the name of corundum, this substance presents itself either in fragments, or in crystals of a pretty large size; sometimes, indeed, of a very considerable one. The surface of these crystals is generally dull and rough; their texture, which is very much lamellated, is shown to be so by their fracture, which is obtained without much difficulty, as the adherence of their crystalline laminæ to each other is not very strong, and is easily overcome; and the crystal or fragment may always be brought to the rhomboid, its primitive form. Their colour, which is most commonly rather dull, is a whitish, greenish, and sometimes yellowish gray. Specimens of a purplish red, or of a blue colour, have always been extremely rare; indeed, a short time since, no such specimens were known, excepting a very few, preserved in the collection of Mr. Greville, and some small fragments he had given away; but the specimens which have been lately sent from the district of Ellor, have contributed to increase their number.

Under the other appearance, (in which this substance is known by the names of sapphire, ruby, &c.) it offers itself, on the contrary, in crystals which are generally of a very small size, and have a smooth and brilliant surface. Their transparency is often very great; and it seldom happens that they are not semitransparent, in a greater or less degree. They are more difficult to break in the direction of their crystalline laminæ; and this difficulty increases, in proportion to their purity and their brilliancy. Their colours are much more beautiful, more variegated, and more lively.

With respect to the name of this substance, as, in its most common state, it is known in India, (its native country,) by the name of corundum, and as that name has been generally adopted in Europe, I have thought proper to continue it, and shall distinguish, by the terms *perfect* and *imperfect*, the two different states in which it presents itself to our observation. Nothing, in my opinion, occasions greater obstacles to the progress of a science, than making a change in its nomenclature, especially when that change is made without a general agreement. For, by this means there exists no fixed basis; and, consequently, every one thinks he has a right to exercise an arbitrary power in this respect, and to reject the name given to a substance by those who first observed and described it, for the purpose of giving it one more suitable to his own ideas. And thus, at last, it becomes necessary, (in order that the labours of our predecessors may not be wholly useless,) to fill the new works on the subject with a tedious list of synonyms, which too often becomes in the end a mass of uncertainty, and a subject of everlasting discussion.

COLOUR.

Although the colour of stones, strictly speaking, may be considered as a very variable circumstance, and as one which can by no means be included among those fixed characters which determine the nature of the stone, it is nevertheless certain, that many stones seem disposed to assume some colours in preference to others; and, therefore, the colour of a stone, though an uncertain character, may sometimes serve as a secondary mark of distinction; particularly, if we are cautious not to draw any inferences from it, except in conjunction with other characters. As its chief use is, to fix the value of precious stones, and as, in those here treated of, it has served as a basis for the former classification of them, it becomes more necessary to give a minute description of it in this substance than in any other.

I have already said, that the colour of common corundum, (which I shall in future distinguish by the name of *imperfect* corundum,) has, in general, very little brilliancy; but, in proportion as the crystals announce, by their greater transparency, a greater degree of purity and perfection, their colour becomes more lively and more brilliant; this, however, seldom happens, except in crystals of a small size. The colour of these crystals is various, and seems to depend very much upon the place where they are found. In the Carnatic, the prevailing colour is a grayish white; which, however, very often approaches to a pale green, and sometimes to a yellowish cast. They are also found, but much more rarely, of a red, and of a blue colour; and, when they are of those colours, the red always inclines to the purple, and the blue is of that azure kind which is generally known by the name of sapphire blue. In the corundum of China, and in that of the kingdom of Ava, the colour is generally a green, more or less deep, with a dull appearance; or it is brown. The corundum of the coast of Malabar, appears of a reddish brown in those parts which are opaque; but, whenever there is, in any part of it, the smallest degree of transparency, the forementioned colour always appears to be accompanied by a tinge of purple.

In the perfect corundum, which is found in Pegu and in Ceylon, but which is now most commonly brought (when in its natural or unpolished state) from the last mentioned place, the colours are much more various, and more lively. The chief of these colours are, red, blue, and yellow. The red colour constitutes the stone known by the name of oriental ruby; but it seldom happens that this colour has not a small mixtureof blue, which gives it a tinge slightly inclining to purple. The blue colour is always that which is known by the name of azure blue; and the stone which possesses this colour is distinguished by the name of sapphire. The yellow colour is seldom pure, being in general more or less mixed with a reddish tint. The oriental gem of this colour is called the oriental topaz. From a duly proportioned mixture of the blue and the red, is produced the purple colour, which constitutes the oriental amethyst. Sometimes the red colour is predominant, at other times the blue; and, in the latter case, the stone possesses that beautiful purple colour which is so pleasing to the eye. Stones of this colour are among the most rare of those belonging to this substance. By the union of the blue colour with the yellow, is formed the green, which produces the oriental emerald; but there is usually mixed with this colour a small proportion of red, which gives to the green a brown and rather dull tinge. Sometimes however the yellow colour is predominant, which of course gives the green a yellowish cast, and then the stone becomes the oriental chrysolite. I have not yet seen any of the green stones, or oriental emeralds, in which the green colour was perfectly pure and brilliant, as it appears in the true emerald, called the peruvian one. In the mixtures of which I have just spoken, the colours are, in general, perfectly blended together; sometimes however they exist in a separate state, and so distinctly, in the same stone, that the mixed colour is only perceptible at the point where the different colours meet. At other times, these colours being only coarsely mixed, and not blended together, the stone presents the one or the other of them more distinctly, according to the position in which it is held.

TRANSPARENCY.

The crystals of corundum from the Carnatic, having their surface always rough, and being usually more or less impregnated with fine particles of the various substances which compose their matrix, very seldom possess any degree of transparency; but, when these crystals are broken, their fragments generally have a degree of semi-transparency, but most commonly a very slight one, unless the fragments happen to be very thin; even then, I have never found them perfectly transparent.

If such of these fragments as have the greatest degree of semi-transparency, are held between the eye and the light, there may be observed, within their substance, a great number of lines or fissures, which cross each other, and prevent the free passage of the light, the greater part of which is reflected. These fissures, which arise from there not being a complete adherence between all the parts of the crystalline laminæ, are the principal cause of the slight degree of transparency commonly met with in the kind of corundum here spoken of; which kind may truly be said not to have attained, in its crystallization, all the perfection it is capable of acquiring, and which may be observed in the perfect corundum of Ceylon.

I think it also right to observe, that the corundum of the Carnatic, when of a red or a blue colour, has always a greater degree of transparency, and is more pure, than that which is of any other colour; and, in these respects, the corundum of a blue colour is much superior to that which is red.

In the imperfect corundum of China and of Malabar, although the surface of the crystals is also generally rough, yet, as they are less impregnated with foreign substances, it is not uncommon to observe in them a greater or less degree of transparency at their edges. Some crystals have, indeed, been sent to us from China, (very small ones, I confess, but very perfect,) which possessed a degree of transparency very little inferior to that of the perfect corundum of Ceylon. The terminal faces of the crystals from the two last mentioned places, are very frequently what is called *chatoyant;* a property of which I shall hereafter speak more particularly.

The perfect corundum of Ceylon, whatever may be its colour, always has a greater or less degree of semi-transparency; and very often is perfectly transparent. Sometimes, indeed, the crossed fissures already spoken of, as existing in the imperfect corundum, are also to be observed in the interior part of this; but, when that is the case, they are less strong, and less numerous. The crystals of the perfect corundum have a smooth and brilliant surface; and they show all the transparency their substance possesses, without its being necessary, as in the imperfect corundum, to break them for that purpose. In general, when they have an inferior degree of transparency, whatever their colour may be, their terminal surfaces possess the appearance called *chatoyant*, which, as I have already said, is very frequently observed in the corundum of China, and in that of the coast of Malabar.

In general, although the perfect corundum of a blue colour, or sapphire, has exactly the same characters as that which is of a different colour, it appears to me certain, if I may judge from the great number of specimens I have seen, that it more commonly possesses a perfect transparency, than that which is of any other colour. I have already made a similar observation, in speaking of the imperfect corundum of the Carnatic. To this circumstance must be attributed, the superior value of an oriental ruby, if without defect and of a certain size, when compared with that of a sapphire of equal size and equally perfect. To the same cause must also be ascribed, the scarcity of fragments of sapphires, in comparison with those of rubies, in the sand of Ceylon which has passed through the hands of the lapidaries; the fragments of the former being usually more transparent, they are selected from it, as more worthy to be cut and polished.

HARDNESS.

Corundum is, next to the diamond, the hardest of all stones; but, with respect to this character, the degrees of intensity are various; and this variety depends principally upon the degree of purity, and the colour, of the stone.

When the imperfect corundum of the Carnatic is neither of a blue nor of a red colour, its hardness is less considerable, in proportion as its transparency is less, and its internal substance more full of those lines or fissures, which, as I have already said, are commonly observed in it. Such corundum may be scratched by that which is more transparent, though of the same colour. The latter, (supposing the degree of purity to be nearly equal,) may in its turn be scratched by that which is of a purplish red; and this last, by the corundum of a blue colour; which is the hardest of all those varieties of this stone that I have distinguished by the name of imperfect corundum. The hardness of the imperfect corundum of China, and of that from the coast of Malabar, appear to be equal. This hardness, which is rather inferior to that of the blue corundum of the Carnatic, is however somewhat greater than that of the other varieties.

The perfect corundum of Ceylon of a red colour, or oriental ruby, the hardness of which seems to be nearly the same as that of the imperfect blue corundum, is superior in hardness to all the other varieties of the latter kind. In the perfect corundum of other colours, the hardness is nearly the same as in the red; that which is of a blue colour, or sapphire, and only that, rather exceeds the others in hardness. We have just seen, that in the imperfect corundum also, the blue colour was accompanied by a degree of hardness greater than that of the other colours.

This substance emits pretty bright sparks, when struck with a piece of steel; but they are by no means proportioned to its hardness. If a piece of flint be struck with the same force, the sparks it produces are more numerous, as well as more bright; and it is possible to obtain sparks from flint, by a very slight blow, such as would not be sufficient to produce them from perfect corundum. It is also necessary, in order to obtain sparks from corundum, that the stone should have pretty sharp edges: if the part that is struck is obtuse, it is with some difficulty that any sparks can be obtained. The imperfect corundum, however, has, in this respect, some advantage over the perfect kind.

PHOSPHORESCENCE.

The substance here treated of becomes, like quartz, phosphorescent by collision; it requires only, in order to exhibit this property, a somewhat stronger degree of friction. The light which it emits has also less intensity; and does not appear to be accompanied by the smell which is peculiar to that obtained from quartz. A very remarkable circumstance may likewise be observed respecting this light. In all the varieties of this stone which are of a red colour, whether of the imperfect or of the perfect kind, or oriental ruby, the light here spoken of is of a very deep fire colour, similar to that of red hot iron, when heated to the degree known by the term *cherry red.* The sparks which are obtained from this stone by means of a piece of steel, have also some appearance of the above colour. These phenomena may perhaps serve to assist us in acquiring further knowledge respecting the cause of the phosphorescence of stones, of which we have hitherto had no very satisfactory explanation.

GRAVITY.

The specific gravity of corundum, in its different varieties, presents a series of interesting facts, particularly when they are compared with what has been already observed with respect to its different degrees of hardness. The great interest I have felt in the study of this substance, has caused me to take particular care in the examination of such of its properties as might lead to a perfect knowledge of it. I will now state the results of the observations with which the character now treated of has furnished me.

Of 33 specimens of the different varieties of imperfect corundum, the mean specific gravity was 3931. The lightest was 3875; and the heaviest 3981. Six of the 33 were above 3900. Eleven were between 3900 and 3931; and the remaining sixteen were above 3931, which, as I have already stated, was the mean proportion.

The mean specific gravity of the perfect red corundum, as determined by 20 specimens of oriental ruby, was 3977. The lightest of these was 3933. Five of the specimens were above 4000. One alone was as high as 4087; it was of a deep red colour, was perfectly transparent, and had been cut.

Sixteen different specimens of sapphire, gave a mean specific gravity of 4016. The lightest was 3907; it had scarcely any colour, and was nearly opaque. The heaviest was as high as 4161; this was of a beautiful deep blue colour, and was very transparent. Three of the 16 were above 4100.

The inferences which I think myself warranted to draw from the results of the above-mentioned trials, are,

1. That the specific gravity of the imperfect corundum is always less considerable than that of the perfect kind.

2. That this gravity varies according to the degree of perfection of the crystallization; and, consequently, according as the stone is more or less transparent.

3. That, in general, the corundum of a blue colour, whether of the perfect or the imperfect kind, is of a greater specific gravity than that of any other colour.

What is here stated respecting the specific gravity of the different kinds of corundum, is exactly analogous to what has been already mentioned respecting their various degrees of hardness.

CRYSTALLINE FORMS.

The primitive form of corundum, whatever may be its degree of perfection, is a rhomboid slightly acute; the obtuse angles of the planes measuring 94°, and the acute ones 86°. (See Plate VI. Fig. 1.) The description of the crystalline forms will be more easily and more clearly understood, by considering (as I shall constantly do in what follows) this rhomboid as being formed by the union of two triedral pyramids, united at their bases; the solid angle of the summit will then be formed by the meeting of three of the more acute angles; and its measure, taken upon one of its edges, and in the middle of the opposite face, will be very nearly 95° 30′.

Whatever the form of the crystals of this substance are, they may always, by dividing them, be ultimately brought to the rhomboid here spoken of; and, when they are broken, such of the fragments as are made in the direction of the laminæ, very often present the same rhomboid, in a very regular form. Indeed, it is the only method of obtaining this crystal, in the imperfect corundum; for, among all the crystals of that kind of corundum which have been sent from the East Indies, not one has yet presented its primitive form. With respect to the perfect corundum, I have been more fortunate; as, besides several fragments which exhibited this rhomboid very exactly, I have found four of these primitive crystals perfectly defined. One of them is a sapphire, and is in the collection of Sir John St. Aubyn; the three others are oriental rubies, and are in the collection of Mr. Greville.

First Modification. The summit of the pyramid, (as very frequently happens in calcareous spar, and in most of the stones which have a rhomboid for their primitive form,) is often replaced by a plane which is perpendicular to the axis. This plane then makes, with those of the rhomboid, an angle which differs very little from 122° 30′; and, as the extent of the plane is more or less considerable, it often causes great difference in the appearance of the crystals. Sometimes it does not descend so low upon the faces of the rhomboid, as to reach their small diagonal. (Fig. 2.) At other times, it exactly reaches to the diagonal. (Fig. 3.) And, very often, it descends more or less below it. (Fig. 4). This last variety is frequently met with in the perfect red corundum, or oriental ruby. I also know four instances of this form in the sapphire. The variety shown in Fig. 3 is rather scarce; but that of Fig. 2 is the most rare of the whole. I have likewise observed the two last, among some small crystals of imperfect corundum from China, which were pretty transparent.

Second Modification. At other times, the edges of the base of the primitive rhomboid are each of them replaced by a single plane, which is parallel to the axis, and which, when its extent is rather considerable, separates the two pyramids by a hexaedral prism with rhombic planes. I have never seen this modification with complete pyramids, as it is represented in Fig. 5, but I have often observed it combined with the preceding modification. This combination is not unfrequently met with in the oriental ruby, in which, the two varieties represented in Figs. 3 and 4 are found with a small beginning of a prism, as is shewn in Figs. 6 and 7. There are also in the collection of Sir JOHN ST. AUBYN, two crystals of sapphire, belonging to the same variety, one of which is tolerably regular in its form; but it is much more common to find these crystals with prisms of rather greater length, as is represented in Figs. 8 and 9. In Mr. GREVILLE'S collection also, there is contained a crystal of a pretty large size, and very perfect, in which the plane that has replaced the solid angle of the summit of the pyramid is very small, as in Fig. 10. All these varieties, but particularly those represented in Figs. 8 and 9, are likewise found among the small transparent crystals of imperfect corundum brought from China.

When the decrease produced by the plane which has replaced the solid angle of the summit of the rhomboid, has begun to take place nearly at the same time with, or even previous to, that which gives rise to the planes which replace the edges of the base, (as is indicated by the length of the sides of the prism,) it often happens that there remains no trace of the planes of the primitive rhomboid: the crystal is then a regular hexaedral prism. (Fig. 11.) This variety, which is very common in the perfect corundum of a red or of a blue colour, is also common in the imperfect kind; it is indeed, in certain districts, particularly in the Carnatic, almost the only form that is met with. In all these crystals, the prism here spoken of differs considerably in its length; sometimes it is very much elongated; at other times it is very short, as is represented in Fig. 12.

Third Modification. The primitive rhomboid is frequently observed to have undergone, in its crystalline laminæ, a decrease at those flat angles which rest upon the common base. This decrease occasions, in each of the pyramids of the rhomboid, six new planes equally inclined, which thereby render the pyramids enneaedral, (as is seen in Fig. 20,) and which, when this modification is perfect, (that is to say, when the planes belonging to it have destroyed every trace of the primitive rhomboid,) change the crystal into a dodecaedron, formed by the union, base to base, of two hexaedral pyramids with isosceles triangular faces, as in Fig. 13. At present, I shall only take notice of the pyramidal form of these crystals, without paying any attention to the inclination of the faces of the pyramids; for we shall see, at the end of this modification, that the decrease which occasions it is subject to considerable variation, changing, at the same time, the inclination the faces of the pyramids have to each other.

It very rarely happens that we find this dodecaedron perfectly complete, that is to say, with each of its pyramids terminating in a single point, by the exact meeting of all its faces. I know only one instance of this form, which I met with in a small sapphire, that I have placed in the collection of Mr. GREVILLE. There are indeed two specimens, nearly similar to the above, in the collection of Sir JOHN ST. AUBYN; but two of the opposite faces of their pyramids have increased to a greater degree than the others, which renders them cuneiform.

It is much more common to find the crystals of this modification combined with the first, and consequently having the solid angle of their summits replaced by a plane. Sometimes this new plane is very small, as is shown in Fig. 14. At other times, it is more considerable, as in Fig. 15. The above varieties are less common in the red perfect corundum, or oriental ruby, than in the blue perfect corundum, or sapphire, of which it is the most usual crystalline form, and in which, the plane that has replaced the summits of the pyramids is frequently very small. These varieties are likewise often found among the crystals of imperfect corundum of China; but it is very rare, on account of the irregularity of their surface, to meet with them perfectly defined. They are met with in a much more perfect state, among the crystals from the coast of Malabar; some of these indeed are so perfect, that, were it not for their reddish colour, they would certainly be taken for very beautiful sapphires. One of these crystals, which is in Mr. GREVILLE's collection, is more than an inch in length. Another, which is cuneiform, and has one of its pyramids broken, is above two inches long. In the crystals of imperfect corundum from the Carnatic, I have never met with any thing more than very slight traces or elements of this pyramidal form.

There frequently remain upon the crystals belonging to these varieties, particularly when the terminal faces are of a pretty considerable size, more or less evident traces of the planes of the primitive rhomboid; as appears by small isosceles triangular planes, of greater or less extent, situated upon three of the alternate solid angles, formed by the meeting of the terminal faces with those of the pyramid. (Fig. 16.)

It very often happens, in this modification, that the plane which has replaced the solid angle of the summit, acquires a more considerable increase in one of the pyramids than in the other; and indeed, most commonly, this increase is such as to cause the pyramid entirely to disappear. The crystal then becomes a simple hexaedral pyramid, which is either complete, as in Fig. 17, Plate VII. (but this very rarely happens,) or has its summit more or less replaced. (Fig. 18, A.) This variety, which is very common in the crystals of perfect corundum, is also frequently met with in those of the imperfect corundum from China; and it is very usual to see, upon the solid angles of its terminal faces, small isosceles triangles, which are occasioned by the preservation of some parts of the planes of the primitive rhomboid; (Fig. 19.) but they are seldom so regular in their form as they are represented in the figure.

I have often seen small crystals of oriental ruby that exhibited a very pretty variety, as they showed, at the same time, the primitive rhomboid with its summit strongly replaced, and the incipient change to the form of the hexaedral pyramid which constitutes this third modification: this variety is represented in Fig. 20. There are, in Mr. GREVILLE's collection, two very perfect crystals of this form.

The second modification, that in which the pyramids of the primitive rhomboid are separated by an intermediate hexaedral prism, is often combined with the abovementioned union of the first and third modifications. There exists, for example, in Mr. GREVILLE's collection, an oriental ruby, which exhibits the variety shown in Fig. 20, with the rudiments of an intermediate prism, as is seen in Fig. 21. This variety is also sometimes found among the small transparent crystals of imperfect corundum from China.

In four other crystals, also in Mr. GREVILLE's collection, the prism is very much elongated; and the plane which has replaced the solid angle of the summit of the pyramid is much more extended, as in Fig. 22. These crystals, which are oriental rubies, are in perfect preservation at their two extremities.

There is besides, in the same collection, another crystal, also an oriental ruby, which differs from the preceding, in having no traces left of the planes of the primitive rhomboid. The crystal, consequently, appears to be a regular hexaedral prism, with the edges of its terminal faces bevelled. (Fig. 23.)

In five others, the pyramid has made more progress; and, in all of them are to be seen, on their terminal faces, some slight traces of the primitive rhomboid. (Fig. 24.)

Lastly, in one other specimen, the pyramid is nearly complete, as in Fig. 25. I also know two sapphires, which exhibit an intermediate variety, between the two last-mentioned forms.

One of the most striking characters of corundum is, the great variety exhibited by this pyramidal modification, in the inclination of the faces of the pyramids to the axis of the crystal, and, consequently, in the more or less rapid decrease that has taken place in the crystalline laminæ, at the plane angles situated on the common base of the two pyramids which compose the primitive rhomboid. Among the crystals of imperfect corundum, from the different districts in which this substance has been hitherto found, which form part of Mr. GREVILLE's collection, and are sufficiently perfect to admit of being measured with accuracy, there is one, of which the solid angle at the summit, taken in the middle of two of the opposite pyramidal faces, is 50°; two of 40°; two of 35°; nine of 24°; and seven of 12°. Among the pyramidal crystals of oriental ruby are, one of 50°; one of 40°; four of 30°; one of 24°; and four of 12°. In the sapphire there are, one of 50°; two of 40°; one of 35°; two of 30°; one of 24°; and two of 12°. If to these measures we add those of two sapphires, and of two oriental rubies, in the collection of Sir JOHN ST. AUBYN, we shall also have 58° and 20°; and we may consequently state, from our present knowledge respecting this substance, that it admits no less than eight different decrements of the laminæ, at the same angle of the base; each of which produces a pyramidal modification. And the measure of the solid angle of their summits, (considering the pyramids as complete, and supposing at the same time that the very great care I have taken has prevented me from committing any error,) are 58°, 50°, 40°, 35°, 30°, 24°, 20°, and 12°.*

This difference in the inclination of the faces of the pyramids, in the corundum of a pyramidal form, often appears in a very striking manner in the same crystal. I have frequently met with oriental rubies, and also with sapphires, in which the faces of the pyramids, after having for some time preserved a certain degree of inclination, evidently appeared to have changed it, in

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* In Figs. 18 A, 18 B, and 18 C, are represented this simple pyramidal modification, having 58°, 35°, and 12°, for the measures of the solid angle of the summit of the pyramid: from these figures, it will be easy to form an idea of the appearance of those crystals which have the other measures above enumerated.

order to assume another; this change caused the crystal to terminate by a pyramid less sharp; and, in many instances, it was evident that it had happened several times successively. These variations do not always take place in a regular order in the same crystal; for it very often happens, that some of the faces have undergone two, three, or even four changes of inclination, while others have not undergone so many; and sometimes, indeed, have not undergone any at all. I have seen some of these crystals, of which the irregularity was such that, upon some of the faces, the degree of inclination was changed from a greater to a less; a circumstance which necessarily formed a depressed angle, and thereby produced a very irregular and even deformed shape, in the crystal itself. Among the very small number of crystals from the Carnatic which shew any disposition to assume the pyramidal form, I particularly observed one, in which this irregularity in the mode of decrease is very remarkable. This crystal, on three of its adjacent sides, appears to be a regular hexaedral prism; but, from nearly the middle of two others, also adjacent, it becomes pyramidal, and of that modification in which the solid angle of the summit is of 50°; and, from about one-third of the remaining side, it also assumes a pyramidal inclination, but of that modification in which the solid angle of the summit is of 40°. This crystal, which is represented in Fig. 26, is preserved in Mr. GREVILLE's collection. These pyramidal modifications also very frequently demonstrate, by the great number of transverse striæ which are on their faces, and which sometimes resemble the steps of a staircase, the irregularity with which their decrements have taken place.

Fourth Modification. The primitive rhomboid sometimes undergoes, in those acute angles which contribute to the formation of the solid angle of the summit, a decrease much more rapid than that we have already mentioned, when speaking of the first modification. This decrease replaces the solid angle by three new planes; which planes, if they were to become of such extent as to cause the primitive faces of the rhomboid to disappear, would occasion a secondary obtuse rhomboid, that would have considerable analogy, in the measure of its angles, with that rhomboid of calcareous spar which is called *lenticular;* that is to say, the solid angle of its summit would measure about 139°; and the plane angles of its rhombs 114° and 66°. I have not yet met with this rhomboid perfectly formed; but it exists, or at least one of its halves, in a very well defined state, at the summit of a simple pyramid, eight or nine lines in height, the solid angle of which summit measures 12°; it is represented in Fig. 27. The great number of striæ, parallel to the small diagonals of the primitive rhombic planes, with which the faces of the secondary rhomboid are covered, prevent me from being perfectly certain respecting the accuracy of the measures I have just stated; but, if they are not strictly exact, they must at least be very nearly so. The crystal I have just described is from the coast of Malabar, and is in Mr. Greville's collection. The planes of the secondary rhomboid are slightly *chatoyant.*

Fifth Modification. Another mode of decrease, of a similar kind, but still more rapid, sometimes takes place at the same solid angle of the summit of the primitive rhomboid. The triedral pyramid which replaces this angle, is then much less elevated than in the preceding modification. When it is complete, that is to say, when there remains no trace of the planes of the primitive rhomboid, the crystal becomes changed into a new rhomboid, which is much more obtuse than the former one. (Fig. 28.) The rhombic planes have 117°, for the measure of their obtuse angles; and 63°, for the measure of their acute ones. The solid angle of the summit of the pyramid is very nearly 150° 30′; consequently, the angle formed by the meeting of the bases is about 29° 30′.*

There are, in Mr. GREVILLE'S collection, two oriental rubies which exhibit this rhomboid completely formed; its planes are deeply striated, in the direction of the decrease; a circumstance which is very common in all planes that are the result of a rapid decrease, or in which the edges of the laminæ last deposited, deviate considerably from the edges of those which were already formed.

There are also, in the same collection, two perfect hexaedral prisms of corundum from the Carnatic, in which this modification shows itself by small isosceles triangular planes, situated upon three of the alternate solid angles of each extremity. (Fig. 29.) These planes may easily be distinguished

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* After having, in this substance, met with a secondary rhomboid that exactly agrees with one of those belonging to calcareous spar, (although the planes which produce it are differently situated upon the primitive crystal,) it appeared to me very extraordinary to meet with a second, which had exactly the same proportions as another of the obtuse rhomboids of the abovementioned substance. In fact, there exists in calcareous spar, a rhomboid much more obtuse than that which ROMÉ DE LISLE named *lenticular*, (called *equiaxe* by the Abbé HAUY,) of which the measures are exactly the same as those which have just been assigned to the rhomboid of corundum; but there is the following difference between them, viz. in calcareous spar, this rhomboid is the result of a decrease along the edges of the pyramid belonging to the primitive rhomboid; whereas, in corundum, it is the result of a decrease at the angles which contribute to the formation of the solid angle of the summit. This modification of calcareous spar has not yet been described; but, indeed, the same thing may be said of many other modifications of that substance.

from those belonging to the primitive rhomboid: first, by their inclination, which is very different, as they make, at their meeting with the edges of the prism, an angle of 110°, whereas the others make an angle of 147° 30′. Secondly, they are usually very deeply striated; a circumstance which rarely occurs in the others. Of the two crystals I have just described, one is nine lines in diameter, and six lines in height; it is also slightly transparent at the edges. The other is much smaller, more transparent, and of a purplish red colour, but rather pale. It is one of the purest specimens of imperfect corundum, particularly of that from the Carnatic, I have ever seen.

There are frequently observed, in the small prisms of imperfect corundum, some traces of the planes above described; they may in general be easily known by their striæ. I have also seen crystals in which were united, at the same time, traces of the two secondary rhomboids of the fourth and fifth modifications, in the manner represented in Fig. 30.

Sixth Modification. There also appears to exist, in this substance, a third rhomboid, which is much more obtuse than either of the two preceding ones; at least it is only to such a modification that I can refer several crystals, both prismatic and pyramidal, of imperfect corundum, which made part of a parcel lately sent to Mr. GREVILLE, from the district of Ellore, in the northern part of the government of Madras. Among these crystals are many hexaedral prisms, of a perfectly regular form, which have their terminal faces inclined in a contrary direction, so as always to make, upon the edges of the prism on which they incline, angles of 100° and 80°. (Fig. 31. Plate VIII.) These terminal faces appear to me to belong to a very obtuse rhomboid, of which, the acute angles of the rhombic planes would be 60° 46′; the obtuse ones 119° 14′; and the solid angle of the summit 165°. The crystal I have just described, would then be nothing more than the prismatic modification, combined with that which occasions this rhomboid; at both extremities of which, one of the faces of each of the obtuse triedral pyramids, belonging to the new rhomboid, would have acquired (in a contrary direction with respect to its extremities) such an increase as would cause the other faces to disappear. These two faces, having now become the terminal ones of the hexaedral prism, would in fact make, with those edges of the prism on which they would incline, angles of 100° and 80°. This very obtuse rhomboid would be the result of a decrease analogous to the two preceding ones, but still more rapid. Many pyramidal crystals of this kind of corundum, present such inclined terminal faces; but with a difference, in the measure of their angles, conformable to the inclination of the edges of the pyramids.

Seventh Modification. The primitive rhomboid of this substance also undergoes sometimes, though very rarely, a decrease at those acute angles which rest upon the base; and this decrease is such, that it replaces each of the solid angles of this same base, by a plane which is parallel to the axis of the rhomboid. If this modification were complete, it would give rise to a regular hexaedral prism, which would differ from the prism of the second modification, in having its sides corresponding with the solid angles of the base of the rhomboid; whereas the sides of the other correspond with the edges of the said base. I know this modification only by a single crystal, which is in the collection of Mr. GREVILLE; in it is combined the modification here spoken of with the three first. This crystal, which is of perfect red corundum, or oriental ruby, is almost exactly similar to that represented in Fig. 22, and indeed only differs from it by the prism being dodecaedral, as in Fig. 32.

Eighth Modification. I am also acquainted with this modification only by a single crystal. This crystal, which is a sapphire of a beautiful deep blue colour, is likewise in Mr. GREVILLE's collection. Its form is a simple hexaedral pyramid, which is almost complete, and has 24° for the measure of the solid angle of its summit. Each of its six edges are replaced by a very narrow plane, which is equally inclined upon the two faces that are adjacent to it. This renders the pyramid dodecaedral, with broad and narrow faces alternately, as in Fig. 33. Three of these new planes appear to me to be occasioned by a decrease, which has taken place at the obtuse plane angles that rest upon the base of the rhomboid, but which differs from those which occasion pyramidal modifications, and is of such a nature that (the new planes to which it gives rise being in pairs, and on the same level,) each of the solid angles of the base is replaced only by a single plane. The three others appear to me to be caused by a decrease at the acute plane angles that rest upon the base; but this decrease differs from that of the seventh modification, in being more rapid, and in having the planes to which it gives rise inclined upon the axis of the crystal. The three latter planes have the following peculiarity, viz. their inclination is exactly equal to that of the three others; so that, if the two modifications which are united together in this crystal were complete and separate, they would produce two acute rhomboids, perfectly similar to each other.

FRACTURE AND TEXTURE.

I have already observed, that all the stones which compose the various kinds of this substance, to which I have given the general name of corundum, have a lamellated texture, in a direction parallel to the faces of a rhomboid of 96° and 84°; and also, that they break in a direction parallel to the said faces.

The blue variety of perfect corundum, or sapphire, follows the above law, as well as all the other varieties. It is true, however, as I have already had occasion to mention, that the ease with which the crystals of this substance may be divided, is very various; but observation shows, at the same time, that these variations are governed, in the first place, by the degree of force existing in the attraction of the molecules which compose the crystals, as well as by the perfect adhesion of the crystalline laminæ (composed of these molecules) at all points of their surface; two facts, the existence of which is shown by the difference in the degrees of hardness and transparency of this stone, and which appears to be very considerable. In the second place, the variations here spoken of seem also to depend very much upon the colour these stones possess; for, as I have already observed, they must be governed by the force of attraction, which, in my opinion, varies with the colour. This force appears to exist in the highest degree, in the perfect corundum of a blue colour, or sapphire; it being with great difficulty that this kind of corundum can be broken, in the direction of its laminæ, in such a manner that its fracture shall present that even surface, and that kind of gloss, which fractures made in the above direction generally exhibit. It may be broken with equal ease in any other direction; for instance, in a direction perpendicular to the axis of the crystal; but, in this last case, the fractures by no means possess such characters as might cause them to be taken for fractures made in the direction of the laminæ; they are always unequal, and partially conchoid. I will even confess, that I have not yet succeeded in breaking a sapphire, according to the direction of its laminæ, in a satisfactory manner. But that which art is not able to perform, is executed by nature: for, besides such sapphires as, upon their terminal faces, retain complete traces of the planes of the primitive rhomboid, I have frequently met with sapphires, both of the prismatic modification and the pyramidal one, in which there were, upon the said faces, one or more fractures, made exactly in the direction of the laminæ; and it was necessary to examine them with great attention, in order not to mistake them for true planes, representing those of the primitive rhomboid.* This kind of fracture is obtained with greater ease in the perfect red corundum, or oriental ruby; and still more easily, in the imperfect corundum. The latter presents, in this respect, a less degree of resistance, in proportion as it is less transparent, and has less colour. This character, however, is subject to great variation: there exist some specimens of this stone, in which such fractures as are here described may be made almost as easily as in calcareous spar; whereas, in others, they are obtained with much more difficulty. I have even seen some pieces which

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* I have placed several of these crystals in Mr. GREVILLE's collection, and also in that of Sir JOHN ST. AUBYN, and in that of Sir ABRAHAM HUME. The owners of these collections have confided to me the care and arrangement of them, with a degree of liberality which gives me every advantage that could be derived from the absolute possession of them, and consequently diminishes my regret for the loss of my own. I feel too sensibly these advantages, and many others resulting from their friendship and society, not to embrace with pleasure this opportunity of testifying my gratitude.

might be broken, with almost as much ease, in a direction contrary to that of the laminæ, as in the direction of the laminæ; but it most frequently happens, in this case, that the fracture, although made in the natural direction, has not the evenness such fractures usually have, but presents some irregularities, and likewise some conchoidal parts: this remark, however, applies only to such pieces as approach nearly to perfection, with respect to transparency.

There may frequently be observed, in these stones, a character which serves to confirm what I have said respecting the imperfection sometimes observed in their crystallization, which appears to me to arise principally from a want of absolute contact between all the parts of their crystalline laminæ. When some of the faces of the crystals correspond to those of the primitive rhomboid, whether these faces are natural ones or are produced by fracture, the edges of the crystalline laminæ are shown upon them, and sometimes very plainly, by lines which cross each other, in such a manner as to form rhombs of 96° and 84°. This character even becomes of great use in this substance, as it serves to distinguish, in fragments, (which are generally of hexaedral prisms, that being the most common form,) those faces which are occasioned by fracture, from those which correspond to the terminal faces of the prism. These last, also, frequently exhibit lines, which are likewise caused by the edges of the crystalline laminæ; but, as they extend to three only of the alternate angles of the terminal hexagonal face, they trace on it, by crossing each other, either equilateral triangles, or rhombs of 60° and 120°. Figs. 34, A, and 34, B, represent these two different appearances; the first upon the planes of the rhomboid; the second upon the terminal faces.

As it is by no means uncommon, in corundum, (in the same manner as is observed in the beryl,) to meet with elongated prisms, formed merely by the connection or contact of several prisms at their terminal faces, it frequently happens that these prisms, after being separated from each other, exhibit, upon the terminal faces which were in contact, a polish or lustre that might easily cause those faces to be taken for fractures, in a direction perpendicular to the axis. But this appearance is an illusion we must guard against: for, if we endeavour to make any fractures at the extremities of these crystals, they will take place, as usual, upon three of the alternate solid angles; and we shall find it impossible to succeed in making any fractures perpendicular to the axis, except such as are extremely irregular, and exhibit an appearance very different from that exhibited by natural ones. It sometimes happens also, that, by means of the above connection, as well as by some causes of compression, which must necessarily have been frequent with respect to crystals inclosed in their matrix, in the manner those of felspar are inclosed in granite or porphyry, that the terminal faces have varied from their natural position, and have assumed another, which inclines more or less upon the sides of the prism. We must, however, distinguish these accidental varieties, from those crystals in which such an inclination really belongs to the mode of crystallization, and which I have already described, in speaking of the sixth modification. In this latter case, the inclination of the terminal faces is constantly the same; whereas, in the accidental case here treated of, it varies considerably.

There exists also, in this substance, and even among the same crystals, (when hexaedral prisms,) not only of imperfect corundum but likewise of the perfect kind, of all colours, another accidental variety, which is particularly met with when their irregularity and their opacity announce a want of perfection in their crystallization. Sometimes the edges of the crystalline laminæ may be perceived upon their terminal faces; and, there being more or less distance between them, they exhibit very much the appearance of an irregularity, or a kind of disturbance, in those laminæ which seem to have been deposited upon these faces, and in a direction parallel to them. But, with a little attention, we may perceive that these laminæ, the edges of which are in the direction of three of the alternate solid angles of this extremity of the crystal, can only belong to the laminæ deposited upon the faces of the primitive rhomboid; and, we are very often able, at the same time, to discover their degree of inclination.

A third circumstance attending these crystals, and one which it is more difficult to explain, consists in the appearance of concentric hexagons, parallel to the hexagon formed by the exterior edges of the crystal. These hexagons may sometimes be observed upon the terminal faces, as is shewn in Fig. 35. Their edges have a degree of thickness very perceptible by the eye; and may besides be frequently distinguished from each other, by a difference in their transparency, and sometimes also by a greater or less intensity in their colour. There are preserved, in Mr. Greville's collection, amongst a pretty large number of crystals in which this circumstance has taken place, two crystals of imperfect corundum from the coast of Malabar, that exhibit it in a very striking manner. In the first of them, one only of these hexagons, placed at nearly an equal distance from the centre and the edges of the terminal face, is of a blue colour, while all the rest of this face is gray, slightly tinged with red, and *chatoyant*. (Fig. 36.) In the other, the last concentric hexagon alone, or that which at the same time forms the exterior part of the crystal, is (for the thickness of about half a line) of a blackish-brown colour, dull and opaque; while the rest of the terminal face (which likewise exhibits concentric hexagons) is of a gray colour, but has a silvery hue, because this part of the stone is *chatoyant*. (Fig. 37.) The above circumstance seems to announce a deposition of laminæ upon the sides of the hexaedral prism; nevertheless, if we attempt to break these crystals according to that direction, we find that it is absolutely impossible to succeed, in such a way as to obtain a fracture that has the appearance of being made in the natural joints of the stone; whereas, on the contrary, fractures may be made with sufficient ease, in a direction corresponding to the faces of the primitive rhomboid. Notwithstanding these concentric hexagons, there may be sometimes perceived, upon the same terminal faces of the prism, traces of the edges of the laminæ already mentioned; and the crystal then exhibits the appearance represented in Fig. 38. As the real direction of the laminæ (which is shown in these crystals by their natural fractures) indicates that the rhomboid of 96° and 84° is the primitive form of this substance, it seems necessarily to exclude the other direction, of the existence of which (as we have seen) there is some appearance, and which would give the hexaedral prism, as the form of the primitive crystal.

The above appearance, however, is certainly owing to a particular cause; but it seems to me, that the laws hitherto established in crystallography, are by no means capable of furnishing one that can account for it in a satisfactory manner. The only explanation of the circumstance which occurs to me, does not agree with the idea we have formed respecting those laws; but the circumstance itself may be perfectly explained by it. It is founded upon a supposition that the primitive rhomboid may have passed, very nearly at the time the crystallization began, to the form determined by the combination of the two modifications which produce the hexaedral prism, and that, in consequence of a law not yet acknowledged, the sides of the prism may have become, at the very moment of their formation, a new centre of attraction, for the regular deposition of a part of the crystalline molecules. This supposition, however, would require another, but which perhaps may be fairly considered as nothing more than a consequence of the former, namely, that the mutual attraction of the molecules situated upon these secondary faces, is more strong than that which exists in the same way between those upon the primitive ones. This stronger degree of attraction between the molecules on one of the faces of a crystal than between those of the other, is already admitted; so that it may rather be considered here, as giving rise to an additional observation, than as affording matter for discussion. I am perfectly sensible of, and make no scruple to allow, every objection that may be made against this explanation, to arrive at which, I have been obliged to make a supposition not yet admitted; but the fact itself exists, and seems naturally to lead to the explanation I have given. I offer it, however, merely as a hypothesis, which still requires the support of observation; and I shall only add, that it is not the first time that the study of crystals has led me to form such an idea.

With respect to the cause which, notwithstanding the abovementioned mode of crystallization, would still occasion the fracture to have the same direction as if the increase of the crystal had been produced by a deposition on the faces of the primitive rhomboid, it may, I think, be explained by supposing that, in this case, the elements of the crystallization might already be real, though small, secondary crystals, for instance, small hexaedral prisms; and that the fracture would then be nothing more than the result of the sum of all the partial fractures of each of them.*

PHENOMENA WITH RESPECT TO LIGHT.

The prismatic crystals of corundum, as well as the pyramidal ones, when their extremities are terminated by faces which are perpendicular to their axes, very frequently have those terminal faces *chatoyant.* This property is the natural effect of the

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* I had finished writing this Paper, when Mr. GREVILLE had the curiosity to cause one of the hexaedral prisms of imperfect corundum, from the coast of Malabar, the terminal faces of which exhibited the concentric hexagons above spoken of, to be cut transversely. This section shewed a very interesting fact, and one that adds some probability to what I have said respecting the cause which produces this phenomenon. One of the parts of this crystal (which crystal is sawed into three, and polished,) exhibits the appearance represented in Fig. 38, A. The whole substance of this segment is of a pale purplish-red colour; but there is, in its centre, a triangular spot, similar to that represented in the above figure, which indicates very clearly that the section was made below the summit of the primitive rhomboid, and perpendicularly to its axis. This spot is also of a purplish-red colour, but much more deep than the rest of the crystal, and therefore strikes the eye very forcibly. It is only to be perceived upon one of the terminal faces; the other terminal face does not show the smallest trace of it. There may, however, be perceived at its centre, a hexagonal plane, nearly as large as that represented surrounding the spot in Fig. 38, A; it is of a different colour from the other part of the substance of this segment, being of a dirty gray. The spot is also seen, but of a smaller size, upon the terminal face corresponding to the segment taken from the top of the preceding; but there are not any traces of it upon the other terminal face.

reflection of light, in the small intervals which remain between the small crystalline laminæ, in those parts where these laminæ are not in perfect contact; it is necessary, therefore, that the crystal, or fragment, which possesses this property, should be in the state most favourable to its developement. On this account, it must not be completely transparent; there being, in that case, too perfect a contact between the laminæ; so that the light, not meeting with any medium to reflect it, but being entirely refracted, cannot occasion any appearance of the property here spoken of. Neither must the crystal, or fragment, be quite opaque; it being necessary that the light, in order to undergo the reflection which produces this pleasing phenomenon, should at least be able to pass through the exterior laminæ of that part of the crystal against which it strikes. The above circumstances are, in fact, those which appear to take place with respect to corundum. The imperfect corundum of the Carnatic, the crystals of which are generally more or less opaque, show no trace of this property upon their terminal faces; whereas, it is frequently observed upon the terminal faces of the crystals of imperfect corundum from China, and also of that from the coast of Malabar, because those crystals generally possess a slight degree of semi-transparency. This character is still more common in the perfect corundum, whether sapphire or oriental ruby. There is not, however, the smallest appearance of it, when these stones possess the beautiful transparency belonging to them in their highest degree of perfection; whereas, on the contrary, it is frequently seen to take place in a very lively and brilliant manner, in such of the stones as have an inferior degree of transparency. It rarely happens, that the crystals of perfect corundum are prevented by opacity from exhibiting the property here treated of; but, as I have already said, the terminal faces which, by their position, replace the solid angles of the summit of the primitive rhomboid, are absolutely the only ones which can in any degree possess it: no appearance of it can be seen in any other part. This is not surprising; for, as the effect here spoken of proceeds from the reflection of light, in the spaces between the crystalline laminæ, the plane which may be considered as produced by a section which would expose the edges of all these laminæ, must necessarily be the most proper to occasion it. This effect also takes place when the crystals are broken, by chance, in a direction more or less approaching to that which is parallel to the abovementioned plane, notwithstanding the fracture then exhibits a very rugged appearance. It even happens sometimes, that this fracture is such that the edges of the laminæ protrude, in the manner observed in the fibres of wood when it is broken across the grain; yet the property here treated of is not less evident; and, in this last case, it is often very distinctly seen proceeding from between the laminæ.

To the above property must also be referred, that beautiful reflection of light, in the form of a star with six rays, which is frequently given, by cutting, to oriental rubies, sapphires, &c. and which causes those stones to be then called by the name of *star-stones*. The manner of cutting which brings the perfect corundum into this state is, most commonly, on the part of the lapidary, rather the result of chance, than the consequence of any determined theory respecting the cause of the effect he means to produce. Accordingly, in the greater number of the stones which have this property, the point from whence the starry reflection proceeds, instead of being in the middle of the stone, is observed to be situated in a part more or less near to its base; a circumstance which considerably diminishes the beauty of the star-stone. The reflection which produces this effect, arises from the same cause as that of which we have already spoken, and proceeds from the same part of the stone; consequently, when an oriental ruby, or a sapphire, which has the qualities necessary for the purpose, is intended by the lapidary to be formed into a star-stone, he ought to make his section pass below that part of the stone which he has found to correspond with the summit of the primitive rhomboid. As the kind of cutting most proper to produce this effect in the stone, is that rounded form which is called *en cabochon*, with as high an ellipsis as is possible, the lapidary ought, at the same time, to take great care that the summit of this ellipsis be situated exactly under the point which corresponds with the summit of the rhomboid; in that case, the light reflected in the interval of the laminæ upon the three edges of the primitive rhomboid, and upon the middle of its three faces, will trace upon the stone, a star, the six rays of which will include the circumference of the rounded part, or ellipsis. The same effect may also be made to take place on one of the solid angles of the base, but in a much less perfect manner.

I have met with many fragments of sapphires, as well as of oriental rubies, which naturally produced the effect here spoken of, in consequence of their having been broken, by chance, in a manner proper to occasion it; that is, they were broken, accidentally, in a direction contrary to that of the laminæ, and perpendicular to an axis passing through the two summits of the pyramid of the primitive rhomboid; after which, the fragment had been a little rounded by friction.

The imperfect corundum may likewise be cut in such a manner as to produce the starry reflection; but it is more rare than in the perfect kind, to meet with pieces which have all the qualities requisite for this purpose. There is, in Mr. GREVILLE's collection, a large piece of imperfect corundum, of a brown colour, which has been cut *en cabochon*, with the above-mentioned intention; but, the cutting not having been made in the proper direction, the starry reflection is exhibited in a very imperfect manner, as it proceeds from a point near the plane of the base of the stone. The effect produced, however, is sufficient to remove all doubts respecting the existence of the property here spoken of, in this kind of corundum.

CHARACTER AFFORDED BY ANALYSIS.

In order to complete the proofs I have already given, that all the stones which form the subject of this Paper are of one and the same nature, I shall borrow this last mentioned character from the analyses made by Mr. CHENEVIX, which will hereafter be described at length by that able chemist; and it may be observed, that few instances can be met with where the chemist and the mineralogist, after having jointly employed themselves in their different provinces, upon the same substance, have arrived at a more satisfactory and correspondent result.

According to Mr. CHENEVIX's analyses, the constituent parts of the various substances here treated of, are as follows.

IMPERFECT CORUNDUM.

	From the Carnatic.	From Malabar.	From China.	From Ava.
Silica	5,0	7,0	5,25	6,5
Alumina	91,0	86,5	86,50	87,0
Iron	1,5	4,0	6,50	4,5
Loss	2,5	2,5	1,75	2,0
	100,0	100,0	100,00	100,0.

PERFECT CORUNDUM.

	Blue, or sapphire.	Red, or oriental ruby.
Silica	5,25	7,0
Alumina	92,0	90,0
Iron	1,0	1,2
Loss	1,75	1,8
	100,00	100,0.

From what has been said it appears, that the analogy existing between the stones hitherto known by the names of corundum, sapphire, oriental ruby, oriental hyacinth, &c. is so strong and complete, as no longer to permit us to doubt that they ought all to be considered merely as varieties of the same substance, to which I have therefore given the general name of corundum.

In the learned work on mineralogy which the Abbé Hauy has just published, this celebrated naturalist says, that nearly at the same time I communicated to the Royal Society my first observations on this substance, he had himself observed the existence of corundum, among the crystals of different substances contained in the sand of Ceylon; having, as he says, seen therein some small hexaedral prisms, of a ruby red colour, and transparent, which, from the analogy that appeared to exist between their external characters and those peculiar to corundum, might very naturally be ranged with that substance. Some particular circumstances certainly prevented him from making the same observations respecting the pyramidal crystals, of the above colour, which are also found in that sand; and he consequently thought it right, (although he appears to have had some doubts upon the subject,) to continue to separate the sapphire from corundum, giving to the former the name of *telesie:* indeed he has placed them at a considerable distance from each other, the sapphire being the third species of his second class of stones, and the corundum the fourteenth. What he seems to consider as the strongest arguments in favour of this separation, are, the laminated texture so evident in all crystals of corundum, and the direction of the laminæ being according to the inclination of the faces of a rhomboid; whereas, in the sapphire, this laminated texture seemed to him not to exist; and he adds, that the fractures of sapphire appeared to him to follow a direction perpendicular to the long axis of the crystal.

With regard to this, I shall observe, that in the foregoing descriptions of the characters peculiar to this substance, (which have been given with all the circumstantial detail necessary in a demonstration which is intended to leave no doubt upon the subject,) the observations of the Abbé HAUY appear to me to have been completely answered. It has there been stated, that one of the peculiar properties of this stone was, that it always preserved a very distinct laminated texture, in all those varieties wherein the crystallization appeared not to have attained its highest degree of perfection, which varieties I have distinguished by the name of imperfect corundum. But it has also been stated, that in proportion as the crystallization possessed a greater degree of perfection, the texture exhibited a less laminated appearance; and that, in this case, it was less easy to obtain a fracture in the real direction of the laminæ.

Another circumstance has likewise been taken notice of, which appears to me to deserve some attention, namely, that in all the different varieties of this substance, the blue colour was generally accompanied with a greater degree of transparency, of gravity, and of hardness; and that, under these circumstances, in proportion as the adhesion of the laminæ was more complete, the laminated texture of the stone became less evident, and it was much more difficult, and sometimes scarcely possible, to obtain fractures in the direction of the laminæ. Nevertheless, among crystals and fragments of sapphire which had but a small degree of transparency, I have frequently met with some, in which the laminated texture was as evident as in the red prismatic variety of perfect corundum, or oriental ruby.

With respect to what concerns the fracture of the sapphire, if the Abbé HAUY was not deceived by an illusive appearance by no means rare in this stone, both in its perfect and imperfect state, (according to which the terminal faces seem to indicate a laminated texture perpendicular to the axis, or a fracture in that direction,) I cannot account for his thinking that he had obtained such a fracture as he describes. I have often tried to obtain fractures of that kind, but without success; never having been able to procure any, except such as were more or less irregular, and exhibited an appearance very different from that of fractures made in a natural direction. Moreover, I have examined a great number of crystals of sapphire, many of which had one of their extremities, many others both their extremities, broken in a direction approaching more or less to that which is perpendicular to their axes, but have never seen, among these fractures, any one that had the appearance of being made in the natural direction of the laminæ; although, at the same time, I have, in many crystals, seen fractures which were perfectly even, and often of considerable extent, in the direction of the planes of a rhomboid, exactly similar (with respect to the measure of its angles) to that belonging to the primitive crystal of imperfect corundum. I have already observed that there sometimes remain, upon the terminal faces of the crystals of sapphire, small facets belonging to the above planes.

I cannot help mentioning also, in this place, a very interesting crystal of sapphire, that is in Mr. GREVILLE's collection. This sapphire, which is of a pale blue colour, is a simple hexaedral pyramid, the solid angle of whose summit measures 40°, and retains upon one of the angles of the summit, which is incomplete, a large triangular facet, belonging to one of the planes of the primitive rhomboid. This plane is striated transversely, in a manner that shews some derangement in the crystallization, perhaps from too great rapidity; and, in the upper part, a still more rapid decrease changes its degree of inclination, causing it to take one which is greater, and which belongs to the secondary obtuse rhomboid already described, in speaking of the fifth modification. These planes, together, completely terminate the crystal at this extremity, in the manner represented in Fig. 39. There may also be observed, two other planes, between which is comprehended the plane I have just described as one of those of the primitive rhomboid: they are produced by the passing of the crystal to a less obtuse pyramidal modification.

Corundum is not the first mineral substance that has exhibited, even in its crystallized state, very striking differences, according to the circumstances that have governed its formation, and the greater or less degree of perfection that has taken place in its crystallization. Felspar is a substance to which the very same remarks may be applied. In the interior part of most kinds of granite and porphyry, it appears in the form of very rugged crystals, generally opaque; whereas, in the fissures of primitive rocks, it frequently has a beautiful transparency; and, when this happens, it rather exceeds the former kind in hardness and in gravity. This difference, which for a long time prevented the latter kind from being joined with the felspar of granites, is so striking, that most naturalists have thought it right still to continue to separate it, at least as a variety, although they allow it a place in the same genus, under the name of *adularia*.

There exists also in the same genus (felspar) a third variety, which, though it had long been known by the name of *white schorl of Dauphiny*, was not, till lately, brought into its proper place. This kind of felspar, which is still more perfect, presents, in such of its crystals as have the greatest degree of transparency, a brilliancy that is even superior to that of the most perfect adularia; this transparency is less similar to that of glass, and approaches nearer to that which is peculiar to the stones that have been hitherto distinguished by the names of gems or precious stones. Indeed, it always appeared to me to possess, in general, the two characters of hardness and gravity, in a somewhat greater degree than adularia. It rather scratches adularia than is scratched by it. In the division which I usually make of the different kinds of felspar, I distinguish this latter, in consequence of the above-mentioned character, by the name of *brilliant felspar*.

We shall see hereafter, that there probably exists a fourth variety of felspar, without reckoning that which is known by its greasy aspect. The fracture of this greasy kind is dull, and resembles that of wax. It exists, in great quantity, in certain granite-rocks, which usually abound with hornblende; of which rocks there is a great number in Scotland. In these, it is frequently of a green colour, which gives it exactly the appearance of jade. This kind of felspar may very probably be a particular kind of substance, nearly allied to one of those (very different from each other) to which French mineralogists give at present the name of *petrosilex*.

COMPACT CORUNDUM.

We have hitherto seen corundum only in a form more or less perfect or determined; it is, however, sometimes met with in a state in which there does not appear the smallest rudiments of crystallization. In this state, (to express which, mineralogists have agreed to make use of the term *compact*,) it resembles, in many respects, a coarse jasper; but its much greater degree of hardness, and its much higher specific gravity, render its true nature easily distinguishable.

In Mr. Greville's collection are many specimens of this compact corundum; they are all of a purplish red colour, not very deep, and are perfectly opaque. By means of a lens, there may be perceived, here and there, some small particles, in which an incipient laminated texture is discernible. These particles are rendered visible by the reflection from the laminæ; they are of a beautiful rose colour, and have a slight degree of transparency. The lens also shows, at the same time, a great number of small globules, of a deep black colour, and of a very brilliant lustre: these globules do not consist of attractable oxide of iron, although that oxide is very common in the substance here treated of; but, on account of their small size, it has not yet been possible to determine their nature.

The compact corundum of a red colour gives pretty strong sparks, when struck with steel; it also gives, by collision, the same phosphorescent fiery red light as the other red varieties of corundum, both perfect and imperfect.

The mean specific gravity of compact corundum, taken from three trials, which differed very little from each other, was 3902.

MATRIX OF IMPERFECT CORUNDUM FROM THE PENINSULA OF INDIA, AND CHIEFLY FROM THE CARNATIC.

This matrix, which, as far as our present knowledge extends, appears to be peculiar to the imperfect corundum of this part of Asia, is a stone of a particular nature: it is sometimes in masses of a loose and granulated texture, with very coarse grains, and pretty much resembles a coarse sand stone; at other times, it has a closer texture, the grains being nearer each other, and less distinct, so as either to give it an appearance similar to the kind of marble known by the name of *coarse-grained saline marble*, or to that kind of prehnite which is composed of a mass of crystals confusedly aggregated. In this matrix, the crystals of imperfect corundum are dispersed, in the same manner as those of felspar are dispersed in porphyry, or rather in certain granites which, besides the aggregated constituent parts belonging to that kind of rock, also contain crystals of felspar which are of a more or less considerable size, and of a perfectly determined form.

When this substance is of that texture in which the grains are closely connected together, it is of a pearly gray colour, sometimes slightly tinged with green, and has a degree of semi-transparency, not unlike that of calcedony. If a piece of this kind is moved about in a strong light, its surface shows a considerable number of small brilliant particles. This appearance arises from the reflection of the light, by the small laminæ that are exposed, in consequence of the fracture of the grains of which the stone consists; and this circumstance proves that it has a laminated texture.

In the last mentioned state, (the most perfect one in which I have observed this stone,) its hardness, although sufficient to scratch glass very easily, is rather inferior to that of felspar. It gives sparks when struck with steel; and, by means of strong collision, emits a phosphorescent light, of a bluish white colour. Friction does not produce any signs of electricity. When put into nitric acid, no effervescence was perceptible.

The specific gravity of this stone, as determined by four trials, which scarcely differed from each other, was 2742; but it is difficult to procure pieces of a tolerable size, which are not mixed, either with hornblende, or with particles of corundum.

It is fusible by means of the blowpipe.

This substance is more usually met with in pieces of a coarser texture, in which the grains are often pretty large, so as to be easily distinguishable by the naked eye. When these pieces are in a perfect state, the grains have exactly the same colour, and the same degree of semi-transparency, as those of the preceding more compact kind. If examined with a lens, the laminated texture of these grains is very evident; and there seems to be, at the first view, a very distinct crystal in each of them. But, if we endeavour to determine the form of any one of these crystals, we find that it is absolutely impossible to do so; as the greatest part of the small facets we perceive, are nothing more than facets formed by compression. I thought, indeed, that I could distinguish some traces that indicated an obtuse rhomboid; but not in such a manner as to permit me to state the fact with certainty. These grains have but a weak degree of adherence to each other; in consequence of which, the stone may often be broken by a very slight effort.

It is, however, still more common to meet with this substance in a state wherein it has undergone, at the surface of each of the grains of which it is composed, an incipient decomposition, that gives them a whiter colour, thereby obscuring, and indeed often destroying, that semi-transparency which I mentioned as being a character of this substance, in its two preceding states. When this is the case, if some pieces of the stone are put into nitric acid, an effervescence soon takes place, the strength of which is in proportion to the degree of decomposition the stone has undergone; but this effervescence, in a short time, entirely ceases. It seems, from this circumstance, that the lime contained in the stone, (which, as will be hereafter seen in the account of its analysis, Mr. Chenevix found to amount to 15 parts in 100,) being exposed to the action of the air, by the alteration or decomposition of the stone, had afterwards combined with a portion of carbonic acid.

To the above mentioned lime, (carried away by the rains which wash the exposed parts of the rocks composed of this substance, and deposited upon the fragments of corundum scattered at the feet of those rocks,) ought no doubt to be attributed, that calcareous incrustation which is frequently observed to cover, either partially or entirely, many fragments of imperfect corundum, found among the specimens of that substance sent to us from India.

If we let a piece of this matrix remain for a certain time in nitric acid, it is attacked by it, without being dissolved, and without changing its form; but if, after being taken out, it is pressed between the fingers, it may be crumbled by a very trifling effort, and may, by being rubbed, be reduced to a sort of paste.

SUBSTANCES WHICH ACCOMPANY THE IMPERFECT CORUNDUM, IN THE ABOVE MENTIONED MATRIX, FROM THE PENINSULA OF INDIA.

Felspar. There are sometimes found, in the matrix here treated of, pieces, more or less considerable in size, of a laminated substance, which has the same greenish gray colour, the same brilliancy, and, in short, the same appearance, in many respects, as the corundum itself. It is indeed the more easy to confound this substance with corundum, as it is frequently accompanied with crystals of the latter. I have myself been several times led into this mistake, before I had paid such particular attention as I have since done, not only to corundum, but also to every thing relating to the substances which accompany it.

The most usual colour of this substance, as I have already said, is gray, slightly inclining to green, which is sometimes mixed with a small portion of brown. It possesses a pretty considerable degree of semi-transparency, which may be compared to that of calcedony, or more properly to that of the stone known by the name of cat's eye. Its hardness is inferior to that of quartz; but appears to be exactly the same as that of felspar. It gives sparks, when struck by steel; and, by collision, emits a yellowish phosphorescent light. Friction does not cause it to give any signs of electricity.

This stone may be divided with great facility, in the direction of two opposite and parallel faces; and the fractures thereby obtained have a brilliant lustre, exactly resembling that of the fractures of corundum. Upon these fractures may be observed very fine but very evident striæ, which indicate that the laminæ have a direction different from the above; but I have not yet been able to obtain an even fracture, in the direction of these striæ. All fractures made in any other direction than that first mentioned, are irregular and unequal; very often also they are dull, and somewhat similar to that of wax.

The mean specific gravity of this substance, taken from four trials, which differed very little from each other, is 2643.

This substance is fusible by the blowpipe, like common felspar.

The result of the analysis of this substance, made by Mr. CHENEVIX, is, in many respects, similar to that of the analysis of adularia, made by Mr. VAUQUELIN; yet it differs very essentially from that, by the want of potash, and by the proportion of lime being more considerable.* The presence of the last-mentioned earth is sometimes rendered evident, in the parts which are slightly decomposed, by the weak and momentary effervescence that takes place in those parts, when the substance is put into nitric acid.

On the other hand, many of its external characters are such as naturally lead to its being ranged with adularia. It differs from it, however, in the facility with which the latter may be broken in two different directions; while, in the substance here treated of, fractures can never be obtained, except in one of those directions; nor have I ever been able to observe on the fractures of any other kind of felspar, those fine striæ which,

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* The analyses made by Mr. VAUQUELIN, of the different kinds of felspar, naturally lead me to make some further remarks upon that substance; which, indeed, may be equally applied to many other substances. The able chemist above mentioned, found 14 parts of potash in 100 of adularia, and 13 in 100 of the green felspar of Siberia; whereas, he did not find an atom of that substance in another kind of felspar, which was in a laminated mass; nor in that decomposed felspar which is known by the name of kaolin. Potash may therefore be considered as not being one of the constituent parts of felspar, but merely as a foreign substance, accidentally interposed therein. Adularia, in that case, would be nothing but an impure kind of felspar; and would present the astonishing phenomenon of a substance constantly impure, in its most perfect state of transparency and crystallization. It is indeed difficult to conceive that the potash is merely interposed, in such very considerable proportion, in the kind of felspar called adularia; yet, if it really formed one of its constituent parts, it would necessarily produce a substance totally different from those which do not contain any of it; whereas, all the mineralogical characters of felspar and adularia, evidently demonstrate that these two substances are perfectly similar in their nature. There still remain, in my opinion, many discoveries to be made, in that part of chemistry which relates to the composition of mineral substances, before the chemist and the mineralogist shall be enabled to proceed together, with a certainty of agreement respecting the object of their inquiries.

as I have already said, are very evident on this stone. It differs also from common felspar, in not being capable of acquiring electric properties by friction; whereas common felspar may, by long continued friction, be made to acquire such properties. The semi-transparency of this stone likewise, and the nature of its lustre, are such as give it a greater analogy to gems or precious stones; and, in these respects, it is very similar to the variety which I have called shining felspar.

As this substance appeared to me to have a great analogy with another, which sometimes, in small fragments, accompanies the perfect corundum in the sand of Ceylon, (in which, however, they are more rare than corundum itself,) I desired Mr. CHENEVIX to be so good as to add to the analyses he was about to make, that of these fragments. The result of his analysis of them differs so little from that afforded by the substance above described, that it strongly confirms the analogy I had supposed to exist between them.

Having been so fortunate as to find, among the few fragments I could collect of the last mentioned substance, three crystals, in which the crystalline form is perfectly determined, I am enabled, by their means, to add the crystalline character of the substance, to those I have given in the foregoing paragraph. These crystals are rhomboidal tetraedral prisms, of about 100° and 80°, the two terminal faces of which are inclined, in a contrary direction, upon the obtuse edges of 100°, in such a manner as to make with them, an angle of 105° on one side, and one of 75° on the other; and as, (in the only three crystals it has yet been in my power to examine,) the planes of the prisms are very nearly equal to the terminal faces, their appearance is exactly that of a rhomboid. The terminal faces of the crystals here spoken of are *chatoyant;* and, in the fragments, the planes which correspond to these faces have a similar property, when held in a proper direction. In some, these faces then appear of a pearly white colour; in others, the colour is rather yellowish: some of them reflect a pale blue colour; in many others, the colour reflected is a beautiful deep sapphire blue, that entirely occupies the whole extent of the face which possesses the property here spoken of. To this stone ought to be referred, that which is known by the name of moon-stone of Ceylon, when it is not of the kind called *cimophane,* (the *chrysoberyl* of WERNER,) which is often found also in the sand of this island, mixed with rubies, sapphires, &c.

The opinion I am naturally led to adopt, in consequence of the detail I have just given respecting this stone, is, that it most probably is a kind of felspar, and ought to be ranged with that substance, as forming an additional variety.

In some of the pieces of this stone, which are found in the same matrix with the imperfect corundum of the Carnatic, a talcy earth (which often also appears in a separate state) is interspersed throughout their substance, and causes them to have a less compact texture, and a very inferior degree of hardness. The stone, at the same time, acquires a slight greasiness to the touch, and loses the semi-transparency which is peculiar to it: it may still, however, be easily divided, in the direction already described as that in which it is naturally divisible.

Fibrolite. The substance I have distinguished by this name, which sometimes also accompanies the imperfect corundum from the Carnatic, in its matrix, has always offered itself to my observation, either of a white colour, or of a dirty gray. Its hardness appeared to me to be rather superior to that of quartz; as, after having rubbed them together, the latter seemed to be the most worn of the two. It gives bright sparks, upon being struck with steel. Collision causes it to emit a phosphorescent light, of a deep reddish colour. It cannot, by friction, be made to give signs of electricity.

Its mean specific gravity, taken from four trials, is 3214.

This substance was tried with a blowpipe, by Mr. FLEURIAU DE BELLEVUE, a mineralogist much accustomed to such operations, and found to be absolutely infusible, even when placed, in very minute particles, upon cyanite.

The external texture of this substance is usually fibrous; the fibres being very fine, and closely connected together. When it is broken according to the direction of the fibres, its internal texture appears to be exactly the same; but, if it is broken in a direction transverse to the fibres, its texture appears to be compact. The lustre of the last kind of fracture is rather vitreous; and there is nothing in its appearance that gives reason to think it was made in the direction of the laminæ. When we wish to try the hardness of this stone, it should be done in a direction which is transverse or perpendicular to the fibres; not in a direction parallel to them.

There exist many pieces of this substance that are merely irregular aggregations, in which the fibres cross each other, in bundles, in different directions. I have only once seen it in a form which could be considered as a determined one; viz. a rhomboidal tetraedral prism, of about 80° and 100°, the terminal faces of which are imperfect. But, as this prism, although pretty regular in its form, is the only one I have yet been able to discover, the above observation requires to be repeated, before we can safely make any dependence upon it. I must however add, that among the pieces of this substance, I have met with several, which appeared to have more or less tendency to the above-mentioned form.

The analysis of this substance, made by Mr. CHENEVIX, concurs with the whole of its external characters, in warranting us to consider it as being different from any of the mineral substances hitherto known; in consequence of which, I have thought it right to distinguish it by the name of *fibrolite*.

Thallite. The substance called thallite (the *epidote* of the Abbé HAUY) also sometimes accompanies the corundum from the Carnatic, in its matrix. This substance is found in three distinct states, hitherto unobserved, in all of which its appearance is so different from its usual one, as to have prevented me, for some time, from knowing it.

In one of the above states, this substance is inclosed in the matrix, in small detached masses, from the size of a pea to that of a hazle nut, and even larger. Its usual colour is either a brownish green or a yellowish green; and it has only a slight degree of semi-transparency, even at the edges.

Its hardness is the same as that of the other known kinds of thallite, which I have always found to be rather superior to that of quartz; and, as most of the other characters belonging to this kind of thallite are similar to those of the kinds already known, I shall, in the following description, mention only such of its characters as, on account of their being different, might lead to false ideas respecting it.

The major part of these small masses present no determined form; in some of them, however, a perfectly regular crystallization may be observed. In this latter state, the greater number of crystals appear in the form of rhomboidal tetraedral prisms, of 128° 30′ and 51° 30′, in which the terminal faces are perpendicular upon the sides, as in Fig. 40. (Plate IX.) This form, which was before unknown in the thallite, and which might at first view be taken for a primitive one, was very likely to lead to an erroneous idea; it may however be explained by another form, which is also met with in perfectly determined crystals. In these last, the prism is hexaedral, with two edges of 114° 30′, two others of 128° 30′, and the two last of 117°; its terminal faces are also perpendicular upon the sides of the prism, as in Fig. 41. Now this form is exactly the same as one of those already observed in the prism of the common thallite, and is produced in the following manner, viz. the primitive rhomboid, the edges of which are 114° 30′ and 65° 30′, has each of its acute edges replaced by a plane, inclined, in a contrary direction, upon one of the sides of the prism, so as to make with it an angle of 128° 30′. I have often found this hexaedral prism terminated, in the same way, by planes perpendicular to its sides, among the crystals of thallite from the Alps of Dauphiny. The preceding rhomboidal tetraedral prism, consequently, is produced by an increase of the faces which have replaced the edges of 65° 30′; which increase has been such as to cause the sides of the primitive rhomboidal prism, on which each of them incline, to disappear: this is represented by the dotted lines in Fig. 42. The direction of the laminæ, in these crystals, strongly supports the foregoing explanation. Sometimes the rhomboidal prisms become of an indeterminate form, by being flattened so as to render the edges of 128° 30′ much more obtuse; when that happens, they have no longer any regular measure.

In this first state of the thallite which accompanies the imperfect corundum from the Carnatic, the pieces, whether they are crystallized or of an indeterminate form, have their surface covered with little asperities, thereby exhibiting an appearance which cannot be better described, than by comparing it to that preparation of fish-skin which is called shagreen. This is the natural effect of their peculiar texture; for, if one of these pieces is broken, we perceive very plainly, that it is not of a homogeneous texture, but is mixed with small particles of the substance we have already described as the matrix of corundum; which mixture is often in such proportion, that the quantity of the latter substance is equal, or nearly so, to that of the thallite itself.*

The appearance the surface of these pieces exhibits, is owing to the destruction, at the said surface, of the forementioned small particles of the matrix, which, as is well known, is very easily decomposed. There sometimes even remains, in the little cavities, which are very numerous, small particles of this matrix, generally in a state of decomposition. In this case, if the pieces are immersed in nitric acid, a slight and momentary effervescence takes place; and, if this immersion is continued for some days, the acid then acts upon those particles of the matrix which are inclosed in the interior part of the substance, as has been already mentioned in the description of this matrix;

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* The regularity of the form in which these crystals are found, will certainly appear surprising, when we consider the immense quantity of heterogeneous particles which are interposed within their substance, and, consequently, between their crystalline molecules, the attraction of which for each other, it would appear, must be thereby considerably obstructed; but the same circumstance takes place in other substances, for instance, in the calcareous spar known by the name of *rhomboidal sand-stone of Fontainbleau.* The Abbé HAUY, in the article *axinite,* (the *thumerstein* of WERNER,) makes the same observation, and gives a very ingenious explanation of the circumstance. This calls to our mind the remark of the celebrated DOLOMIEU, viz. that it appears, in some cases, that a foreign substance, when interposed in a crystal, instead of obstructing its crystallization, tends rather to give it a greater degree of regularity.

in consequence of which, the pieces, when taken out of the acid, may be easily crumbled by the slightest pressure of the fingers; and nothing remains in its former state, except the small particles of the thallite.

There exist some pieces, in which the particles of the matrix are infinitely more numerous than those of the thallite itself; the latter then only appears in the form of small greenish or yellowish points, disseminated in greater or less proportion, and in detached spots.

In the second of the states in which this substance is found in the matrix of corundum, it appears in the form of pretty thick prisms; these prisms have deep grooves or channels, which, as is often observed in the crystals of tourmalin, render their shape absolutely deformed. The substance, in this second state, is more pure; no particles of the matrix, which were said to be mixed with it in its first described state, are to be seen. The semi-transparency is more general, and in a greater degree. The green or yellowish colour is also more deep; and sometimes a slight tinge of red is mixed with those colours. Some parts of the pieces are less grooved than others; and those parts indicate the forementioned rhomboidal prismatic form of 128° 30′ and 51° 30′; but it is very difficult to obtain an even fracture of this stone.

In the third state, this substance is so very similar to the purest imperfect corundum, that at first I supposed it to be of the same nature; and it was not until I had examined it more particularly, that its specific gravity and its hardness, so different from those of corundum, led me to think it could not possibly belong to that substance, and that it ought, from those characters, to be ranged with the thallite. The analysis of it, made by Mr. Chenevix, has proved the truth of my observations.

Its semi-transparency, in this state, is more considerable, and approaches very nearly to complete transparency. Its colour is generally a beautiful topaz yellow, which sometimes inclines slightly to green. I have hitherto met with it only in pieces of an indeterminate and irregular form, the size of which, though more or less considerable, never exceeded that of a small nut. Its fracture is generally irregular, and often partially conchoid. In some pieces, however, may be perceived small particles which seem to have a laminated texture, the direction of the laminæ being such as to announce the primitive crystal of the thallite; but I have never been able to bring this substance to the shape of that crystal, by any artificial division or fracture of it.

Hornblende. This substance is that which is most constantly, and most abundantly, contained in the matrix now treated of. There are indeed some pieces of the matrix, wherein the proportion of hornblende is as great as in some granite rocks of which it constitutes the principal component part; and those pieces have an appearance very similar to that of such rocks. It is generally of a deep black colour, and opaque; but I have sometimes seen it in the form of small elongated crystals, of a fine green colour, and transparent. Its texture is very evidently laminated; and it is seldom that any determinate form can be perceived in it; sometimes, however, the rhomboidal tetraedral form of its prism may be distinguished.

Quartz. In this matrix is also found quartz, in small detached fragments, of an indeterminate shape. This substance, however, is by no means common; on the contrary, of the various substances that are met with in this matrix, quartz is one of the most rare. It is generally of a dull white colour, and has but a small degree of transparency.

Mica and Talc. These two substances are not very common in this matrix, yet they are more so than quartz. The mica has a silvery hue, sometimes slightly inclining to green; and, in the pieces of the matrix in which it is found, it generally appears in small detached spangles.

The talc is generally of a pale green colour; and, in those parts of the matrix where it is met with, it is in pieces nearer each other than was the case with respect to the spangles of mica. Sometimes it forms small masses, little or not at all mixed with any other substance. At other times, it is found in that very divided or earthy state (seldom without some heterogeneous mixture) which has been hitherto distinguished, after Mr. WERNER, by the name of *chlorite*.

There are also, but more rarely, met with in this matrix, pieces of real steatite, of a white or a greenish colour.

According to a letter written from Tritchinopoly, the 10th of November, 1792, to Sir CHARLES OAKLEY, then governor of Madras, and communicated by him to Mr. GREVILLE, it appears that the imperfect corundum of the Carnatic, as well as the matrix in which it is contained, forms, in the place from whence it is procured, distinct strata; and that these strata are accompanied by a substance which is in considerable abundance, and which cannot be better distinguished than by the name of talcy mica. This substance is easily separated from the matrix of corundum; and it is usual to separate it, on the spot, before the pieces containing the corundum are sent away for the purposes of commerce. Some of it was sent to Mr. GREVILLE by Sir CHARLES OAKLEY himself. The colour of this is a blackish brown; and its exterior appearance is nearly similar to that of mica; but the lustre of its surface is somewhat less bright. Its texture is very distinctly laminated; the laminæ, which are very thin, being chiefly evident at the edges; they adhere, however, more strongly to each other than those of mica. These laminæ may be bent, without breaking; but they do not show the smallest signs of elasticity. This substance possesses but a small degree of transparency, and that only when it is brought into the state of very thin laminæ; its colour then appears a brownish yellow, not much unlike that of resin. It is much more greasy to the touch than mica; it is also less hard, so that it may be easily scratched with the nail; and, if we scratch it with the point of a penknife, we are not sensible of that kind of slight shivering which takes place when mica is so treated. Mr. GREVILLE, in the Paper upon corundum which he presented to the Royal Society, in June, 1798, was perfectly aware of the difference between this substance and that properly called mica. In the collection he received of the former, are many crystals, several of which are nearly an inch in length, and two or three lines in thickness. Some of these are in the form of a rhomboidal prism, of 60° and 120°; others have the form of a regular hexaedral prism. Upon the whole, the characters of this substance may be considered as partaking both of those belonging to mica and those belonging to talc.

Its mean specific gravity, taken from three trials, which differed very little from each other, is 2709.

Garnets. In the matrix here spoken of, and also in the corundum itself, garnets are sometimes met with; they are of a deep red colour, and of a roundish form. There was lately sent to Mr. GREVILLE, a parcel of imperfect corundum, found among the sands of the river Kirtna, in the district of Ellore,* in the northern part of the government of Madras. This corundum, some of the crystals of which were the best defined of any I had yet seen, was mixed with pretty large angular fragments of garnets, of a very deep blood-red colour, and of the most beautiful transparency.

Zircon. The same parcel of imperfect corundum, of which I have just spoken, from the district of Ellore, was also mixed with crystals of zircon, the jargon of the lapidaries. These crystals, which were in perfect condition, deserve to be mentioned, not only on account of their size, but also on account of the great number of varieties and rare forms they exhibit. Such, for instance, is the primitive very obtuse octaedron, which is in large crystals, with sides of more than six lines in length. I had observed this form, for the first time, fifteen years ago, in some crystals found in the sands of a rivulet, called Riou Pezzouliou, which runs between the volcanic rocks at Expailly, near Puy in Velay; but these crystals were very small. The celebrated ROME' DE LISLE, who published my account of these crystals, in his excellent work on the external characters of minerals, mentions the opinion I then entertained, and had communicated to him, that the jargon and the hyacinth were only two differently-coloured varieties of the same substance, and were both derived from the same primitive form.

The most usual colour of these crystals of zircon, is a brown, which sometimes inclines to yellow: they often, however, have that fine yellowish red colour, which causes this stone to be

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* This district is contiguous to that in which the diamond mines are situated.

distinguished by the name of hyacinth. Their size, and the perfection of their crystallization, enabled me to ascertain, that the angle formed by the meeting of the planes of the octaedron at the base, measures 85°; and that formed by their meeting at the summit, 95°; as is stated in the work I have just mentioned. The Abbé Hauy, in his excellent work on Mineralogy, fixes the first of these measures at 82° 50′, and the other at 97° 10′. I imagine he must have been deceived, either by the crystals having been of too small a size, or by their not having been of a perfectly regular form.

Amongst the pieces of the stone which serves as a matrix for the imperfect corundum, are found some, in which may be perceived a great number of very brilliant small points, of a yellowish red or orange colour. When viewed with a lens, these points appear to be minute crystals, perfectly transparent; but it is impossible to ascertain their form. On some of them may be perceived small facets; others have the appearance of prisms: they are of very considerable hardness. I am unable to form a decided opinion respecting the true nature of these microscopic crystals: but, all things considered, I am inclined to think it probable that they belong to the zircon.

Although these crystals, in the state I have just described, are extremely small, that state is by no means the smallest in which they are found in this substance; they also exist in it, so very minute in size, that our eyes, even when assisted with instruments, are scarcely able to distinguish them. In this state, they become a real colouring matter, for those parts of the matrix in which they are contained; which parts thereby acquire a fine orange colour, more or less deep. By attentively examining these parts with a lens of sufficient power, we may perceive the crystals approaching nearer to each other, and diminishing in size, so as at last to become invisible: very often, they shew themselves only in the form of small filaments, scarcely perceptible.

The above is not the only substance which presents the phenomena just described, even in the stones here treated of; the thallite sometimes has the same appearances; and, in that case, it gives to the matrix a green colour, similar to its own. When this happens, we may sometimes, by means of a lens, perceive small microscopic crystals of thallite; very often, however, they are too small to be distinguished.

It appears therefore that coloured stony substances, by interposing themselves, in particles too small to be seen, in stones, may sometimes produce the same effects (and probably in the same manner) as are produced by the various metallic oxides.

Very attractable black Oxide of Iron. This ore of iron (which is the *fer oxidulé* of the Abbé HAUY, and the *magnetic iron ore* of the Germans,) is also found sometimes in the matrix of imperfect corundum from the peninsula of India; but, as we shall hereafter see, it is by no means so general, nor so abundant, in that matrix, as it is in the matrix of imperfect corundum from China. In the former, it appears in small grains of an indeterminate shape, which are sometimes interposed between the particles of hornblende, in such a way as might easily lead us to suppose, that the latter substance has the property of being acted upon by the magnet. In those parts of the matrix which contain this oxide of iron, are found hexaedral prisms of corundum, the surface of which is entirely covered by a layer of the oxide, about a quarter of an inch in thickness, and absolutely moulded upon them.

MATRIX OF IMPERFECT CORUNDUM FROM CHINA, AND SUBSTANCES WITH WHICH IT IS ACCOMPANIED.

This matrix is totally different from that of the imperfect corundum of the Carnatic, being a granite rock, composed of an aggregated mixture of felspar, fibrolite, mica, and very attractable black oxide of iron. I have not yet seen in it any particles of that particular substance, already described, which composes the principal part of the matrix of imperfect corundum from the Carnatic.

The four substances above-mentioned, are unequally distributed throughout the mass; some pieces being composed almost entirely of one of them; while, in other pieces, those substances are mixed together in various proportions, and sometimes in nearly equal ones. The crystals of corundum are disseminated in the mass, in the same manner as those of the Carnatic are in their matrix; but, as the particles of the matrix now treated of have a much stronger adherence to each other, and also to the crystals of corundum, it is difficult to detach the said crystals from the matrix, without breaking them.

The felspar has, in this matrix, the same appearance it usually has in granites. Its colour is generally reddish; very often, however, it is of a grayish white colour. I have never observed it to have any determined crystalline form; but, when it is in masses of a certain size, their texture is evidently laminated.

The mica has a silvery appearance, sometimes inclining a little to a yellowish colour, at other times to a greenish one. Its laminæ are frequently united together, so as to form prisms, which are pretty thick, but most commonly of an irregular shape; sometimes, however, the appearance of a regular form may be observed in them.

The fibrolite is in much greater proportion in this matrix, than in that of the imperfect corundum from the Carnatic; and it is more generally dispersed throughout its substance; its fibres, however, are shorter, and form small detached diverging pencils, which unite together, crossing and penetrating each other in all directions, so as to present masses of a more considerable size. In this manner, it often entirely surrounds the crystals of corundum, and it is then impossible to disengage them from it. Its most usual colour is a whitish gray, but it is also frequently of a dull white. It is sometimes mixed, nearly in equal proportions, with felspar, and the attractable black oxide of iron; and thus produces a stone which, if polished, would have a very beautiful appearance. The analysis which Mr. CHENEVIX has made of this substance, concurs with all its other characters to demonstrate, that it is decidedly of the same nature as the fibrolite of which I have already spoken, as being found in the matrix of imperfect corundum from the Carnatic.

The very attractable black oxide of iron is, of the various substances found in the matrix of imperfect corundum from China, that which is most constantly, and most universally, mixed with it. In the smallest piece of this matrix that can be broken off, some particles of the oxide may generally be perceived; even the crystals of the corundum itself are hardly ever free from it, it being observable, not only upon their exterior surface, but also within their substance. This oxide of iron is usually disseminated, in this matrix, in small masses of an indeterminate shape, which very often are nearly contiguous to each other. It is very rare to find among them any crystals perfectly formed; yet I have sometimes observed octaedrons, dodecaedrons, and segments of the first of these two forms, or octaedrons, which had in each pyramid, and exactly opposite, one of the faces much larger than the three others. This last form, appeared to me to be the most common one.

This oxide sometimes exists also in masses of a much larger size; but they are almost always of an irregular shape. I have often observed pieces as large as a hazel nut; and sometimes, though much less frequently, of a still more considerable size.

The mean specific gravity of this oxide of iron, taken from four trials, was 5073. This is rather superior to what has been considered as the specific gravity of this ore of iron, it having been always estimated at less than 5000. I know nothing to which I can attribute this difference, except to the peculiar texture of the oxide here described; which, as far as I have been able to observe, has always appeared to me to be much more compact than is usual in this species of iron ore. In other respects, it has, when perfectly pure, all the other characters belonging to this species.

There are some pieces of the matrix now treated of, in which the small masses of the above oxide, by being mixed with fibrolite and mica, exhibit an appearance that might cause them to be considered as pieces of a true granite; in others, it is mixed, in different proportions, with the substance of the corundum itself, in such a manner, that it is impossible, by the eye, to distinguish this mixture from the pure metallic oxide. Mr. Chenevix analyzed one of these pieces; and found that it contained nearly equal quantities of corundum and of oxide of iron.

If, to what has been already said, I add, that there are sometimes found in this matrix, small pieces of green pulverulent talc, (chlorite,) and small masses of thallite, in thin elongated crystals, of a beautiful yellowish green colour, in the form of diverging rays, I shall have mentioned all the substances I have been able to observe, in the matrix of imperfect corundum from China.

Of the matrix of imperfect corundum from the kingdom of Ava, a small quantity only was sent; but that quantity was sufficient to demonstrate, that its nature is exactly the same as that of the matrix of imperfect corundum from China.

MATRIX OF PERFECT CORUNDUM FROM THE ISLAND OF CEYLON, AND SUBSTANCES WITH WHICH IT IS ACCOMPANIED.

I cannot help regretting, that it is not in my power to give much information respecting the matrix of perfect corundum from Ceylon. The precious stones comprised under that denomination, which are selected from the sands washed down by the rivers of the island, and sold under the name of *sand of Ceylon*, have never been brought to Europe in any kind of matrix, nor has any account of their matrix ever been transmitted to us. Perhaps, indeed, no more information on this head could be procured on the spot, than was obtained by those naturalists who sought for the origin of the sapphires, &c. found in the sands of the small rivulet at Expailly, already spoken of. I may also observe, that the great care taken to free the sand of Ceylon from every substance, except such as, on account of their hardness and their lustre, are considered as of value in commerce, deprives us of all chance of obtaining that knowledge respecting the matrix here treated of, which might otherwise be acquired, from an attentive examination of the various substances which it is natural to suppose are brought down, with the sand, by the streams. We shall, however, presently see, that one of those fortunate events by which nature sometimes rewards the labours of those who devote themselves to the study of her works, has presented us with some very interesting facts on this subject.

In order to render as complete as possible, every information which is connected with the investigation of corundum in general, and particularly to make known every thing I have been able to learn respecting this stone in its highest degree of perfection, I think it right to make some remarks on the various substances with which it is accompanied, in the sand sent to us from Ceylon; although I cannot undertake to assert positively, that these substances really accompany it, when in its matrix.

Spinelle. The first of these substances, and one which composes more than nine parts in ten of the whole mass of the sand, is the spinelle ruby, now generally known by the name of spinelle. Notwithstanding the great number of crystals of this substance which are found in the sand, it is very uncommon to meet with one of a tolerable size, that is both transparent and of a perfect form: indeed most of them are merely fragments. The selection that has already been made in India, where these stones receive their first polish, in order to be distributed for sale, is no doubt the chief reason of the above circumstance:

we cannot therefore hope to find in the sand, any crystals of consequence, except such as have by accident escaped this first search; some of these, however, I have had the good fortune to meet with.

Among the beautiful series of crystals of this substance which I have been so happy as to procure, and to place in the several collections with the care of which I am entrusted by the friendship of their proprietors, there are four, in Mr. GREVILLE'S collection, that I think it right here to take notice of. The forms of these crystals appear to me to be hitherto absolutely unknown; for the Abbé HAUY, who may be justly considered as the most learned of those who devote themselves to the study of crystallography, does not even mention them, in the treatise on mineralogy he has just published.

One of these forms, is a complete tetraedron, as in Fig. 43. It is produced by the enlargement of four of the faces of the octaedron, at the expence of the other four, which it has entirely caused to disappear. There are, in the same collection, many other crystals which are passing into this form, and are more or less advanced towards it. One of them, in which there still remain some traces of the octaedron, which had entirely disappeared in the preceding, deserves also to be mentioned. This variety, which is more common than the preceding, is represented in Fig. 44.

The second of the above forms, is a very acute rhomboid, the rhombic planes of which have 120° for the measure of their obtuse angles, and 60° for the measure of their acute ones. Fig. 45. This crystal is produced by the enlargement of six of the faces of the octaedron, at the expence of two opposite faces, one in each pyramid; which last faces have entirely disappeared. There are also several crystals in a progressive state, and more or less advanced, from the octaedron to this form. (Fig. 46.)

The third form, is a complete dodecaedron, with rhombic planes. Fig. 47. It is produced by the enlargement of the planes which have replaced the twelve edges of the octaedron; a modification to which the Abbé HAUY has given the name of *emarginée*. This enlargement is such as to have caused the entire disappearance of the eight primitive planes of the octaedron. There are also, in Mr. GREVILLE's collection, crystals more or less advanced towards this form, some of which no longer show any traces of the planes of the octaedron, except by extremely small equilateral triangular planes, as in Fig. 48. In these crystals, it is very common to find the decrease of the laminæ evidently indicated by striæ.

The fourth form, is a rectangular tetraedral prism, terminated by two pyramids, also tetraedral, which are situated upon the sides of the prism, and have equilateral triangular planes. This crystal is produced merely by the edges of the base of the octaedron being replaced; which replacement separates the two pyramids, by a prism more or less elongated. There are some crystals in which this prism is pretty long, as in Fig. 49; others in which it is, on the contrary, very short, as in Fig. 50.

Although the Abbé HAUY has described the cuneiform octaedron, I think it right to add to his description, that, in this variety, the separation of the two opposite faces in each of the pyramids, becomes sometimes so considerable, that the crystal thereby changes its appearance, and acquires that of a rhomboidal tetraedral prism, of 109° 30′, and 70° 30′. This prism is terminated by two diedral summits, with isosceles triangular planes, the apices of which are situated upon those edges of the prism which measure 70° 30′, making with them an angle of 125° 15′, and meeting, by their bases, at the top of the crystal, in an angle of 109° 30′, as in Fig. 51.*

I also think it right to add, to what the Abbé HAUY has said respecting the colours of this substance, that it is sometimes perfectly colourless, sometimes of a yellow colour, and sometimes of a bluish one.

We were as completely ignorant of the nature of the stone which serves as a matrix to the spinelle, as we were respecting that of the matrix of the perfect corundum of Ceylon, when a number of specimens were sent from India to Sir JOHN ST. AUBYN, by Mr. WHITE, amongst which were two pieces of the highest value, inasmuch as they served to show us, for the first time, the substance now treated of, inclosed in its matrix. I flatter myself a description of these two pieces will be thought worthy the attention of the Royal Society, particularly as they also contain a species of iron ore hitherto unknown.

One of these pieces is a calcareous spar, of a granulated texture; the grains are very large, and are intermixed with each other, so as to adhere very strongly together, but their

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* The dodecaedron, and the octaedron passing very rapidly to the tetraedron, had already been mentioned by Mr. ESLINGER, (*Journal de Physique,* Vol. LII. p. 225,) as making part of the collection of crystals of this substance in Mr. WERNER'S possession: the other varieties had not yet been described. According to some of the external characters by which Mr. ESLINGER describes the spinelle, I am inclined to think, that he includes some Ceylanites in that description, and also some oriental rubies. Such, for instance, I suspect to be, that which he says has a starry reflection; also the hexagonal prism with the alternate angles of the base replaced; and the cube, (without doubt, slightly rhomboidal,) which has a small plane upon two of its solid angles diagonally opposite to each other: a form that is very rarely met with, even in the oriental ruby.

fracture shows that they are very evidently laminated. In the substance of this spar are contained a great number of small prismatic crystals of mica, of a beautiful yellow colour, like that of the topaz; they have also the lustre, and the transparency, of that precious stone, for which they might the more easily be mistaken, as several of them, which show the sides of their prisms on the exterior part of the stone, appear to have their surface slightly rounded.* Very thin laminæ may without difficulty be detached from the terminal faces of the crystals; these laminæ are perfectly elastic.

There are also, in this calcareous spar, small pieces of a metallic substance, which deserves to be particularly described.

The colour of this substance is gray, slightly inclining to red, so as very much to resemble that of arsenical cobalt, or of nickel. The substance is very brittle; the slightest blow breaks it; and it may, by a moderate degree of pressure, be reduced into a black powder. Its fracture is conchoid, with a very fine and compact grain; and it has a very brilliant lustre. The magnet

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* All the authors who have treated of mica, say that it is transparent only when in very thin laminæ. This is a mistake. When the crystals of this substance are in as perfect a state as they possibly can be, that is to say, when their crystalline laminæ are in complete contact with each other throughout the whole extent of their surface, (a circumstance very uncommon, but which is known by the sides of their prisms being perfectly smooth,) they are usually transparent. I have seen crystals of mica, of a pretty considerable thickness, which were perfectly transparent, in whatever direction they were viewed; although sometimes such crystals, when their terminal faces have a very shining silvery lustre, (which shows that they reflect all the light that falls upon them,) have not the smallest transparency, when viewed in a direction perpendicular to their axis; many of them, however, appear transparent, when viewed through the edges of the laminæ, that is to say, in a direction parallel to that of their axis. The above is not the only mistake that has been made with respect to this substance; a correct description of which, I hope, some time hence, to be able to lay before the Royal Society.

acts upon it, very nearly as strongly as it does upon iron in a perfectly metallic state. When this substance is immersed in nitric acid, no effervescence takes place. By means of a file, or merely by the blade of a knife, a black powder may easily be obtained from it, without in the least diminishing the lustre of the part from which it is taken. If a magnet be brought near this powder, it is instantly attracted by it. Those parts of this substance which appear to have been exposed for any length of time to the contact of the air, are become of a black colour.

I know no other metallic ore whose exterior characters are analogous to those I have just described; and I very much regret that the scarcity and the consequent value of this specimen, as well as of that about to be described, prevent their being made use of for the purpose of an analysis, the result of which it would be so desirable to be acquainted with. If, without such analysis, I might be permitted to form an opinion respecting this substance, I should be much inclined to consider it as a martial pyrites, or sulphuret of iron; but in which the iron, in a metallic state, is combined with a much smaller quantity of sulphur than in common pyrites; some small traces of the latter, however, may be perceived in this specimen, by the side of the metallic substance above described.*

In this same calcareous spar may also be observed, small crystals of a greenish colour, which have hexaedral prisms; they are of very inconsiderable hardness. I believe they belong to that particular species of phosphate of lime, which the Germans have distinguished by the name of *spargelstein.*

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* Since the above was written, I gave a few grains of this substance to Mr. CHENEVIX; who, from that small quantity, was able to determine that it contained nothing but iron and sulphur.

But, what renders the specimen I am now describing, in the highest degree interesting, is, that there are some perfectly well formed octaedral crystals of spinelle, of a pale purplish red colour, inclosed therein. Here then we have a fair and unquestionable instance of the spinelle within its matrix: we shall however see presently, that the nature of this matrix is not constantly the same.

The second of the two pieces I have mentioned above, as being the matrix of the spinelle, is a mass of adularia, of a grayish white colour, about six inches in length, and of a proportionate thickness. This adularia is tolerably pure, in one half of the piece; but, in the other half, it is mixed with particles (much more considerable in size, and in much greater proportion than in the preceding piece,) of the very brittle and very attractable metallic substance already described. There may also be observed in it, some small pieces of a substance of a brownish green colour, but which becomes grayish when scraped; this substance, which is by no means hard, appears to me to be of the nature of steatite. If this specimen is moved about in a very strong light, there may be perceived in it, here and there, small particles, which have a silvery appearance, and which are rendered very evident, by their laminæ being in a direction contrary to those of the adularia which is near them. I consider these small pieces as belonging to the kind of felspar I have already described, and mentioned as being found in the sand of Ceylon which contained the perfect corundum and the spinelle, and as frequently reflecting a beautiful deep sapphire blue colour. This specimen contains fewer crystals of spinelle than the preceding one; some, however, may be perceived in it. It seems also to contain particles of calcareous earth, which appear to be situated between the laminæ of felspar; at least, if a piece of it be broken off, and put into nitric acid, a slight effervescence is produced, which however is but momentary. These particles are most numerous, at those parts where the felspar and the metallic substance already described come into contact with each other.

I have placed a specimen of each of these stones in Mr. GREVILLE's collection.

Notwithstanding there is a considerable difference in the nature of the matter which may be considered as the basis of these two pieces, yet the particular nature of the substances contained in them, which are perfectly similar to each other, seems to render it highly probable that the place of their origin was the same. But it also appears probable, from every circumstance respecting these stones, that they must have come, not from a mass of rock of the same nature as themselves, but from some veins, to the destruction of which may also very likely be owing the great quantity of spinelles contained in the sands of certain rivers of Ceylon. Would it be hazarding too much, to suppose that the crystals of perfect corundum which are found in this sand have also the same origin; and that (being much more rarely met with, and in much less quantity,) they have only a partial existence, or one that is confined to certain parts of the veins already spoken of. The small portions of felspar, and also of calcareous spar, which are sometimes, although very rarely, found in this sand, (perhaps because the sand has been already freed from such substances,) tends to support the supposition I have just made, namely, that these two substances are among those which compose the real matrix of the stones here treated of.

Tourmalin. This substance is also frequently found in the sand of Ceylon: indeed it is in this sand that the most perfect crystals of tourmalin, the most transparent, and the most various in colour, are generally found. It is certainly to be lamented, that these crystals are seldom of any considerable size; but that defect is compensated by the perfection and regularity of their form. Among these, I have found two in particular, of which, as they have not hitherto been noticed, I think it right to give a description.

The first of these forms, is the very obtuse rhomboid which is represented in Fig. 52, and is the primitive crystal of this substance. The Abbé HAUY, who also thinks that this rhomboid is really the primitive form of this substance, appears not yet to have met with it; for he has not placed it at the head of the description of tourmalin given in his mineralogy, as he has done with respect to the other substances of which he has observed the primitive form. It is indeed very scarce. I have, however, met with it several times; and have placed a very fine specimen of it in Sir JOHN ST. AUBYN's collection. This crystal, which is about four lines in diameter, and nearly two lines in thickness, is of a brown colour with a tinge of orange; it is also pretty transparent, even in the direction of its axis. Its form is perfectly well defined; and the two pyramids, of which its rhomboid may be considered to be formed, are exactly similar to each other; neither of them having any supernumerary facets.

I think it right here to observe, that there appears to me to have been an error committed, with regard to the measures that have been given as those belonging to the primitive crystal of the tourmalin. The Abbé HAUY fixes the measure of the solid angle of the summit of the pyramid at 136° 54′ 41″. ROME DE LISLE's measure is nearly the same, namely, 137°. I have measured this angle with more than usual care, (on account of my not agreeing with these two celebrated naturalists,) having taken the precaution of using several different goniometers, and I have constantly found it to be 139°; which would make the angles of the rhombic planes 114° 12′, and 65° 48′, instead of 113° 34′ 41″, and 66° 25′ 19″, as stated by the Abbé HAUY.

The second of the forms abovementioned is a prism, either hexaedral, enneaedral, or dodecaedral, of which the terminal faces are perpendicular to the axis. This variety is produced in the following manner, viz. the plane that has replaced the solid angle of the summit of the pyramid, (which plane is represented by the Abbé HAUY in Figs. 119 and 120, Plate LII. of his Mineralogy,) has acquired an increase of sufficient extent to cause the planes of the pyramid entirely to disappear.

I think it right to add here, a variety of this substance, which also comes from Ceylon, and has not yet been described, namely, a prism which has become of a triedral form, with equilateral bases, by the enlargement of the planes that have replaced the three alternate edges; the formation of which planes is known to change the hexaedral prism into an enneaedral one; and the enlargement is such as to cause the six others entirely to disappear. The tourmalins of Ceylon are not the only ones in which I have observed this triedral prism: I have also met with it among the tourmalins of Saxony, and among those of Bohemia.

Lastly, I shall add, as forms not yet described, (although they do not belong to tourmalins of Ceylon,) two complete triedral pyramids, which, if they were not separated by an intermediate prism, would produce two secondary rhomboids, the one more acute, the other more obtuse, than the primitive rhomboid.

The first of these pyramids, is the natural produce of the increase of the planes which have replaced the acute angles of the rhombic planes of the primitive crystal: these planes are represented at the letter *o*, in Figs. 114, 115, 116, and 121, Plate LII. of the Mineralogy lately published by the Abbé HAUY. This learned mineralogist has indeed represented a considerable increase, but not a complete one, of the above-mentioned planes, in Fig. 121, which he says was communicated to him by Mr. LA METHERIE. From the appearance of this form, I think it probably belongs to the tourmalins of Regensberg, in the Upper Palatinate; for many crystals of tourmalin from that place exhibit, at one of their extremities, the pyramid represented at Fig. 121 of the work just mentioned, and the pyramid I have here described, at the other. This triedral pyramid measures 107°, at the solid angle of its summit.

The second of the pyramids, is produced by the increase of the planes which have replaced the edges of the pyramids of the primitive rhomboid: these planes are represented by the Abbé HAUY at letter *n*, in Figs. 118, 119, and 120, also of Plate LII. The triedral pyramid which these planes produce, after having caused every trace of the planes of the primitive rhomboid entirely to disappear, has, very nearly, 159° for the measure of the solid angle of its summit. I have seen this variety among the tourmalins from the Ural mountains, in which, very often, the solid angle of their summit is replaced by a plane, of greater or less extent, which is perpendicular to their axis.

Among the various colours exhibited by the tourmalins which are found in the sand of Ceylon, there are three which deserve notice, because they have not yet been mentioned by any author; these are, a light yellow, like the colour of honey, a beautiful clear emerald green, and a red slightly inclining to purple. The green variety, which indeed might easily lead to a false idea of the stone, is, most probably, what has caused some authors to mention the true emerald as being indigenous to Ceylon, where, hitherto, no trace of that stone appears to have been met with. This error was the more likely to be committed, as it was not then known that the regular hexaedral prism, with terminal faces perpendicular to the axis, was one of the crystalline forms belonging to the tourmalin; and that tourmalins of a beautiful emerald green colour, and perfectly transparent, were sometimes met with of that form. I have placed some very pretty small crystals of this kind in Mr. GREVILLE's collection.

The tourmalin of a purplish red colour, found in the sand of Ceylon, is exactly similar to that of Siberia, to which the names of *rubellite*, of *daourite*, and of *Siberite*, have been successively given, and which the Abbé HAUY has ultimately distinguished by the name of *apyrous tourmalin*. Its form is precisely the same as that of the tourmalin, properly so called; nor does the measure of its angles exhibit any difference; especially if that measure is taken upon crystals which are of a perfectly determined form, and which have not, upon their pyramidal planes, any aggregation that can cause a change in the form of those planes. I have placed in Mr. GREVILLE's collection, a small group of this kind of tourmalin, from Ceylon, the colour of which is a beautiful red; among its crystals, which have triedral pyramids with rhombic planes, may be observed one that has a dodecaedral prism, with its terminal faces perpendicular to its axis. In Sir JOHN ST. AUBYN's collection, I have placed a detached crystal, which has also a dodecaedral prism; one of the extremities of this crystal is of a green colour.*

Lastly, I have, in this same sand, met with a crystal, perfectly colourless, the prism of which is completely triedral;

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* The scarcity of the red tourmalin of Siberia, which hitherto has been known only by very small specimens, for which the dealers demand an extraordinary price, seems to be what has hitherto prevented naturalists from forming a decided opinion respecting its proper place in the system of minerals. I am therefore happy in announcing, that there is in Mr. GREVILLE's collection, a specimen of this kind of tourmalin, (from India,) the size and perfection of which are truly admirable. This specimen, which is not accompanied with any kind of matrix, is nearly as large as a man's head; and is entirely composed of crystals placed by the side of each other, in a diverging form, or rather penetrating each other at one of their extremities, and separating or diverging a little at the other extremity. Every one of these crystals, most of which are as long as the height of the specimen, is nearly as thick as the little finger. Their form is a hexaedral prism, which is deeply striated, and terminated by a triedral pyramid with rhombic planes, the angles of which, measure exactly the same as those of the corresponding pyramid in the common tourmalin. All the crystals are pretty transparent; and terminate on the top of the specimen, by the forementioned pyramids, but at different heights; a circumstance that gives to the top also a triedral pyramidal form, but much less obtuse than that belonging to each crystal of which it is composed. The greatest part of this specimen is of a pale purplish red, or flesh colour; but, towards the base, this colour grows much more deep, so that, at last, it becomes absolutely black. I have observed the same division of colour, in specimens of this red tourmalin from Siberia.

The superb specimen here described was brought from the kingdom of Ava: it was given by the sovereign of that country, as a present of very great value, to Colonel SYMES, who was sent on an embassy to him, by the English government. Colonel SYMES placed it in Mr. GREVILLE's collection; and he could not possibly make a better use of it; that collection being, in my opinion, one of the finest in Europe, with respect to the beauty of the specimens and the instructive series of each substance which composes it, and certainly superior to all others, with respect to precious stones in a state of perfect crystallization.

The Abbé HAUY, in his Mineralogy, expresses a wish, that the prismatic enneaedral form, terminated by the triedral pyramid of the primitive rhomboid, (which he and the pyramidal planes of which, in the only extremity of the crystal that remains, are situated upon the edges of the prism.

Ceylanite. The stone called Ceylanite, by Mr. LA METHERIE, who is the first author that has considered it as a particular and distinct species, (distinguished by the name of *pleonaste,* in the Mineralogy of the Abbé HAUY,) is also sometimes found in the sand of Ceylon; it is, however, in general, by no means common. Of the crystals of this substance that I have collected from this sand, many are perfectly transparent; a character which appears to have been hitherto unobserved in it. Its colours are very various. Besides black and green, which have already been mentioned by authors, I have seen it of a reddish or flesh colour, with a yellowish cast; of a fine bluish green, like the aqua marine; and of a fine sky blue, rather pale. When the Ceylanite is of the last-mentioned colour, whether it be a fragment or a flattened octaedron, it might very easily be mistaken for a sapphire. Its most usual colour is a brownish green.

As this substance has, in all its external characters, a striking resemblance to the spinelle, of which it is perhaps only a species, I think I cannot be too particular in pointing out those characters which may in some measure serve to distinguish it; I shall therefore add, that its hardness is rather inferior to that of the spinelle, the Ceylanite being scratched by the spinelle, while the latter cannot be scratched by the Ceylanite; also, that it usually exhibits, by irregular striæ, parallel to the edges of the regular

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calls *isogone,*) may be met with in this substance, in order to determine its nature. He will no doubt feel satisfaction in hearing, that there exists, in the collection of Sir JOHN ST. AUBYN, a small detached crystal of this substance, of a fine red colour, which has exactly the above-mentioned form. This crystal I found in the sand of Ceylon.

octaedron, its primitive crystal, a tendency to the replacing of all those edges; an appearance which is very common in the octaedron of the diamond. I shall remark also, that the surface of its crystals has generally less lustre than is commonly observed in the crystals of spinelle.

The desire of contributing every thing in my power, to render as complete as possible our knowledge respecting this substance, which has been but lately known to mineralogists, induces me to add to the variety of forms that have been described by the Abbé Hauy, those represented in Figs. 53 and 54, although the Ceylanite to which those figures belong comes from a different place. The first is nothing more than the modification represented by the Abbé Hauy in Fig. 104, Plate L, of his work, but in which the four planes that have replaced each of the solid angles of the octaedron, are situated upon these same angles, in the primitive crystal itself, instead of being situated upon the planes that have replaced the edges. I have frequently seen these planes encroach upon each other, to such a degree as to render it very probable that there exists, in the Ceylanite, that form of crystal which consists of 24 trapezoidal facets, and which, by its derivation from the cube, the regular octaedron, and the regular dodecaedron, is already so very common in crystallography.

The second of the forms just spoken of, (Fig. 54.) is the same variety, but with a very slight replacement of the edges of the octaedron: it is the beginning of the change to the abovementioned Fig. 104, of the Abbé Hauy. These two varieties belong to the Ceylanite which is inclosed in pieces of stone brought from Somma; and are indeed the most common varieties found in them, except that in which the edges only are replaced.

Zircon. This substance is, next to the spinelle, that which is most frequently found in the sand of Ceylon. It is true, that it is generally in crystals of a very small size; but these crystals often possess the most beautiful transparency, and they are of many different colours. To the colours already mentioned as belonging to them, I may add, that they are sometimes of a reddish purple, and sometimes of a pale blue.

Lastly, if to the substances which have already been described, I add, that there are also some small scattered fragments, but in very inconsiderable quantity, of quartz, of felspar, of calcareous spar, of a brownish yellow mica, and of attractable oxide of iron, I shall have enumerated all the substances that are found in the sand of Ceylon, in the state in which it is sent to us. I have always been astonished at not finding in it any of the peridot, which, as is well known, also comes from Ceylon: hitherto, however, I have not perceived the smallest trace of it.

Of the various substances that have been here described, the spinelle is that which more particularly constitutes the sand of Ceylon, such as it comes into Europe; but it is natural to suppose, as I have already had occasion to observe, that the sand has been previously examined, and deprived of every substance, except those which are found by experience to be fit for the purposes of commerce. The other substances above mentioned, are not so constantly found in it, nor are they found always in any regular proportion. I have seen, for instance, some of this sand which did not contain an atom of perfect corundum; other parcels which contained only a very small quantity; and others in which the proportion of that substance was pretty considerable: the same remark may be applied to every one of the other substances. It is therefore, I think, fair to conclude, from the above circumstances, that these sands come from different rivers or rivulets, or, if from one river only, from one into which other rivers discharge themselves; and that the nature of the sand varies, according to the particular circumstances which may have caused one or more of those rivers to bring down a greater, and others a less proportion, of the substances of which it consists. It may indeed also be asked, if what is called the sand of Ceylon comes exclusively from that island? To this question, I can give no decisive answer. I shall only observe, that the length of time it has gone under that denomination, without any alteration, gives some reason for thinking it has really some claim to it.

It is, at this time, a doubtful point, whether corundum is found in any part of the world, besides certain districts of the East Indies; although, as will presently be seen, I have strong reasons for thinking that it also exists in one of the mountainous provinces of France.

I have seen many specimens which were sent from Germany, under the name of corundum; some of them were nothing more than felspar of a brownish red colour; others were the stone called *schorlartiger beryl*, by WERNER, (the *pycnite* of the Abbé HAUY,) but in pieces which were rather less striated than is usually the case with respect to that stone.

It was thought, for some time, that a stone found at Tiree, on the eastern coast of Scotland, was of the nature of corundum.

But, after examining a specimen of that stone, which is in the British Museum, I found that its hardness, and its specific gravity, were both very inferior to those of corundum. In its exterior appearance, it very much resembles the felspar that accompanies the imperfect corundum from the Carnatic, and which I have already described, when speaking of the substances which accompany that kind of corundum in its matrix.

It is also said that corundum has been found in America, at Chesnut Hill, near Philadelphia. But there are, in the Philosophical Magazine, No. 45, for February last, some observations made by Mr. RICHARD PHILIPS, upon the external characters of the American stone, intended to show that it cannot possibly be corundum. Mr. PHILIPS has since told me, that the specimen upon which his observations were founded, was sent to him directly from Philadelphia, as a piece of the corundum found near that city. He also recalled to my mind, (which I had entirely forgot,) that he had shown me the specimen some time before; and that I then gave it as my opinion, that the crystal it contained, supposed to be corundum, was nothing more than an ill-defined crystal of quartz. Nevertheless, Mr. SMITH, a well-informed mineralogist, from America, has since assured me of the truth of the discovery of corundum, in the neighbourhood of Philadelphia. In that case, there must have been some mistake respecting the specimen that was sent to Mr. PHILIPS. Upon the whole, there still remains some uncertainty with regard to the existence of corundum in the neighbourhood of Philadelphia; and it is necessary, in order to remove all doubt on this head, either that some of the substance should be sent to us, or that some mineralogist in that country should give such an accurate description of its characters as may serve to ascertain its real nature.

It remains for me to speak of the corundum I formerly found, or at least thought I found, in Forez, in the mountainous parts of that province which are near Montbrison. I find, by the Mineralogy of the Abbé HAUY, (Vol. IV. p. 362.) that the substance I had considered as corundum, is now looked upon in France to be of a different nature. That learned mineralogist, in the abovementioned work, seems inclined to consider it as a species of felspar, and gives it the name of *apyrous felspar.* He admits however, at the same time, that it scratches quartz; that its specific gravity is 3165; and that it is infusible by means of the blowpipe. All these characters seem to place it at a considerable distance from felspar.

The total loss of a very considerable collection of minerals, intended expressly for the purposes of study, (and which I regret the more from its having been entirely formed, and most of the specimens collected in their native places, by my own hands,) leaving me no objects of comparison, I can only consult, with regard to the above substance, the few notes I have been so fortunate as to preserve, assisting them with such circumstances as my memory has been able to retain.

I find in my notes,

First, That this substance was inclosed in a yellowish felspar, which formed a small vein in a granite rock; that, in some parts of the felspar, it appeared in the form of small spots, easily distinguishable by their colour, which was red with a purplish tinge; and that, in other parts, it was in masses of a rather larger size, from which I was able to extract some fragments.

Secondly, That the appearance of this substance was entirely different from that of felspar; and that, where it came in contact with the felspar, it seemed to mix itself with it in such an insensible manner, that, after having sawed and polished a piece composed partly of felspar and partly of the substance here spoken of, it was impossible, by the eye, to distinguish exactly where the felspar began, or, which is the same thing, where the other substance terminated.

Thirdly, I find also by my notes, that the pieces I had collected, varied considerably in their degree of hardness, although all of them were harder than felspar usually is; for many of these pieces would scarcely scratch felspar; whereas others could scarcely be scratched by the greatest number of gems or precious stones. The characters of the last mentioned or hardest pieces, appeared to me to be very similar to those of the imperfect corundum from China, a crystal of which Romé de Lisle had sent me a short time before. The above observations, joined to the remarkable manner in which this substance is mixed with felspar, made me adopt the erroneous opinion mentioned by the Abbé Hauy, in his observations upon corundum, namely, that this substance might be nothing more than a more dense variety of felspar. I soon, however, entirely gave up this idea, after I had it in my power to examine more particularly the nature of corundum.

Fourthly, and lastly, I find by my notes, (and I also remember it perfectly well,) that among the pieces I was able, by patiently and carefully using the tools employed for that purpose by mineralogists, to extract from the vein above mentioned, there were some to which adhered small irregularly shaped pieces of a substance that was perfectly transparent, and had a fine sapphire blue colour. The hardness of this substance was such as to be equalled only by that of the sapphire itself; and, in some of the pieces, instead of adhering to the outside, it was dispersed, in very small particles, within the interior part.

As I cannot, even at this time, consider this blue substance as any thing else than the blue perfect corundum known by the name of sapphire, I still retain the opinion I formerly thought it right to adopt, namely, that the substance to which it adhered, and which I found in the province of Forez, was really a kind of corundum. I still think also, that the variety I observed in the degree of hardness, and in the specific gravity, of different pieces, was owing to their being mixed, in various proportions, with felspar. If it should happen that, among the remains of a collection of which nothing is left to me but a painful remembrance, (although, as I have before said, my present situation is such as much alleviates my regret,) any of the specimens above spoken of still exist, and should fall into the hands of well informed naturalists, I hope they will let them serve as a basis for fresh observations. The description of the Abbé HAUY is alone sufficient to show, that the above substance cannot possibly be a kind of felspar. I am sorry, however, that he did not join to his description, the analysis of the substance; it certainly would have been very interesting, particularly if, as would most probably have been the case, the hardest pieces had been selected for that purpose.

The great difference sometimes observed in different specimens of the same substance, is exhibited in a very striking manner, in the emeralds which I found, at the same period, in a large vein of the fore-mentioned rock, but which was situated in the part of the rock opposite to that wherein I discovered the blue substance already described. The Abbé HAUY, in his Mineralogy, (Vol. IV. page 361,) mentions these emeralds, but expresses some doubts respecting them. These doubts I think would be removed, if I had it in my power to send him the specimens I then collected. Among them were some crystals, which possessed a degree of hardness fully equal to that which is known to belong to the emerald: the hardness of many others was, however, very inferior; owing no doubt to the interposition of some heterogeneous substance, which I always suspected to be of a magnesian nature.

The Abbé HAUY, in order to fix his opinion respecting this substance, appears to require nothing but to see some crystals of it which possess the additional facets peculiar to the true emerald. I cannot indeed shew him such crystals; but I can supply the want of them, not only by my notes, but also by models cut in wood, which I was so fortunate as to bring away with me, as well as the whole collection of models of which they form a part. I find, among the models I made of these emeralds from Forez, all the varieties the Abbé HAUY has represented in Plate XLV. of his work, excepting only Fig. 50. of that Plate.

Fig. 2.

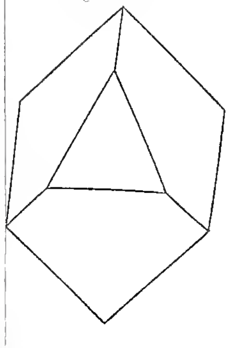

Fig. 3.

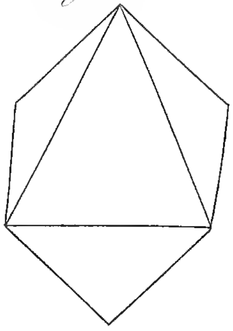

Fig. 4.

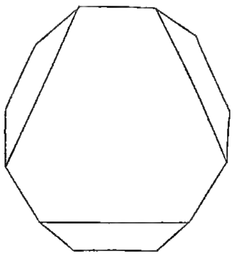

Fig. 6.

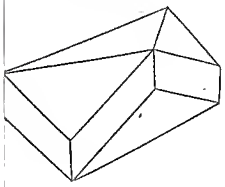

Fig. 7.

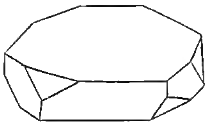

Fig. 8.

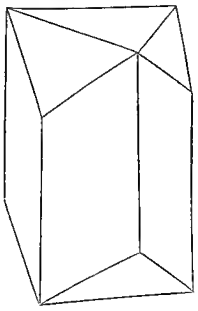

Fig. 10.

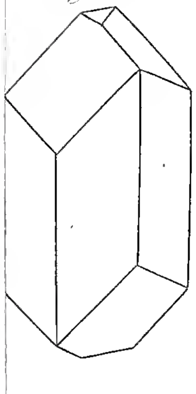

Fig. 11.

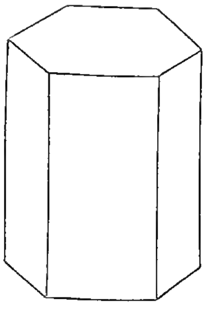

Fig. 12.

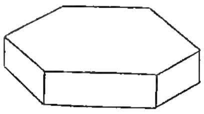

Fig. 14.

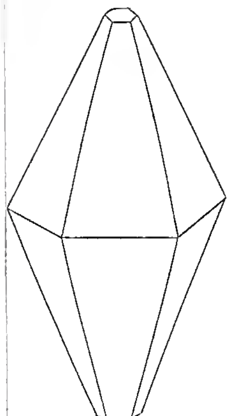

Fig. 15.

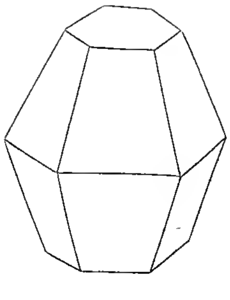

Fig. 16.

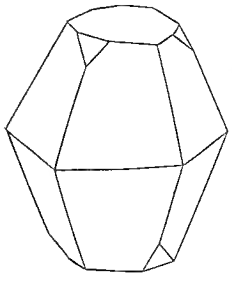

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Fig. 1.

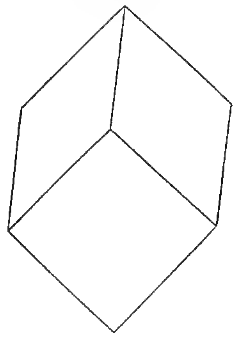


Fig. 2.

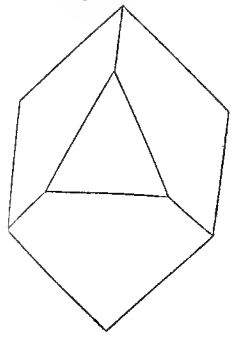
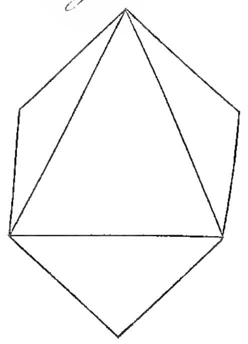


Fig. 3.



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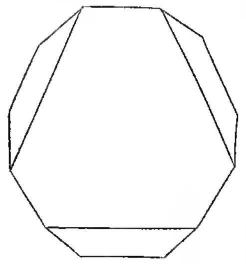

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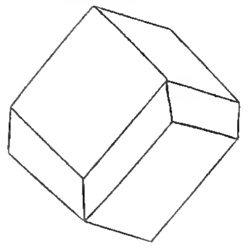

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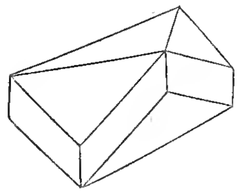

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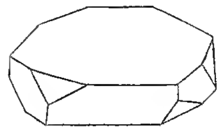

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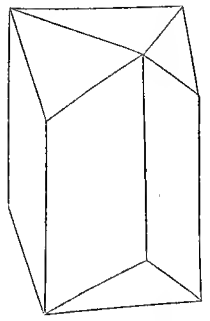

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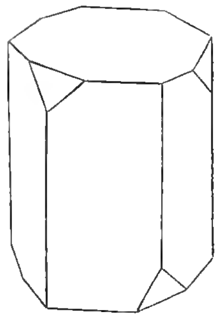

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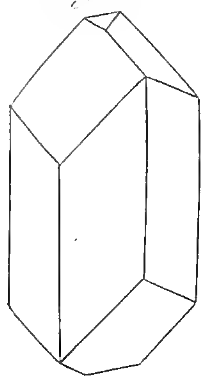

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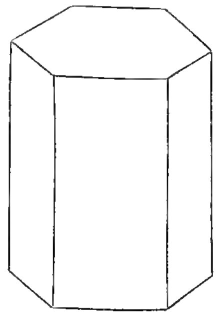

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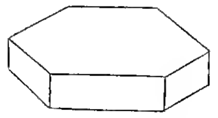

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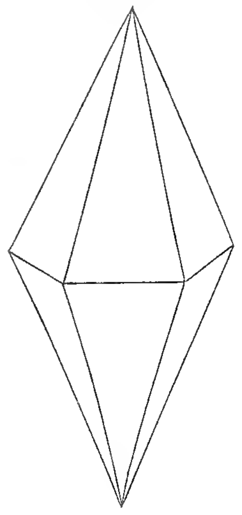

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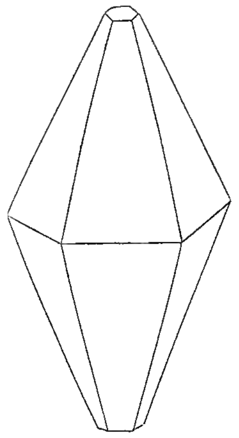

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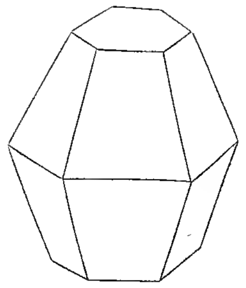

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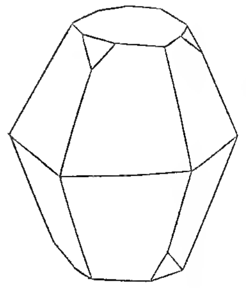

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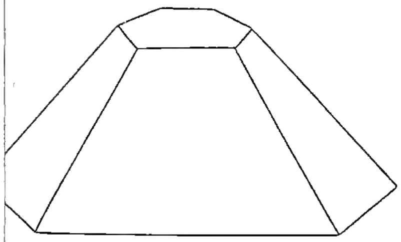

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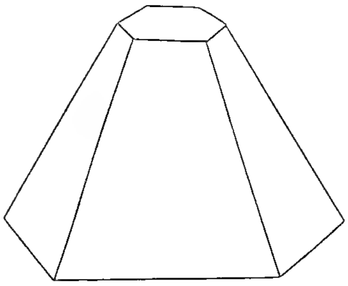
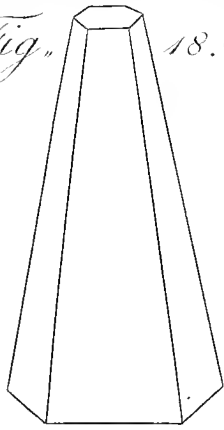


Fig. 18. C



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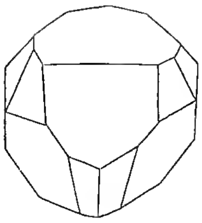

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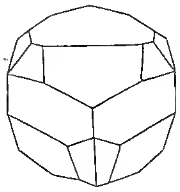

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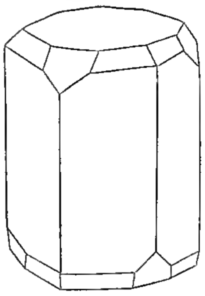

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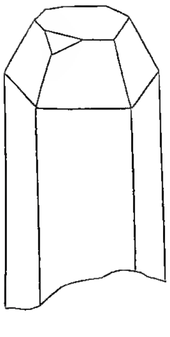

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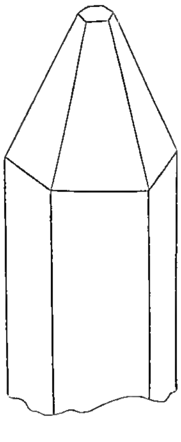

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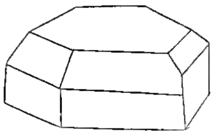

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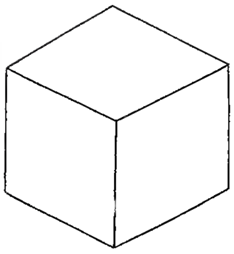

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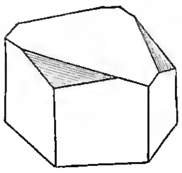

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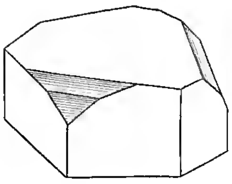

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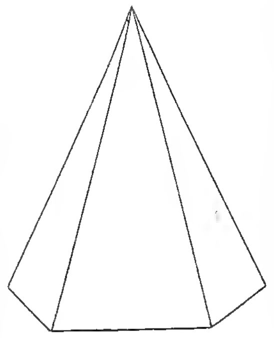
Philos. Trans. MDCCCII. Plate VII. p.326

Fig. 17.



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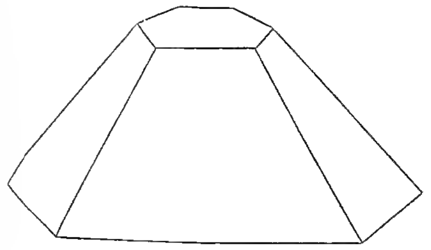

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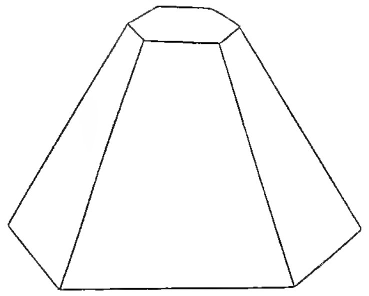

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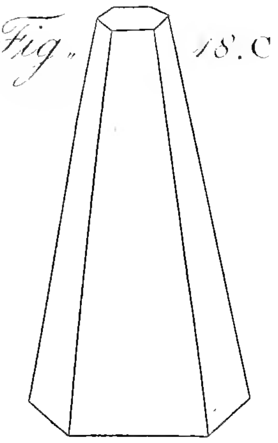

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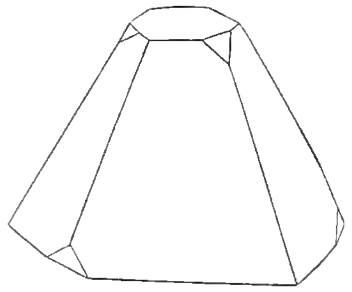

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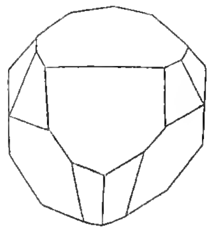

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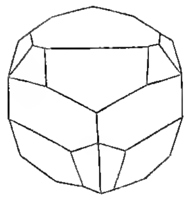

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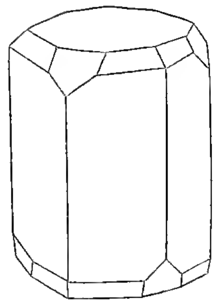

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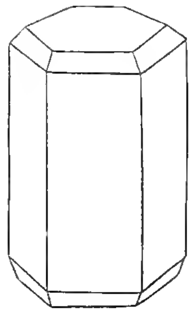

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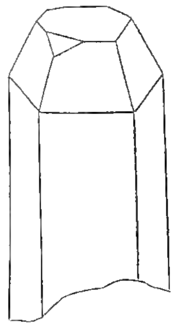

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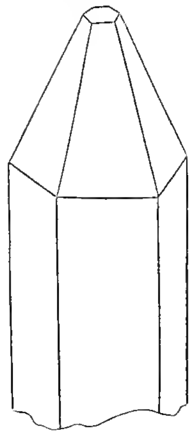

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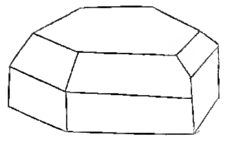

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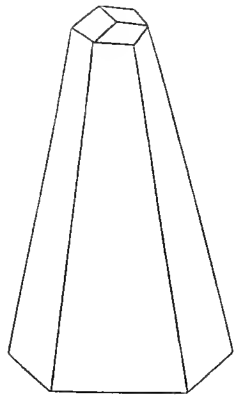

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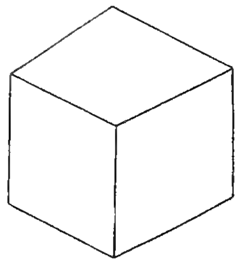

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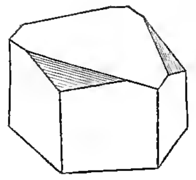

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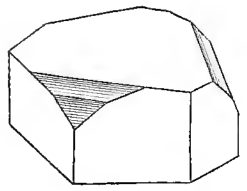

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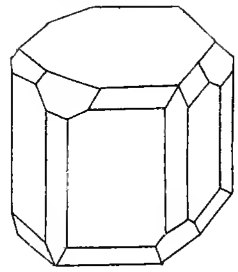

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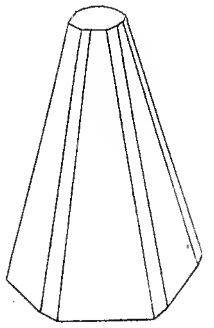

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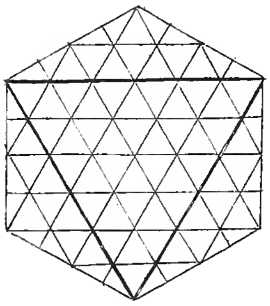

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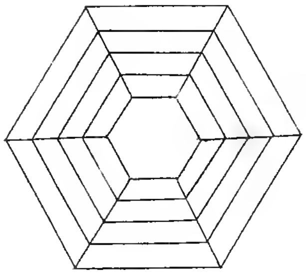

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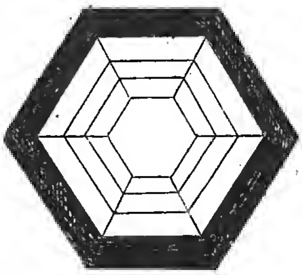

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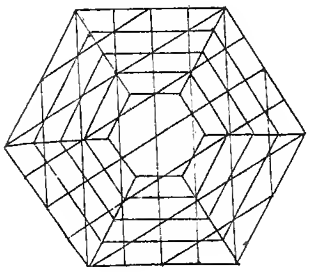

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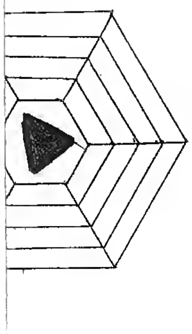

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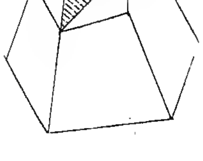

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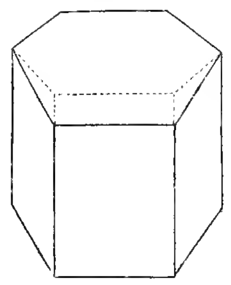

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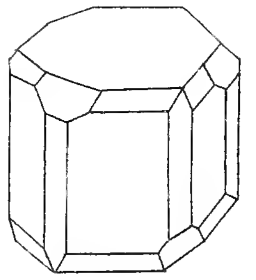

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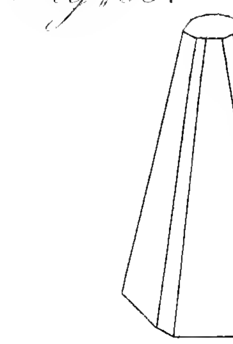

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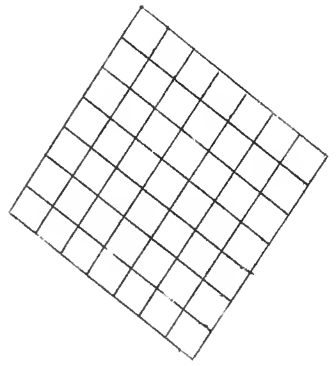

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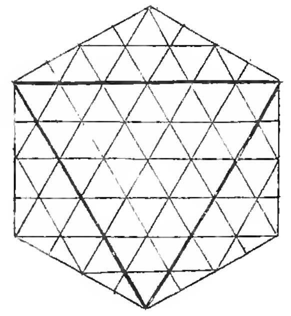

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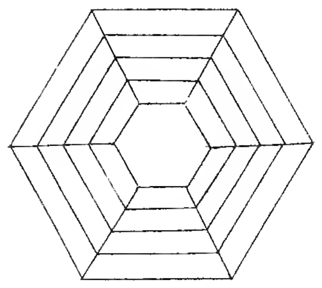

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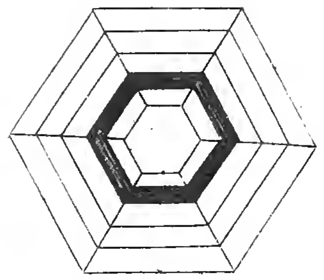

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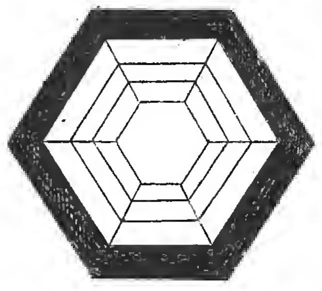

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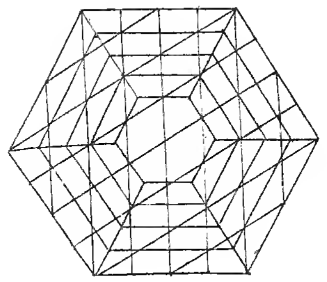

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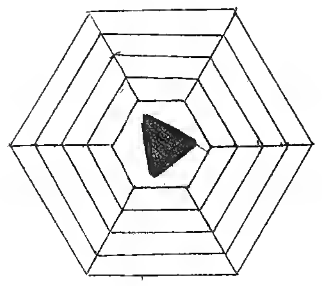

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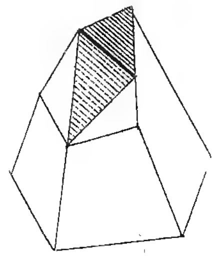

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Fig. 42.

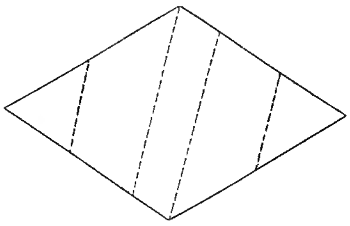

Fig. 43.

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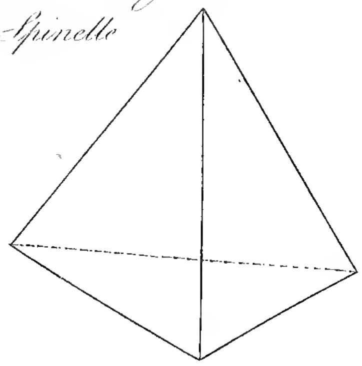

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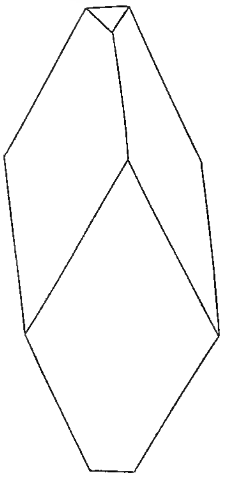

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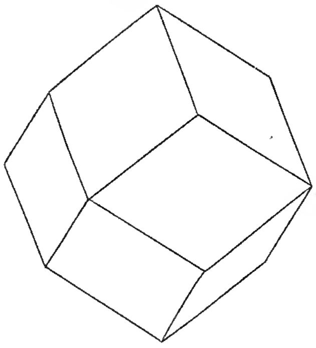

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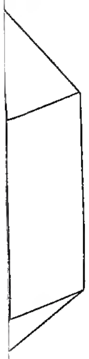

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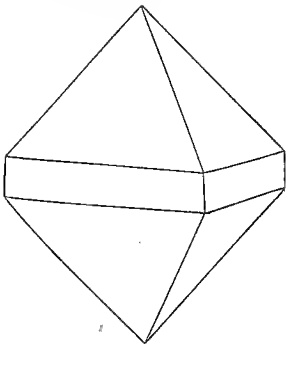

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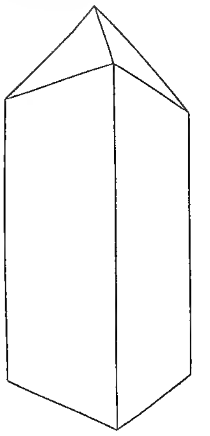

Fig. 53.

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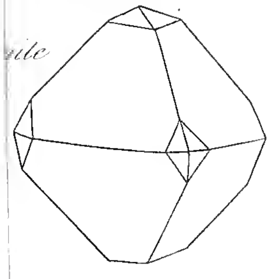

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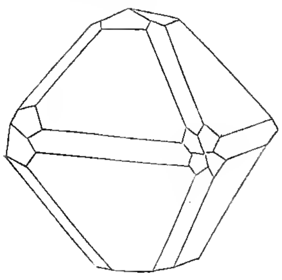

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Fig. 40.

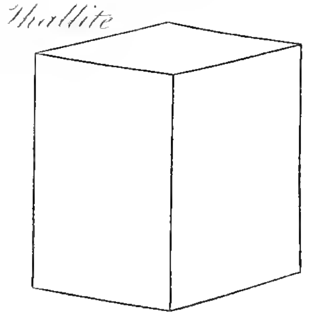


Fig. 41.

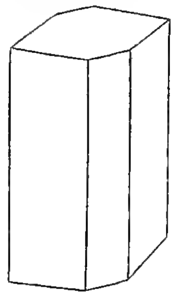


Fig. 42.

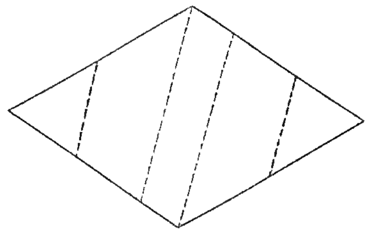


Fig. 43.

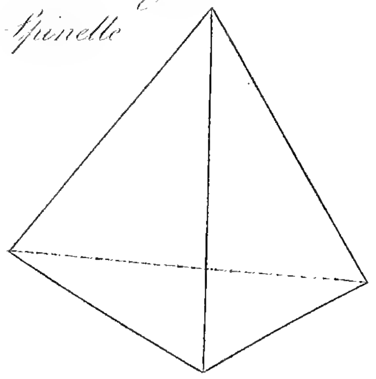
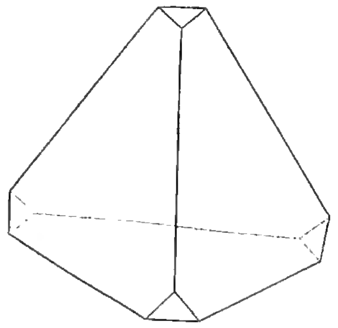


Fig. 44.



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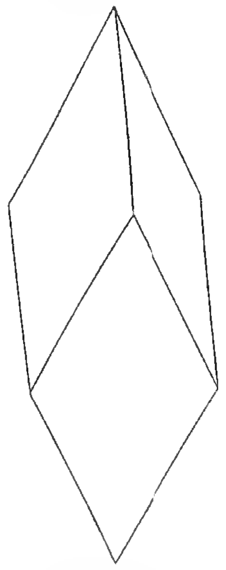


Fig. 46.

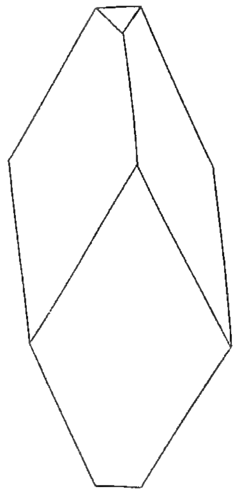


Fig. 47.

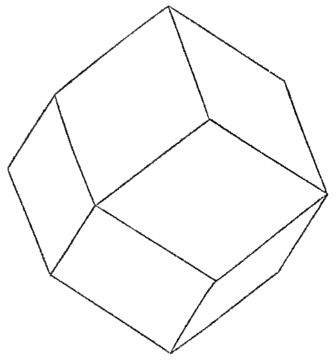


Fig. 48.

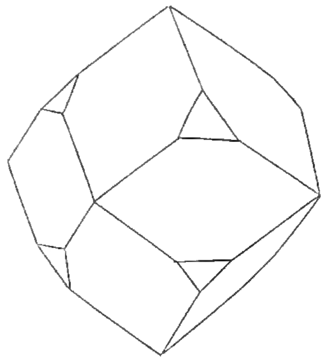


Fig. 49.

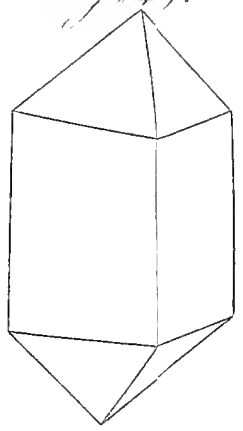


Fig. 50.

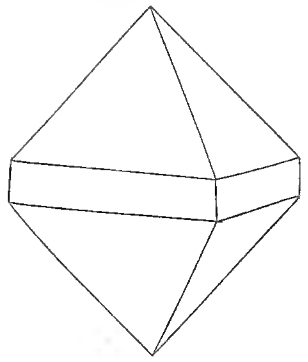


Fig. 51.

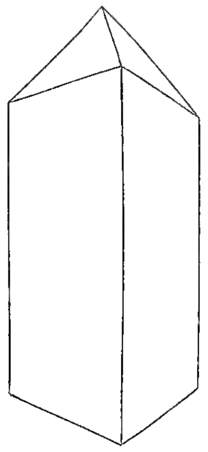


Fig. 52.

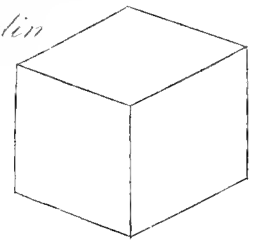


Fig. 53.

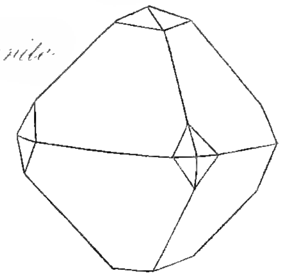
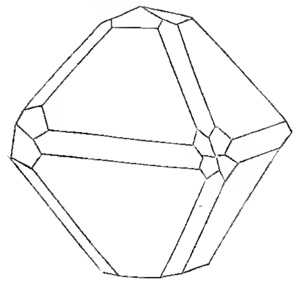


Fig. 54.



J. Basire sculp.

X. *Analysis of Corundum, and of some of the Substances which accompany it; with Observations on the Affinities which the Earths have been supposed to have for each other, in the humid Way. By* Richard Chenevix, *Esq. F. R. S. and M. R. I. A.*

Read May 20, 1802.

§ I.

Some kinds of corundum, such as the adamantine spar of China, and the sapphire, have already been analyzed by Mr. Klaproth. This would have rendered any further experiments unnecessary, were it not, that I have had at my disposal many kinds of corundum he did not possess, and also some substances accompanying it, which were unknown before the preceding communication of the Count de Bournon.

As, from the result of my analyses, it appears that all the different kinds of corundum are nearly similar in their constituent parts, and differ only in their proportions, it would be tedious to mention every experiment I made upon each kind. I shall therefore confine myself to stating, once for all, such modes of analysis as were employed with stones of a similar nature; and then present a summary of the results: lastly, I shall conclude with an enquiry into a much contested point, which lately threatened a revolution in docimastic chemistry.

A principal character of corundum in general, as may be found in the Count de Bournon's mineralogical description, is extreme hardness; and thence, the difficulty of reducing that substance into fine powder will be easily conceived. We are told by docimastic chemists, that the most advantageous method of pulverizing hard stones, is to make them red hot; and, in that state, to plunge them into cold water. But I found that this operation, when performed but once, was by no means sufficient for corundum. I therefore repeated it, till the stone appeared to be fissured in every direction. After this, the specimen to be pulverized was put into a steel mortar, about three-fourths of an inch in diameter, and three inches in depth, into which a steel pestle was very closely adjusted. A few blows upon the pestle caused the stone to crumble; and the fragments were then easily reduced into an impalpable powder, in an agate mortar, with a pestle of the same material. The abrasion from the mortar, usual in the pulverization of hard stones, was much diminished by the above precaution; rubies and sapphires being, in a short time, ground to a powder nearly as minute as the finest precipitate.

Mr. Klaproth, in his analysis before mentioned, had observed with how much difficulty the stones were acted upon by potash or soda. I found that the greatest heat a silver crucible could support, without melting, was not sufficient to produce a satisfactory fusion of one part of corundum, with six parts of either of those alkalis; nor did an exposure to that temperature during several hours, seem to render the treatment more effectual. Not more than half the quantity of the corundum was ever rendered soluble in any acid; and what remained was the powder of the stone, wholly unchanged. The repeated filtrations and evaporations with which this treatment must be attended, not only render it tedious, but also produce uncertainty in the results. Even when very finely powdered corundum was exposed, with six times its weight of potash, in a platina crucible, to a heat of 140° of Wedgwood, for two hours together, it was not acted upon in such a manner as to be fit for analysis. From all these experiments I concluded, that some more efficacious mode of rendering corundum soluble in acids was to be sought.

I boiled a great quantity of sulphuric acid upon very finely powdered corundum, in a platina crucible. But, although the acid, after a great length of time, had dissolved a little of the stone, I did not find this method more satisfactory than the others. Nitric, muriatic, and nitro-muriatic acids, were less effectual than the sulphuric. Phosphoric acid, held in fusion with corundum, did not dissolve any notable portion of that stone, or render it soluble in other acids.

I then had recourse to sub-borate of soda, (borax,) which I found to answer beyond my expectation. Two parts of that salt, calcined, and one of corundum, enter into fusion, at a temperature which I judged to be about 80° of Wedgwood;* and a glass, more or less coloured, is formed. This glass is soluble in muriatic acid; and, by this method, it is easy to obtain a complete solution of corundum. My general method of operating was as follows.

I took one hundred grains of corundum; and, having several times made it red hot, and plunged it into cold water, I put it into the steel mortar, and treated it as already mentioned. I then poured some very dilute muriatic acid upon it, to wash off whatever iron might have adhered, in consequence of its mechanical action upon the mortar. After it was dried and weighed,

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* I have no doubt that a lower temperature would be sufficient.

I put it into the agate mortar, and ground it as fine as I could. The augmentation of weight was then noted; and was always taken into account in the general result. I then put the whole into a platina crucible, with 200 grains of calcined sub-borate of soda, and exposed the mixture for an hour or two to a violent heat. When the crucible was cool, muriatic acid was boiled upon it and its contents; and, in about twelve hours, all the glass disappeared. If I wished to obtain the silica directly, I evaporated the whole to dryness; but, if otherwise, I precipitated by an alkaline carbonate, and washed the precipitate, in order to get rid of all the salts contained in the liquor. This latter mode I believe to be preferable. I then re-dissolved the precipitate in muriatic acid, and evaporated for silica. But, as corundum contains only a small portion of this earth, there was little or no appearance of jelly. When the silica was thus precipitated by evaporation, I filtered the liquor, and boiled it with an excess of potash. By this operation, the alumina was precipitated, and then re-dissolved by the excess of potash, from which it was finally obtained by muriate of ammonia; the iron which had remained undissolved by the potash, having of course been previously separated from the alumina. This earth, and the silica, after being washed and dried, were ignited, and thus the weight of both was obtained.

I shall exemplify, in a single instance, this mode of treatment; and then present the results obtained from the different kinds of corundum. For this purpose, I shall select the blue perfect corundum, or sapphire, as the stone which has been the most ably analyzed by Mr. KLAPROTH. From a view of both analyses, the efficacy of the fusion with borax will be evident; and the results of the several experiments may be compared.

1. 100 grains of sapphire, pulverized in the agate mortar, as above stated, had increased to 105. These 105 were mixed with 250 of calcined sub-borate of soda, and put into a platina crucible. They were then exposed to a violent heat for two hours, and afterwards allowed to cool. The mass was vitrified, and had the appearance of a greenish blue glass, fissured in many directions.

2. This glass being strongly attached to the platina crucible, the whole was put into muriatic acid, and boiled for some hours. By these means, a total and limpid solution was obtained.

3. The matter of the stone was next precipitated, by ammonia not entirely saturated with carbonic acid; the liquor was filtered; and the precipitate well washed and dried. It was then redissolved in muriatic acid, and evaporated.

4. By this evaporation a precipitate was formed, which, when well washed and ignited, weighed 10,25 grains, and was silica.

5. The liquor, together with that which had washed the precipitate, was boiled in a silver vessel, with an excess of potash; this redissolved all the precipitate, except one grain.

6. Muriate of ammonia was poured into the alkaline solution (No. 5.) The potash expelled the ammonia from the muriatic acid, and, forming muriate of potash, could no longer retain the earth in solution; a very copious precipitate, therefore, was formed. This precipitate had all the properties of alumina; and, when well washed and ignited, weighed 92 grains. Consequently, deducting 5 from the silica, for the abrasion of the mortar, we shall have for result,

Silica - - - -	5,25
Alumina - - - -	92
Iron - - - -	1
Loss - - - -	1,75
	100,00.

The chief difference between these proportions and those established by Mr. KLAPROTH, is in the silica. That chemist did not find any notable portion of it in the specimens he examined. This naturally induced me to make a very strict research, into every possible means by which any silica might have been introduced into the results; whether by the borax, the alkali, or any of the other re-agents I had used. But, finding very clearly, that none of these substances did contain any, I could no longer hesitate to believe, that the proportion I have here stated, was actually contained in the sapphire I analyzed. I am likewise convinced, that no more than the quantity I have mentioned was worn from the agate mortar and pestle; for my constant practice was, to weigh them, both before and after I had used them, in scales which, when charged with four pounds on each end, turn easily with the tenth part of a grain.

The general results, from all the different kinds of corundum, were as follows.

Blue perfect Corundum, or Sapphire.

Silica - -	5,25
Alumina - -	92
Iron - - -	1
Loss - -	1,75
	100,00.

Imperfect Corundum from the Carnatic.

Silica - -	5
Alumina - -	91
Iron - -	1,5
Loss - -	2,5
	100,0.

Red perfect Corundum, or Ruby.

Silica - -	7
Alumina - -	90
Iron - -	1,2
Loss - -	1,8
	100,0.

Imperfect Corundum from Malabar.

Silica - -	7
Alumina - -	86,5
Iron - -	4
Loss - -	2,5
	100,0.

Imperfect Corundum from China.	
Silica	5,25
Alumina	86,50
Iron	6,50
Loss	1,75
	100,00.

Imperfect Corundum from Ava.	
Silica	6,5
Alumina	87,0
Iron	4,5
Loss	2,0
	100,0.

As I could not discover chrome, or any other colouring substance, except iron, in these stones, I can attribute their difference of colour only to the different state of oxidizement of the iron; but it is impossible to ascertain what that state may be, from so small a quantity.

The matrices of these stones, and the substances accompanying them, are more easily fused than the six kinds of corundum just mentioned. The usual and well known mode of treatment by potash, was sufficient to render these substances soluble in the acids. Since the many experiments of KLAPROTH, VAUQUELIN, and others, the mode of analyzing mineral bodies is become so familiar to chemists, that I shall mention particulars with respect to one only of the following substances.

MATRIX OF CORUNDUM FROM THE PENINSULA OF INDIA.

1. A certain quantity of this matrix was reduced to powder, in the manner already described. 100 grains of it were treated with potash, in a silver crucible: they then afforded a limpid solution in muriatic acid. The liquor was evaporated; and, long before the mass was entirely dry, it had assumed the appearance of a jelly. When the saline matter in the evaporating-dish was dissolved in a slight excess of acid, a white powder remained at

bottom, which had all the properties of silica, and, when washed and ignited, weighed 42,5 grains.

2. Into the liquor which had served to wash the above powder, I poured ammonia. A copious precipitate was thus formed, which was separated by filtration, and well washed.

3. Carbonate of potash also caused a precipitate in the liquor of No. 2. This precipitate was found to be carbonate of lime, and weighed 23,5 grains, = 15 of lime.

4. The precipitate of No. 2. was redissolved in muriatic acid; then boiled with an excess of potash, and filtered. There remained undissolved, 3 grains, which were iron.

5. The liquor of No. 4. was precipitated by muriate of ammonia, and afforded alumina; which, being washed and ignited, weighed 37,5 grains.

I could also perceive a trace of manganese.

The proportions therefore are,

Silica	42,5
Alumina	37,5
Lime	15,0
Iron	3,0
Loss, with a trace of manganese	2,0
	100,0.

By a similar treatment, the following substances, contained in this matrix, afforded the under-mentioned results.

Felspar.

Silica	64
Alumina	24
Lime	6,25
Iron	2,00
Loss	3,75
	100,00.

Fibrolite.

Silica	38
Alumina	58,25
A trace of iron, and loss	3,75
	100,00.

This is the only stone I have ever met with, that yielded nothing but silica and alumina; for the quantity of iron was so small as hardly to be taken into account. I have repeated this analysis three times; and have not found a difference of half a grain.

Thallite in Crystals, with a rough Surface.

Silica	45
Alumina	28
Lime	15
Iron	11
Loss	1
	100.

Thallite in Prisms like the Tourmalin.

Silica	40
Alumina	25
Lime	21,5
Iron	11,5
Loss	2
	100,0.

Thallite in Fragments, of a fine transparent Yellow Colour.

Silica	42
Alumina	25,5
Lime	16
Iron	14
Loss	2,5
	100,0.

Fibrolite accompanying the Matrix of Corundum from China.

Silica	38
Alumina	46
Iron	13
Loss	3
	100.

Felspar from the Sand of Ceylon.

Silica	68,5
Alumina	20,5
Lime	7
Iron	1,5
Loss	2,5
	100,0.

As the greater part of the above substances were fusible without difficulty in potash, I preferred using a silver crucible to any other. It may be laid down as a general rule, with respect to delicate experiments, that in the treatment of metallic substances, we should not use metallic crucibles; but, in the treatment of earthy bodies, they alone are to be depended upon. The easily oxidizable metals cannot be employed; but silver and platina present advantages which no other metals seem to possess.

Theory would certainly give a general preference to platina, from its resistance both to heat and to acids; and practice will justify this preference, in all but a single instance. If a quantity of potash be kept for some time in fusion, in a platina crucible, it will be found that the crucible has lost several grains of its weight. The platina so dissolved may be looked for in the potash; and, if this be saturated with muriatic acid, and evaporated, we shall find the well-known triple salt, formed by the combination of muriatic acid with potash and oxide of platina. This action of potash upon platina, does not depend upon any mechanical cause, such as friction, the force that determines it being purely chemical. If a salt formed by potash, or a salt formed by ammonia, be mixed with a salt of platina, a precipitate ensues, which is a triple salt; and it is by this method, that the Spanish government detects the platina, in the ingots of gold sent from their American possessions. It is therefore evident, that an affinity does exist between potash and platina, in a certain state; and I imagine it to be this affinity, which causes the oxidizement of the platina, when potash is kept in fusion upon that metal. I must however observe, that my crucible was prepared by JANETTY, in Paris, according to a method he has published in the *Annales de Chimie;* and that he always employs arsenic, a little of which certainly remains united to the platina. What influence arsenic may have, remains to be determined. Soda does not form a triple salt with the oxide of platina; for I have frequently kept this alkali in fusion, in a platina crucible, for a long time; yet very little action was produced upon the metal. This fact seems to corroborate my assertion, that the affinity of potash for oxide of platina, determines the oxidizement of the metal.

Whenever I suspected that platina had been dissolved, I could easily detect the smallest portion of it. A solution of platina, so dilute as to be nearly colourless, manifests, in a very short time, the colour of a much more concentrate solution, and becomes reddish, by the addition of a solution of tin in muriatic acid. This I have found to be, by many degrees, the most sensible test for platina; and it would answer the purposes of the Spanish government, much better than that they usually employ.

The alkalis have no immediate action upon silver; but I have observed, that crucibles of this metal, after they have been a long time in use, become somewhat more brittle than they were before.

Potash and soda have long been termed fixed alkalis; and it is certain that, if we compare them with ammonia, they are so. But *fixed* is an absolute term, and cannot admit of degrees. If potash, such as we obtain from Mr. BERTHOLLET'S method of preparing it, be kept in fusion at a very strong heat, it may be totally volatilized. The vapour of the alkali may be perceived in the room; and vegetable colours will undergo the change which is usually produced by alkalis. Indeed, in preparing Mr. BERTHOLLET'S potash, the vapour of the alkali may be easily perceived. Soda is not quite so volatile; though far from being fixed. It appears also, that a little water increases the volatility of both potash and soda, as happens with boracic acid. This volatility of potash, has been advantageously applied of late to the art of bleaching.

§ II.

On the Affinities the Earths have been supposed to have for each other, in the humid way.

In the course of the foregoing analysis, I had occasion to make some further observations concerning a subject upon which I had been formerly engaged, namely, on the affinities the earths have been supposed to have for each other, when held in solution by acid or alkaline menstrua.

In the XXVIIIth volume of the *Annales de Chimie*, page 189, I published a paper upon the analysis of some magnesian stones. In this paper, I took notice of the following affinities of the earths for each other, namely, the affinity of alumina for magnesia, of alumina for lime, and of alumina for silica. In the XXXIst volume, page 246, there is a memoir, by GUYTON DE MORVEAU, upon a similar subject;* and he there reports some experiments of his own, by which he was induced to think, that the earths do really possess a chemical attraction for one another. Since that time, the affinity of the earths has been received among chemists as an undoubted fact; and, at the end of Mr. KIRWAN's *Essay on the Analysis of mineral Waters,* we find a list of earthy salts which produce a reaction upon one another, supposed to be caused by an affinity that tends to unite their bases, in the form of a precipitate, insoluble in the acids. Some other detached observations are to be found, in the *Journal de Physique*, and in the *Annales de Chimie*. The fact is certainly one of the most important in the docimastic art, and merits all the attention of the skilful in that branch.

In the XLth volume of the *Annales de Chimie*, page 52,

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* He has taken no notice of any of the experiments contained in my paper.

DARRACQ has published a paper, intended as a refutation of the conclusions drawn by GUYTON. I had myself repeated the greater part of the experiments of the latter; and the results I obtained were exactly similar to those of DARRACQ. In fact, I had intended to continue the researches; but the very satisfactory paper of DARRACQ, appeared to me to render a further prosecution of them totally useless. However, a paragraph inserted in the *Annales de Chimie*, (Tom. XLI. p. 206.) and of which GUYTON appears to be the author, shows that he has not derived from the Memoir of DARRACQ, that conviction which it certainly conveys. The paragraph in question is founded upon a letter, written from Freyberg, by Dr. G. M. to Dr. BABINGTON, dated December 17, 1800, and inserted in the IVth volume of NICHOLSON's Journal, page 511. This letter contains an opinion which deserves to be canvassed, as it is not perfectly just; and the use GUYTON has made of it, has determined me to add my observations to those of DARRACQ.

I shall follow the order of GUYTON's experiments, in the enumeration of those I made.

Exp. 1. From a mixture of lime-water and barytes-water, GUYTON obtained a precipitate. I obtained none.

Exp. 2. A solution of alumina in potash, mixed with a solution of silica in the same, gave a precipitate, after standing some time. This had been observed by DARRACQ, and by GUYTON, and agrees perfectly with the affinity which, before GUYTON published his paper, I had asserted to exist between these two earths.

Exp. 3, 4, 5. Lime-water, strontia-water, and barytes-water, produce a somewhat similar effect upon a solution of silica in potash.

Exp. 6. No precipitate took place from a mixture of barytes-water and strontia-water; nor from solutions of the carbonates of those earths, in water impregnated with carbonic acid.

Exp. 7. GUYTON obtained a precipitate, by mixing solutions of muriate of lime and muriate of alumina. I could not obtain any.

Exp. 8. Solutions of muriate of lime and muriate of magnesia, when mixed, did not afford a precipitate.

Exp. 9. Muriate of barytes did not, as GUYTON has asserted, form a precipitate with muriate of lime. He was right in saying, that muriate of strontia gave no precipitate with muriate of lime.

Exp. 10. Muriate of magnesia and of alumina, afforded me no precipitate. GUYTON says, that the liquors became milky.

Exp. 11. Muriate of magnesia, whether mixed with muriate of barytes or of strontia, afforded me no change; although GUYTON says he obtained an abundant precipitate, by mixing muriate of magnesia with muriate of barytes.

Exp. 12. Muriate of alumina and of barytes, did not, when mixed together, yield any precipitate. GUYTON asserts, that there is a precipitate in this case.

Exp. 13. Muriate of barytes and of strontia, did not form a precipitate. GUYTON has remarked the same.

Exp. 14. From muriate of strontia and of alumina, I obtained no precipitate. With GUYTON the liquor became milky.

From all these experiments it appears very clearly, that GUYTON has pronounced too hastily, upon the affinity which he supposes barytes to entertain for lime, for magnesia, and for alumina; and that he is equally in the wrong, with regard to the affinity of strontia and alumina. With regard to *Exp.* 3, 4, and 5, although they appear to be true, yet it would require the respective precipitates to be further examined, before we admit a decided affinity between the earths. The quantity of carbonic acid also, which must of course combine with the potash, during the treatment of the silica by that alkali, should be taken into account, in considering the cause of the precipitate.

The solutions which I used, of all the above salts, were in the most concentrate state; therefore, in the state most favourable for showing precipitation, if any had taken place.

It is not very difficult to account for the appearances that deceived Mr. GUYTON in his experiments, and for the cause that produced them. In one instance, he obtained a precipitate from muriate of lime and of alumina, because, in all probability, the alumina he dissolved in muriatic acid had been precipitated from alum; and alumina, thus prepared, retains a small portion of sulphuric acid.* In the next place, it is very likely that his solutions were sufficiently concentrate to give a precipitate of sulphate of lime. The same was the case with regard to his mixture of muriate of strontia with muriate of alumina. As to the general conclusion, that barytes has an affinity for lime, magnesia, and alumina, which strontia does not appear to possess, it is to be explained as follows. Lime often contains a little sulphate of lime. Mr. GUYTON's magnesia, as well as his alumina, had probably been obtained from the sulphate; and we are indebted to Mr. BERTHOLLET, for the true nature of many similar precipitates.

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* It is somewhat singular, that GUYTON should have observed this fact elsewhere. See his experiments on the diamond, in the *Annales de Chimie*. The preparation of a barytic salt, by alumina prepared from the sulphate of this earth, had been observed by SCHEELE, in his *Essay on the Affinities of Bodies*. But that great chemist referred the phenomenon to its right cause, viz. to some sulphuric acid remaining in all alumina thus prepared.

Barytes is a much more delicate test than strontia, for sulphuric acid; and, therefore, barytic solutions were affected by quantities of sulphuric acid, which strontia could not render sensible. This I have ascertained to be the case: for I have obtained copious precipitates, by barytes, in a liquor composed for the purpose, wherein strontia did not produce the smallest cloud, or show the presence of sulphuric acid.

A little care and attention are necessary, in preparing the earths, which are to be dissolved in the muriatic acid, for these experiments; and, if Mr. Guyton had taken the requisite precautions, he would not have been led into error. The object to be kept in view is, to free the earth from sulphuric acid; and, if this be obtained, there is not the smallest precipitate or cloud, in any of the cases I have mentioned. If any further proof be necessary, with regard to the cause of precipitates obtained in the manner stated by Mr. Guyton, I may add, that I have repeated his experiments, and have always found the precipitates to be sulphate of barytes.

The general conclusion to be drawn from the observations of Mr. Kirwan, already alluded to, is, that barytes has an affinity for lime, magnesia, and alumina, upon which earths strontia does not seem to have any influence. But these mistakes are to be accounted for in the same manner as those of Mr. Guyton, viz. by sulphate of barytes being much less soluble than sulphate of strontia, and therefore showing the presence of a smaller portion of sulphuric acid, or, in other words, being a much more delicate test for that substance.

With regard to the letter already mentioned as being inserted in Nicholson's Journal, and which drew some reflections from

Mr. GUYTON, it is necessary to examine as much of it as may be thought objectionable.

The author says, that he repeated the experiments of Mr. GUYTON, with an alkaline solution of silica and alumina, and that he obtained a precipitate; which precipitate, though containing silica, was totally soluble in the acids. "Here," he says, "the properties of the silex must be considerably altered. This "must render all analysis with alkalis suspicious; and shows on "what fallacious grounds the proud dominion of chemistry rests, "which she has exercised so long, in such an arbitrary and over-"bearing manner, in the mineral kingdom." This opinion is by no means likely to overthrow the pretensions of chemistry; for the very circumstance of rendering silica soluble in the acids, is one of the discoveries that has most contributed to render certain, and to extend, our knowledge of analysis. No earthy substance is now thought fit to be submitted to further experiment, till a complete solution of it in an acid be first obtained; and, when that solution cannot be effected directly by the acid, it is always attempted by previous fusion with an alkali. This mode of rendering silica soluble in acids, is no new discovery; it has been long known; and the analysis of minerals has never been brought so near to truth, as since it has become an indispensable condition.

I have no doubt as to the fact of a precipitate being formed, by mixing together an alkaline solution of silica and alumina. Alumina indeed appears to exercise an attraction, as I before stated, for silica, for magnesia, and for lime. All stones in which there is but little alumina, and a great quantity of silica, leave, after fusion with potash, a light and flocculent substance, which cannot be dissolved by the acids: this substance, however, which is silica, has been in solution in the alkali. But, if a greater proportion of alumina be present, none of this flocculent precipitate appears; hence it is evident, that alumina must determine its solution. Its easy solubility, in the latter case, cannot depend upon the division of the particles of the silica in the stone; for, in the first place, after being fused with potash, the tenuity of the particles of every stone must be nearly the same; and, in the next place, I have not observed, that any earth, except alumina, can promote the chemical solution of the silica, though they must all occasion its mechanical division.

As to the affinity of alumina for magnesia, it is by much the most powerful of all those which any of the earths have for each other. I attempted to precipitate magnesia from muriatic acid, by ammonia, even in excess; but found that the whole muriate of magnesia had not been decomposed, and that a triple salt, or an ammoniacal muriate of magnesia,* had been formed. I then poured an excess of ammonia into a solution of muriate of magnesia, mixed with a large proportion of a solution of muriate of alumina. All the earth was precipitated; and nothing remained in solution, except muriate of ammonia. The liquor was then filtered, and the precipitate washed and dried. I dissolved it in muriatic acid, and boiled it with a great excess of potash. Some alumina was taken up, but by no means all the quantity that had been used. The precipitate which had resisted the action of potash, was again dissolved in muriatic acid, and precipitated by carbonate of potash. The carbonate of magnesia was held in solution by the excess of carbonic acid; and, by using potash and carbonic acid alternately, (the first to

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* This salt is well known in chemistry.

dissolve alumina, the second to dissolve carbonate of magnesia,) I effected a separation of the earths. These experiments show, that there is an affinity between alumina and magnesia, and a certain point of saturation, where the action of potash upon alumina is wholly counteracted by the affinity of that earth for magnesia.

When a solution of potash is boiled upon a mixture of lime and alumina, the alumina is dissolved, together with a much greater portion of lime than can be attributed to the dissolving power of the water alone. But, if a solution of potash be boiled upon lime, without alumina, no more lime is taken up than would have been dissolved by an equal quantity of water not containing potash in solution; consequently, alumina seems really to promote the solution of lime in potash. The affinity of alumina for lime, I had mentioned in the paper to which I allude; and it has since been noticed by Mr. VAUQUELIN.*

If the conclusions of Mr. GUYTON had been well founded, it would have been chemically impossible to arrive at truth in analysis. There were already real difficulties enough to be overcome; and Mr. BERTHOLLET has lately discovered some, which are not so easily answered as those I have just considered. The position of this chemist, however, has been too generally extended by him. If the power of masses were as great as he represents it to be, and if it increased *ad infinitum*, in proportion to the mass, it must follow, that, with any given substance, we could decompose any compound, provided the mass of the decompounding body were sufficiently great; but this is well known not to be the case.

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* SCHEELE was, in fact, the first who perceived this affinity. See his *Essay on Silex, Clay, and Alumina.*

From the experiments which I have related, it appears to be proved.

1st. That there exists an affinity between silica and alumina.

2dly. That there exists a very powerful affinity between alumina and magnesia.

3dly. That alumina shews an affinity for lime; but that the said affinity is not so strong as Mr. GUYTON had supposed, nor, if pure reagents be used, is it to be perceived under the circumstances stated by him.

4thly. That Mr. GUYTON was mistaken in every instance of affinity between the earths, excepting in the case of silica with alumina, which had been observed before his experiments; and that, in the other cases, he has attributed to a cause which does not exist, phenomena that must have resulted from the impurity of his reagents.

5thly. That neither the experiments of Mr. GUYTON, nor the opinion maintained in the letter from Freyberg, are sufficient to diminish, in any degree, the value of the assistance mineralogy derives from chemical investigation.

XI. *Description of the Anatomy of the* Ornithorhynchus Hystrix. *By* Everard Home, *Esq. F. R. S.*

Read June 3, 1802.

At the time I had the honour of laying before this learned Society, an anatomical description of the Ornithorhynchus paradoxus, (see page 67,) I did not attempt to point out any quadrupeds as being nearly allied to it, there being none at that time within my knowledge; but the discovery of another of the same tribe, which is the subject of the present Paper, enables me to trace one step further, in the gradation between that extraordinary animal and the more perfect quadruped.

The subject from which the following description was taken, was sent from New South Wales, preserved in spirit. It is a male, and had arrived nearly at its full growth, as the epiphyses were completely united to the bodies of the bones, which is not the case in growing animals.

A description and figure of this animal is given by Dr. Shaw, in his Zoology, under the name of *Myrmecophaga aculeata.*

Description of the external Appearances.

The animal is 17 inches long, from the point of the bill to the extremity of the tail: the bill is 1¾ inch long, and the tail half an inch.

The body of the animal is nearly of the same general thickness, but rather larger just below the shoulders. The greatest circumference of the body is 17 inches.

The back and sides are covered with short coarse hair, half an inch long, and with quills like those of the porcupine, only shorter and less pointed; they appear to be ranged in rows, in the direction of the animal's length; those on the sides are 2½ inches long, the others between one and two inches. The quills on each side of the body, between the setting on of the hind legs and the tail, have a direction forwards, so as to be opposed to the others.

The head and neck are covered with a coarser hair than the rest of the body, and are almost entirely without quills.

On the breast, the hair is long and soft, and without quills; on the skin of the belly, it is almost entirely wanting.

No appearance of false nipples could be detected, either on the belly or breast.

Externally there is no appearance of organs of generation; the orifice of the anus being a common opening to the rectum and the prepuce of the penis.

The bill, which projects from the head in a tubular form, is 1¾ inch long. It is conical in its shape, convex upon the upper surface, and flat upon the lower; at its point, it is ⅜ of an inch in diameter, and ⅞ at its base: it has the same smooth cuticular covering as the bill of the Ornithorhynchus paradoxus, but has not the lateral lips, the sides being closed to within half an inch of their extremity. The upper part of the bill is formed by an elongation of the nose and palate; and the lower portion by a continuation of the two bones of the under jaw, as in the paradoxus.

The nostrils are two small orifices, close to each other, within a quarter of an inch of the end of the bill.

The eyes are very small, and are situated laterally on the head, close to the base of the bill.

The external ears are two oval slits, an inch long, situated nearer to the upper part of the head than the eyes, and $2\frac{1}{2}$ inches further back.

The teeth, if they can be so called, being, like those of the paradoxus, composed of a horny substance, and not of ivory and enamel, as in all other quadrupeds, are not situated on the margin of the palate and lower jaw, but are confined to the tongue and surface of the palate. On the posterior part of the tongue, which is thicker and broader than the rest, there is a space, one inch in length and $\frac{3}{4}$ broad, covered with a strong cuticle, and having about 20 small teeth, blunt at their ends, projecting about $\frac{1}{10}$ of an inch; there are also several others, less prominent. On that part of the palate immediately opposite, there are seven transverse rows of very slender horny teeth, with their points directed backwards: each row looks somewhat like a small-toothed comb, laid flat upon the palate.*

The appearance of these horny teeth, and a general view of the palate and tongue, are represented in Plate XI.

The fore legs are short and thick, and have five toes, with strong blunt claws, intended probably for the purpose of digging; the middle claw is the longest, the others becoming gradually shorter. The leg, to the end of the longest claw, is

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* In the duck, both upon the tongue and palate, there are horny papillæ, which have a slight resemblance to the horny teeth just described; those on the tongue are lateral, six on each side.

three inches long; the palms of the feet are covered with a strong cuticle.

The hind legs are longer than the fore legs, and have five toes; four of these have long strong claws, the innermost is the longest. The fifth toe is short, and, being opposed to the others, resembles a thumb. The length of the leg, to the point of the longest claw, is six inches. Just at the setting on of the heel there is a spur, similar to that of the paradoxus, only weaker and smaller; it is $\frac{3}{8}$ of an inch long.

The tail is covered with hair, and is about half an inch in diameter; it terminates in a blunt end.

Description of the internal Parts.

The internal structure so nearly resembles that of the Ornithorhynchus paradoxus, that a particular description of many of the parts will be unnecessary.

The panniculus carnosus is similar to that of the paradoxus. The tongue is cylindrical, very small towards the point, and eight inches long. Near the root there is an oval portion, more massy than the rest, on which are placed the horny teeth already described.

The velum pendulum palati, and glottis, resemble those of the paradoxus; but, at the termination of the fauces in the œsophagus, there is a projecting fold or valve, peculiar to this species; and the epiglottis is bifid in a small degree.

In the structure of the bones of the chest, there are the same general peculiarities as in the paradoxus; but, in the Hystrix, there is a xiphoid cartilage, having its origin from the under surface of the sternum, and being about one inch in length.

The heart and lungs, both in their structure and relative situation, resemble those of the paradoxus, with the exception of the heart having only one vena cava superior, instead of two.

The diaphragm is similar to that of the paradoxus.

The œsophagus is small, but has several longitudinal folds, which render it capable of dilatation; it is lined with a strong cuticle, which is continued down to the cavity of the stomach.

The stomach is a thin membranous bag, nearly of the shape of the human stomach; in its collapsed state, it measured $4\frac{1}{2}$ inches in length, and 3 inches in breadth.

Its internal membrane is smooth, and without the appearance of glands, except towards the pylorus: it is lined with a cuticle; and the glandular part has horny papillæ, $\frac{1}{10}$ of an inch long, which appear to be the excretory ducts through which the gastric juice is conveyed into the cavity. This uncommon appearance is represented in Plate XI.

Similar cuticular papillæ are to be observed in the paradoxus; but they are so extremely small as to require a particular examination to detect them: the stomach of that animal also appears to be lined with a thin cuticle.

Along with the food, a quantity of sand is received into the stomach, and passes down through the bowels; it was met with in different parts of the small intestines, and also in the colon; it was very fine, and of a white colour.

It is deserving of observation, that in this animal, the mode of managing the food is different from that employed in the paradoxus; which accounts for the difference in the appearance both of the teeth and stomach.

In this species, the food is bruised between the teeth placed upon the tongue and those of the palate; and, immediately afterwards, the whole is conveyed into the stomach, and along with it a quantity of sand.

The stomach therefore is sufficiently large to contain the food, and the extraneous matter connected with it; and is defended from injury by its cuticular lining. In the paradoxus, the food is received into the mouth, is retained in the lateral pouches, and is prevented, by the two projecting teeth on the tongue, from getting into the stomach, till all the indigestible parts are separated; the nutritious matter alone being allowed to reach the stomach, which is of a very small size.

The course of the intestines, and the form of the cæcum, are the same as in the paradoxus; the cæcum is shorter, being only half an inch long.

The small intestines are seven feet, the colon and rectum two feet long.

The rectum is similar in every respect to that of the paradoxus.

The mysentery, its glands, and the lacteals, are also similar to those of the paradoxus.

The internal membrane of the duodenum has a corrugated appearance, but no valvulæ conniventes. The cavity of the small cæcum is not loculated; and there are ten or twelve excretory ducts of glands on the membrane of the colon, near the opening of the cæcum; but these are placed irregularly; and there are many similar orifices, in different parts of its course.

The liver and gall-bladder, with their ducts, and also the omentum, are similar to those of the paradoxus.

The pancreas is not so much separated into detached parts as in the paradoxus; but is less compact than in quadrupeds in general.

The spleen is shorter and thicker than in the paradoxus; but has the same general shape.

The kidneys and bladder are exactly similar to those of the paradoxus.

The skull, in its general shape, is similar to that of the duck; and has not the bony falciform process observed in the paradoxus.

The brain was not in a state to admit of particular examination.

The olfactory nerves are divided into numerous branches.

The optic nerves are small; and the fifth pair of nerves is much smaller than in the paradoxus; the second branch, which in that species is very large, and supplies the upper part of the bill, is either extremely small, or altogether wanting. This animal has therefore, probably, a less acute sense of feeling in the bill than the paradoxus; and, as the organ of smell is more complex, the increase of that sense may make a nice discrimination by touch less necessary.

The eye-lid is very loose upon the eye-ball, has a circular aperture, and appears to have great extent of contraction and relaxation. The membrana nictitans is wanting.

The eye-ball is $\frac{8}{20}$ of an inch in diameter; the cornea $\frac{3}{20}$, surrounded by a zone of a black pigment, $\frac{2}{20}$ in breadth.

The organ of smell differs materially from that of the paradoxus. Immediately below the cribriform plate of the ethmoid bone there are bony processes, forming a cellular structure, nearly half an inch thick, which constitutes the principal part of the organ; from this there is a convex projecting turbinated bone, of a very slender form, extending half way to the external opening of the nostril, with a corresponding concave one to receive it, in each nostril; and there is a small slit or opening between the two nostrils. The structure of the organ is shown in Plate XI.

The external opening of the ear is large enough to admit the end of the finger; the meatus takes the same sweep as in the paradoxus; just before it reaches the membrana tympani, it contracts to the size of a crow-quill, then again dilates, forming a cavity round the membrana tympani: it is lined with hair, till it forms this constriction.

The membrana tympani is externally concave, and is covered by a cuticle. It is of an oval form; the long axis of the oval is $\frac{4}{20}$ of an inch, the short one $\frac{3}{20}$. Its centre is attached to a small bone, connected with the bony rim by which the circumference of the membrane is supported: this bone corresponds to the malleus of the quadruped. On the inner side of this, and united to it by a smooth surface, is a small bone, in the form of a trumpet, which may be considered as the stapes, as it fills the opening of the foramen ovale.

There is no perfect cochlea, as in quadrupeds in general; but there is the imperfect cochlea met with in the bird, which has been accurately described by Mr. CUVIER.* It consists of a conical cavity, a little bent, in the middle of which there is a double cartilaginous septum: the two laminæ unite before they reach the end of the cone; by this means, the surrounding cavity becomes a spiral canal, one end of which opens into the vestibulum, the other terminates at the foramen rotundum.

The male organs of generation bear a close resemblance to those of the paradoxus. The testicles are in every respect similar: the vasa deferentia open into the urethra, close to the neck of the bladder, as is seen in Plate XII. and it is at the same part they open in the paradoxus.

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* *Leçons d'Anatomie comparée.* Vol. II. p. 464.

The urethra for the urine opens into the rectum, about an inch from the anus; and the passage for the semen goes into the penis, in the same manner as in the paradoxus.

The penis is very elastic in its substance; when drawn out, it is about three inches long; but, from having been so long kept in spirit, is not sufficiently ductile to allow of an accurate judgment respecting its real length. The glans is externally subdivided into four equal processes; in the centre of each of these is an orifice, surrounded by concentric circles of infinitely small prominent papillæ.

There is a gland on each side of the rectum, the size and situation of which are delineated in Plate XII.; each of these has a small excretory duct, which passes to the root of the penis, where they unite, and then open by one common orifice into the urethra for the semen, $\frac{1}{10}$ of an inch after it has entered the penis.

These glands must be considered as corresponding to COWPER's glands in the human subject, and not as a substitute for the prostate gland, or the vesiculæ seminales, since something analogous is met with in the female.

In my account of the Ornithorhynchus paradoxus, these glands are described as belonging to the rectum. This mistake arose from the parts being so much coagulated, by long continuance in strong spirit, as to make it impossible to distinguish the excretory duct from the surrounding blood-vessels, or other parts. In the specimen of the Hystrix from which this description is taken, the parts were in the same state, and would have led me into a similar error, had I not been favoured by Sir JOSEPH BANKS with a specimen of the paradoxus, brought from New South Wales by Mr. BELMAIN, which had been kept in weak spirit; and, although many other parts had become putrid, those connected with the organs of generation had been preserved, and were in a flaccid state, more favourable for anatomical examination.

I was not only enabled to examine these glands and their ducts, but also, by fixing a pipe into the urethra where it enters the penis, to inject water along that canal, so as to make it fill a small cavity in the centre of each glans, and from that pass through all the papillæ, which became erect as soon as the glans was turgid, and scattered the water by so many small streams, about the size of a horse-hair, in every direction.

Upon re-examining the female organs of the paradoxus, after they had been steeped in water, I was enabled to trace the ducts of the glands, which correspond with those of the male, to one common orifice on the posterior surface of the vagina, $\frac{1}{4}$ of an inch within the orifice of that canal.

A clitoris was also detected, with two crura, arising from the outer side of the common vestibulum to the rectum and vagina. The clitoris was very slender, half an inch long; its glans a little bifid, and inclosed in a thin prepuce; the end of the glans only projected into the vestibulum.

The female organs of the Hystrix have not been examined; but there can be no doubt of their bearing the same resemblance to those of the male as in the paradoxus.

Another species of Ornithorhynchus, of the same size as the Hystrix, was shot at Adventure Bay, Van Diemen's Land, by Lieutenant GUTHRIE, in the year 1790, a drawing of which was made by Captain BLIGH, and sent to Sir JOSEPH BANKS, who has allowed me to annex a copy of it to this Paper. The quills of this species, as I am informed by Captain Bligh, are so short, that the points only are seen projecting beyond the hair.

The Ornithorhynchus Hystrix is a nearer approach to the more perfect quadruped than the paradoxus; and, as its tongue is similar in some respects to those of the Manis and Myrmecophaga, it was natural to look among the different species of these genera, for other points of resemblance.

I have examined a figure of the Manis of Sumatra, drawn by the late Mr. Bell, while resident there, whose abilities as an anatomist and draughtsman, make his death a considerable loss to science.* The form of the head, the opening of the mouth, and the general appearance of the animal, led me to believe it a still further remove from the Ornithorhynchus than the Myrmecophaga; and the following circumstances, in the internal structure of these two genera, confirm this opinion. The Myrmecophaga has two cæca, which resemble that of the Ornithorhynchus; whereas the Manis has no appearance whatever of cæcum.

There are two specimens of Manis preserved in spirit, in the Hunterian Museum, one male, the other female; both of these I have examined.

The tongue was small, cylindrical, and very long; and the muscle by which it is retracted lay between the abdominal muscles and peritonæum of the right side, forming a semicircle between the lower end of the sternum and the navel: the theca in which it was inclosed, had an attachment to the lower end of the sternum. The tongue was smooth; and there was no appearance of teeth on it, or on the palate.

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* This drawing is in the possession of Mr. Marsden, who proposes publishing it in the next edition of his History of Sumatra.

There was no cæcum, the intestine suddenly enlarging to form the colon: on each side of the anus there was a bag, as in the otter, and most other animals which have no cæcum.

The organs of generation, in both sexes, were distinct from the anus; the penis was small. In the female there were two nipples upon the breast. The uterus was broad at its fundus; and the two horns separated from each other, nearly at right angles to the middle line of the uterus.

The didactyla is the only species of Myrmecophaga which has come under my observation. The Trustees of the British Museum allowed me, in the most liberal manner, to examine both the male and female. The tongue had a general resemblance to that of the Ornithorhynchus Hystrix; but there were no cuticular teeth upon it, or on the palate. The cæcum was of the same kind, but double, and each of them was only $\frac{1}{8}$ of an inch in length. In the other parts there was no similarity. The male had four false nipples, two on the breast and two on the belly, corresponding with the true nipples of the female.

The organs of generation were not connected with the rectum. The uterus was nearly of the shape of the human uterus; its coats were very thin; and the cavity larger in proportion than in most quadrupeds. There were no horns; and the fallopian tubes went off from the posterior part. This is an approach to the uterus of the Opossum.

With a view to procure information respecting the other species of Myrmecophaga, I wrote to Mr. CUVIER of Paris, whose abilities and extensive researches in comparative anatomy, have so deservedly distinguished him in that branch of science.

By a letter from him, I find that the Myrmecophaga jubata, Tamandua, and capensis, belong decidedly to the class Mammalia;

and therefore are not so nearly allied to the Ornithorhynchus as I had at first been led to imagine. The Myrmecophaga jubata, which is described by Mr. Zan to have the organs of generation, in both sexes, concealed within the verge of the anus, appears to be a nearer approach to it than the other species.

The peculiar characters of the Ornithorhynchus, as a genus, or more properly a tribe of animals, are,

The male having a spur on the two hind legs, close to the heel.

The female having no nipples.

The beak being smooth, while the rest of the animal is covered with hair.

The tongue having horny processes, answering the purposes of teeth.

The penis of the male being appropriated to the passage of the semen; and its external orifice being subdivided into several openings, so as to scatter the semen over an extent of surface, while the urine passes by a separate canal into the rectum.

The female having no common uterus; and the tubes which correspond to the horns of the uterus in other quadrupeds, receiving the semen immediately from the penis of the male.

These characters distinguish the Ornithorhynchus, in a very remarkable manner, from all other quadrupeds, giving this new tribe a resemblance in some respects to birds, in others to the Amphibia; so that it may be considered as an intermediate link between the classes Mammalia, Aves, and Amphibia; and, although the great difference that exists between it and the Myrmecophaga, the nearest genus we are at present acquainted with, shows that the nicer gradations towards the more perfect quadrupeds are not at present known, the facts which have been stated may induce others to prosecute the inquiry, and render that part of the chain more complete.

Between it and the bird, no link of importance seems to be wanting.

The great affinity between the male organs of the Ornithorhynchus and those of birds, is best illustrated by comparing the penis of the former with that of the drake, a figure of which is annexed. (Plate XII. Fig. 2.) It is six inches long when drawn out to its full extent; but, when left to itself, (so great is the contractile power of the urethra,) it retracts, and confines the whole penis within the verge of the rectum.

The urethra begins by a blunt end; and the vasa deferentia open into it close to its origin: its sole use, as in the Ornithorhynchus, is to eject the semen.

When more of this extraordinary tribe of animals, which, although quadrupeds, are not Mammalia, shall have been discovered, and naturalists thereby enabled to divide them properly, the two which I have described will doubtless be arranged under different genera; till then, I have thought it best to consider them as species of the same genus, rather than encumber science with an additional name, or attempt to frame generic characters from one species only.

PLATE X.

A figure of the Ornithorhynchus Hystrix, (on a scale of half an inch to an inch,) to show its general appearance, but more particularly its cuticular bill.

PLATE XI.

Fig. 1. A view of the bill and throat, laid open, to show the tongue and palate.

a. The tongue in its natural situation.
b. The cuticular teeth upon the tongue.
c. The cuticular teeth upon the palate.
d. The bifid epiglottis immediately above the glottis.
e. The valvular projection at the beginning of the œsophagus.

Fig. 2. A section of the nose and skull, to show the peculiarities of the organ of smell, and the shape of the cavity of the skull, in which the bony falx met with in the paradoxus is wanting.

a. The cavity of the skull.
b. The peculiar structure of bone through which the branches of the olfactory nerve pass, after leaving the cavity of the skull.
c. The turbinated bone, or what corresponds to it.
d. The septum of the nose.
e. The slit through the septum.
f. The posterior nostrils.

Fig. 3. The appearance of cuticular papillæ on the internal

membrane of the stomach, situated at the termination of the pylorus in the duodenum.

Plate XII.

Fig. 1. The penis and testicles in their relative situation, to show the urethra for the passage of the urine, and that for the semen.

aa. The glans penis divided into four projecting processes, which in the relaxed state are concave; the orifice is in the centre of each of the projections.

b. The body of the penis.

cc. The rectum laid open.

dd. The orifices of the glands of the rectum.

ee. The two glands which correspond to Cowper's glands, their excretory ducts opening into the urethra of the penis.

f. The termination of the urinary urethra in the rectum.

g. The urethra laid open through its whole course.

h. The opening leading to the urethra for the semen.

i. The orifice of the neck of the bladder.

k. The urinary bladder.

ll. The openings of the vasa deferentia into the urethra.

mm. The bodies of the testicles.

nn. The epididymis of the testicles.

Fig. 2. The penis of the drake, in its extended state.

aa. The verge of the fundament surrounded by the feathers.

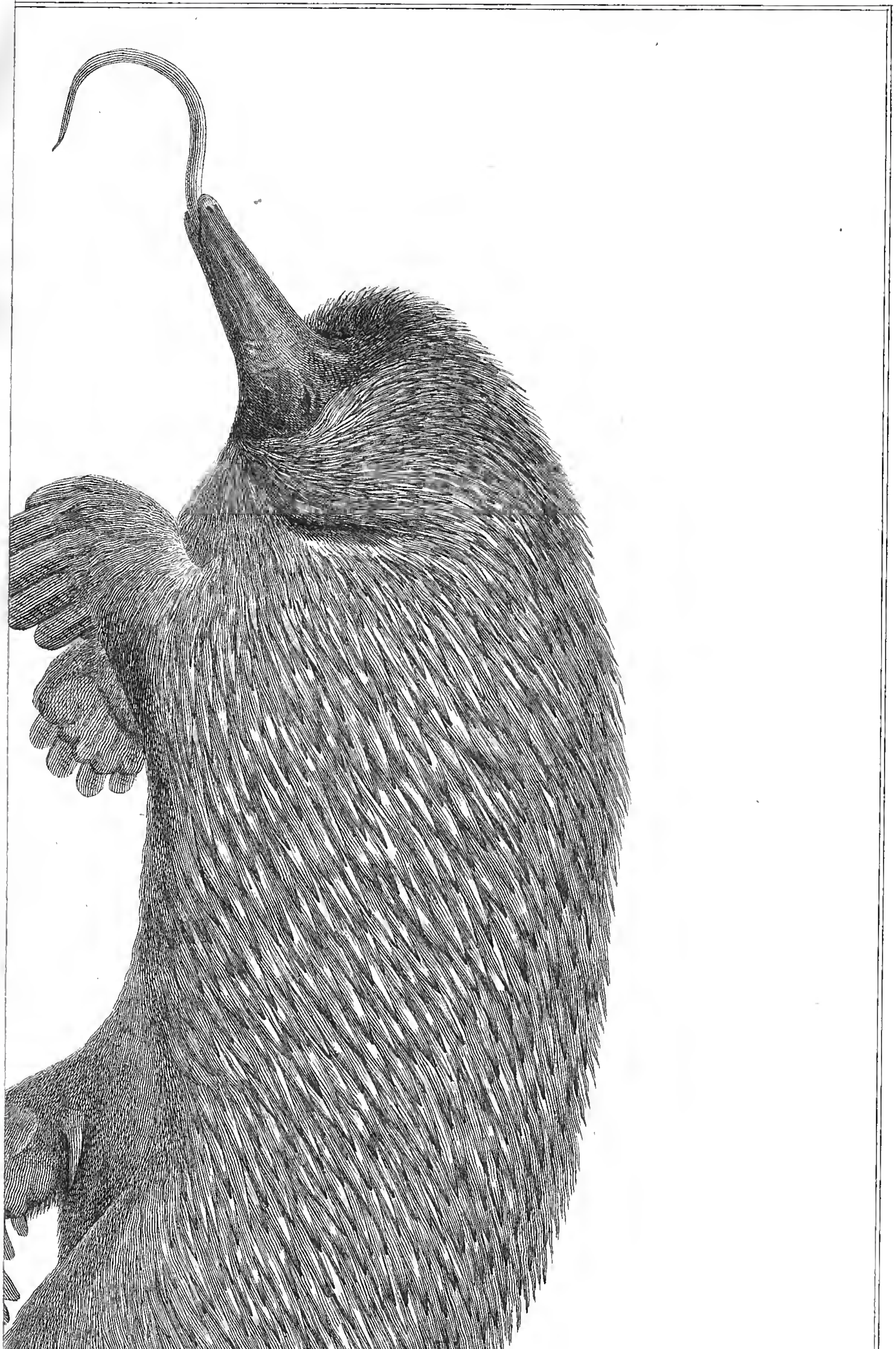
bb. The urethra laid open through its whole extent.

cc. The orifices of the vasa deferentia.

dd. The prepuce of the penis laid open, and, from its elasticity, thrown into serpentine folds.

PLATE XIII.

Another species of Ornithorhynchus, 17 inches long, with small quills, about one inch long. The animal, when it walked, had its body raised about two inches from the ground. It was shot at Adventure Bay, Van Diemen's Land.



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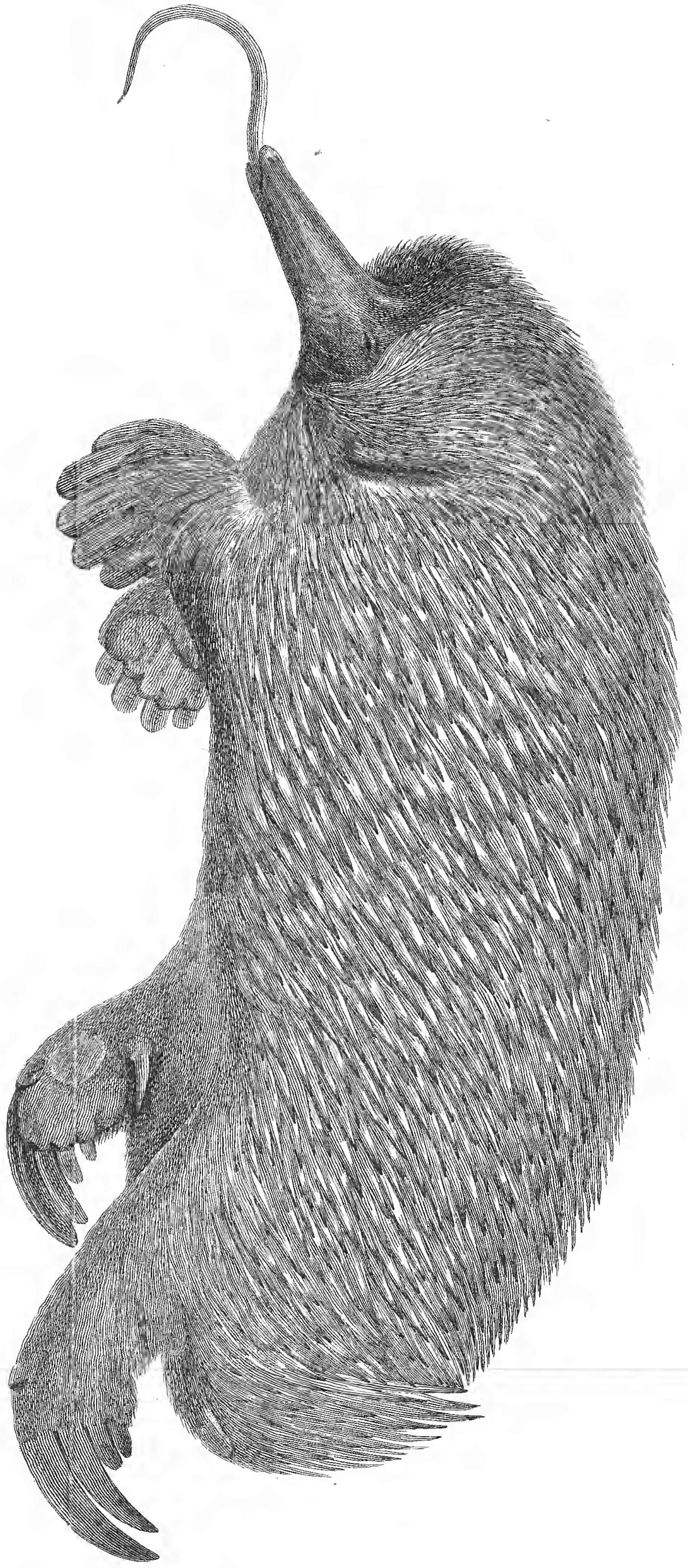

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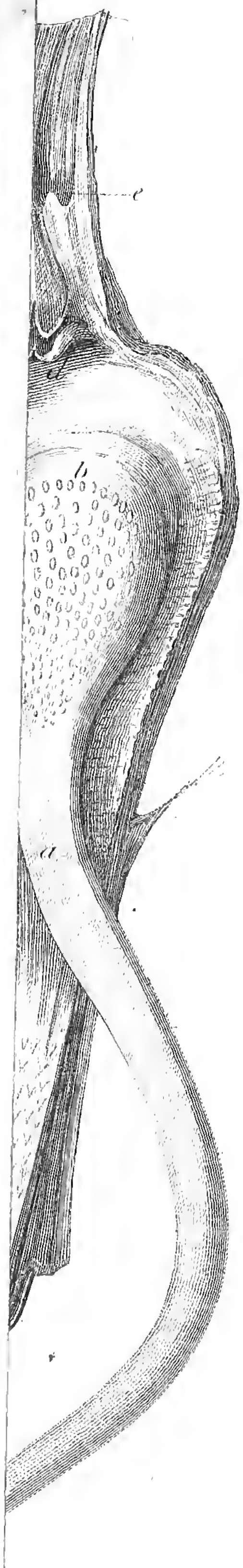

Fig. 2.

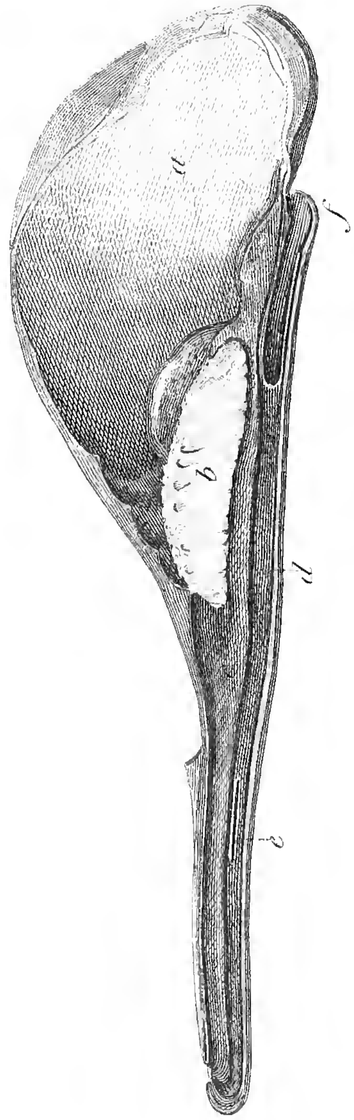

Fig. 3.

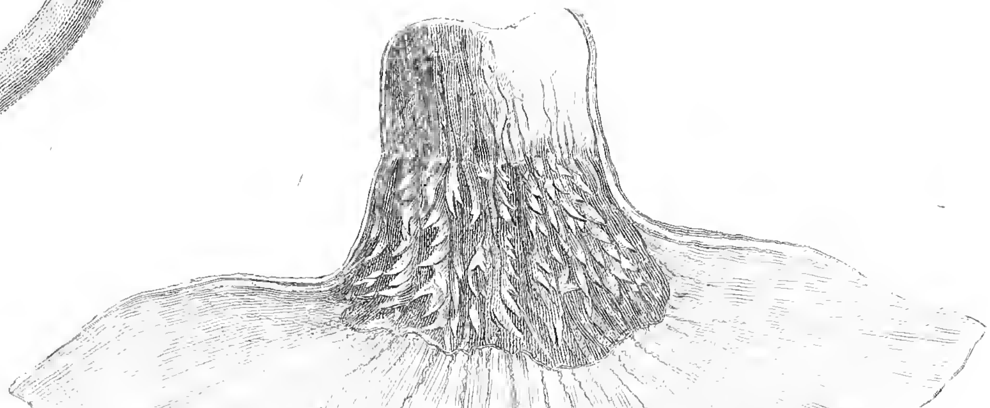

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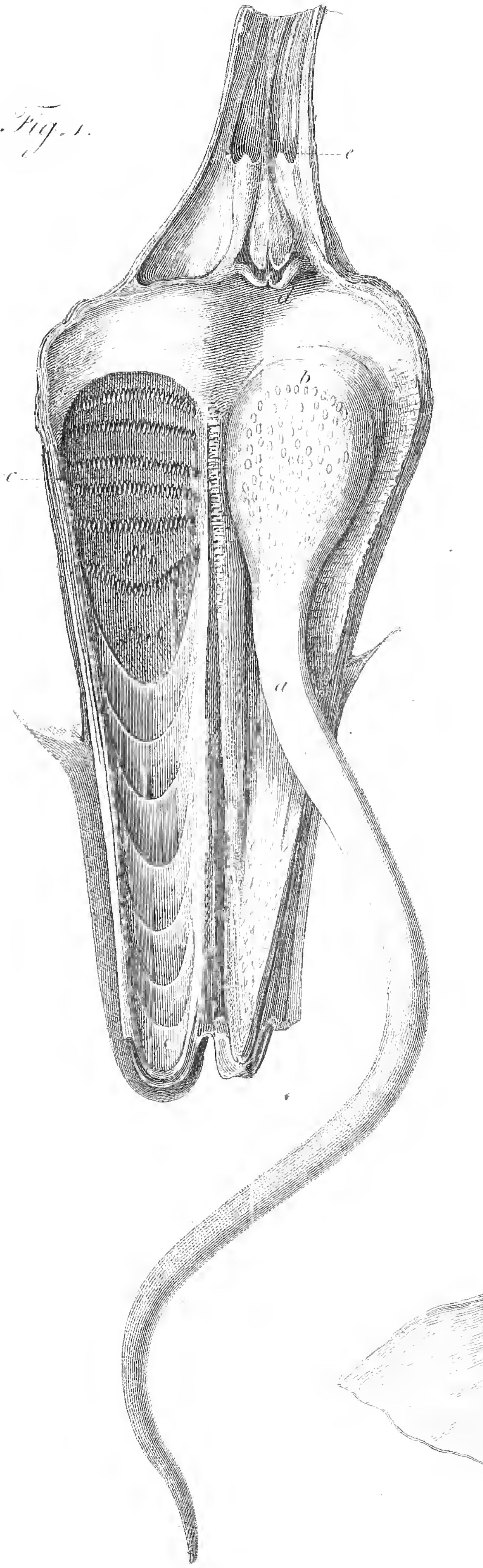
Fig. 1.



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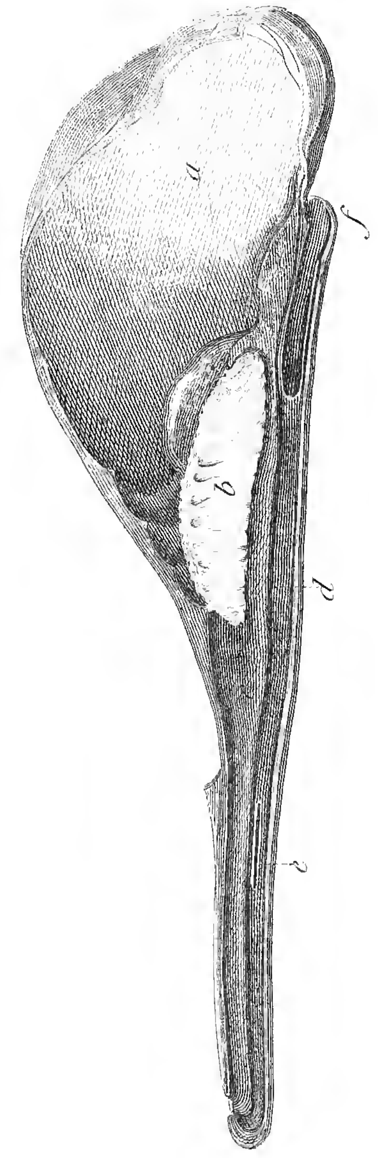

Fig. 3.

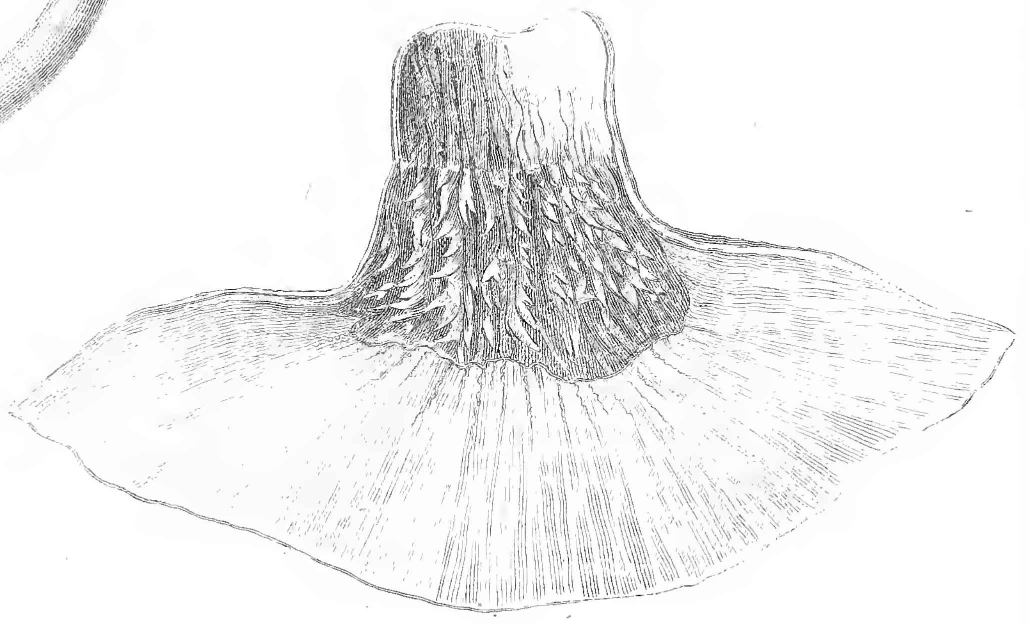

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Fig. 2.

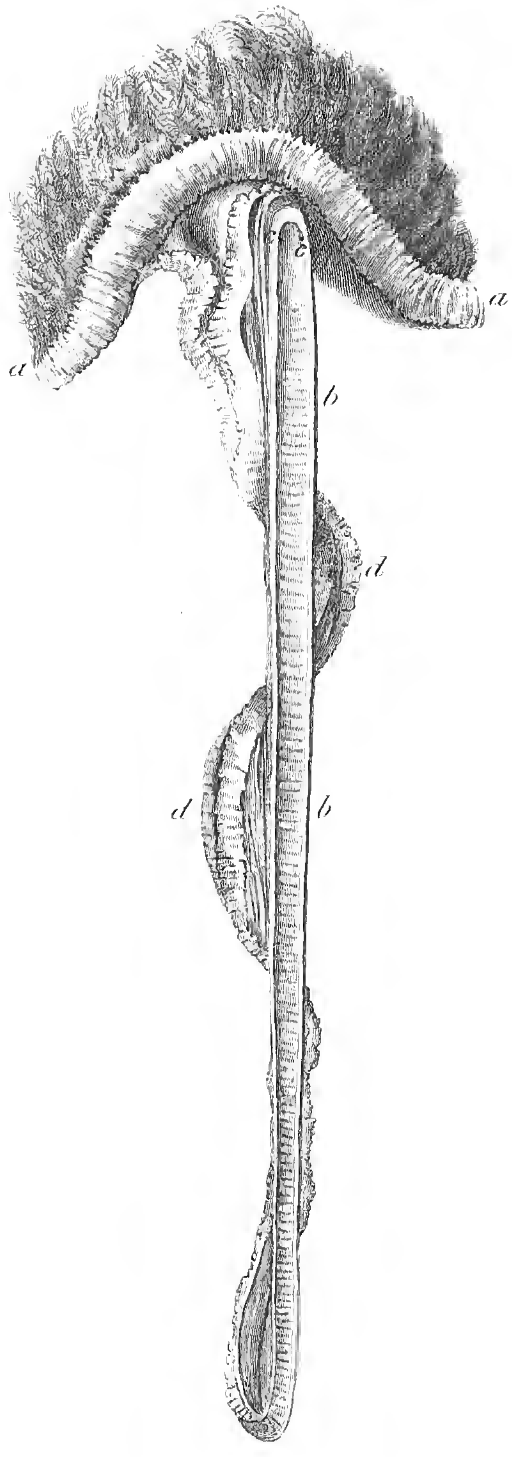

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Fig. 1.

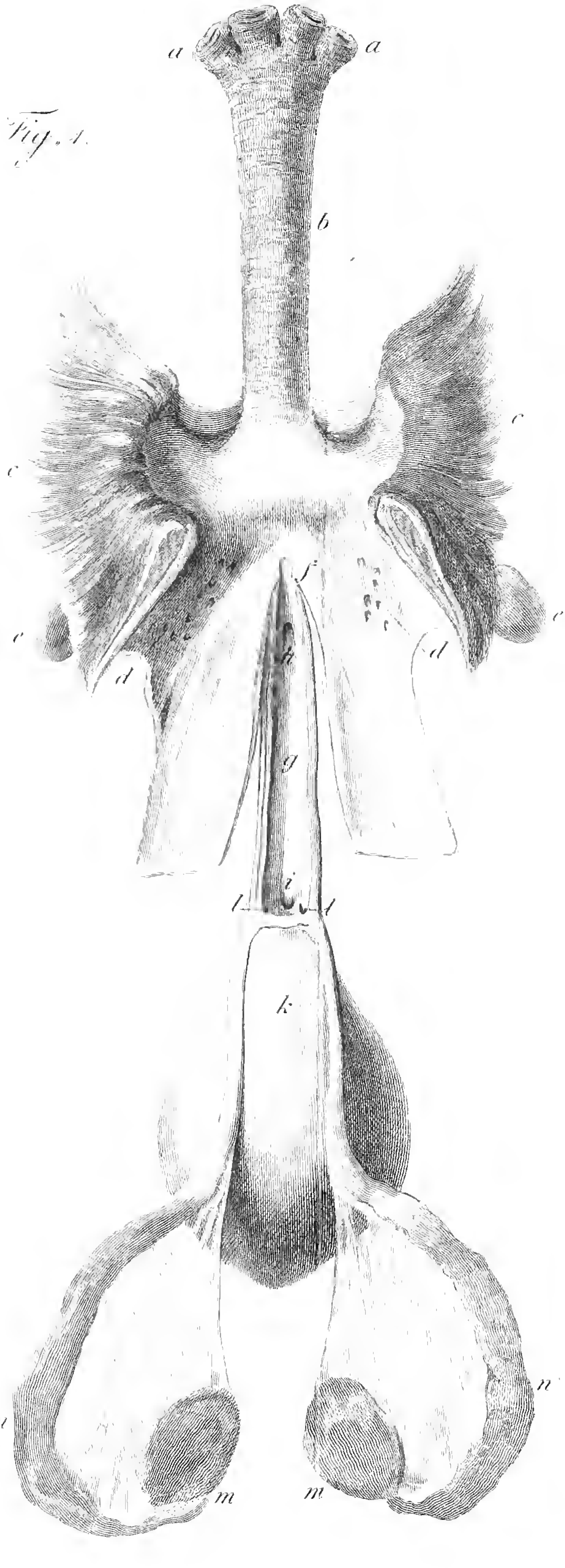
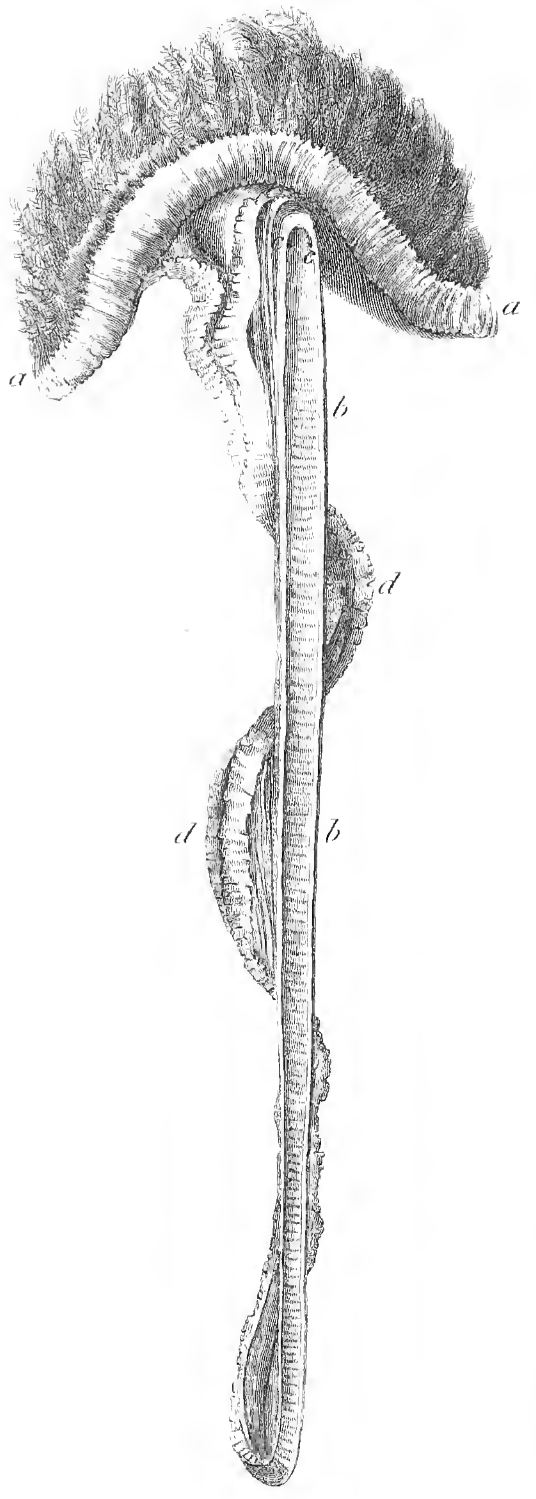


Fig. 2.



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Philos. Trans. MDCCCII. *Plate* XIII. *p.* 364.

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Philos. Trans. MDCCCII. *Plate* XIII. *p.* 364.

W. Clift del.

J^s Basire sc.

XII. *A Method of examining refractive and dispersive Powers, by prismatic Reflection.* *By* William Hyde Wollaston, *M.D. F.R.S.*

Read June 24, 1802.

In examining the power with which various substances refract and disperse light, I have for some time past employed a method unnoticed by writers on optical subjects; and, as it is not only convenient in common cases of refraction, but also capable of affording results not attainable by other means, I have been induced to draw up a short account of the method itself, and of the most remarkable instances of its application.

This method was suggested by a consideration of Sir Isaac Newton's prismatic eye-glass, the principle of which depends on the reflection of light at the inner surface of a dense refracting medium.

Since the range of inclination within which total reflection takes place, depends not only on the density of the reflecting prism, but also on the rarity of the medium adjacent to it, the extent of that range varies with the difference of the densities of the two media. When, therefore, the refractive power of one medium is known, that of any rarer medium may be learned, by examining at what angle a ray of light will be reflected from it.

For instance, when any object is laid under a prism of flint-glass, with air alone interposed, the internal angle of incidence at which the visual ray begins to be totally reflected, and at which the object ceases to be seen by refraction, is about 39° 10′; but, when the object has been dipped in water, and brought into contact with the glass, it continues visible, by means of the higher refractive power of the water, as far as $57\frac{1}{2}°$ of incidence. When any kind of oil, or any resinous cement, is interposed, this angle is still greater, according to the refractive power of the medium employed; and, by cements that refract more strongly than the glass, the object may be seen through the prism, at whatever angle of incidence it is viewed.

In examining the refractive powers of fluids, or of fusible substances, the requisite contact is easily obtained; but, with solids, which can in few instances be made to touch to any great extent, this cannot be effected without the interposition of some fluid, or cement, of higher refractive power than the medium under examination. Since the surfaces of a stratum so interposed are parallel, it will not effect the total deviation of a ray passing through it, and may therefore be employed without risk of any error in consequence.

Thus, resin, or oil of sassafras, interposed between plate glass and any other prism, will not alter the result.

If, on the same prism, a piece of selenite and another of plate-glass be cemented near each other, their powers may be compared with the same accuracy as if they were both in absolute contact with it.

For such a mere comparison of any two bodies, a common triangular prism is best adapted; but, for the purpose of actual measurement of refractive powers, I have preferred the use of a square prism, because, with a very simple apparatus, it shows the sine of refractive power sought, without the need of any calculation.

Let A, Fig. 1, Plate XIV. be a square or rectangular prism, to which any substance is applied at *b*, and let any ray of light parallel to *cb* be refracted through the prism, in the direction *bde*.

Then, if *ef* and *ed* be taken proportional to the sines that represent the refractive powers of the prism and of air, *fg*, which is intercepted between *f* and the perpendicular *eg*, will be the corresponding sine to represent the refractive power of the medium *b*. For, since *edg* (opposite to *ef*) is the angle of refraction, *efg* (opposite to *ed*) must be equal to the angle of incidence *bdh*; and *ef* : *fg* : : *bd* : *dh* : : sine of *cbi* : sine of *hbd*.

All therefore that is requisite for determining the refractive power of *h*, is to find means of measuring the line *fg*. On this principle, the instrument in the annexed sketch (Fig. 2.) is constructed. On a board *ab* is fixed a piece of flat deal *cd*, to which, by a hinge at *d*, is jointed a second piece *de*, 10 inches long, carrying two plane sights at its extremities. At *e* is a second hinge, connecting *ef*, 15,83 inches long; and a third at the other extremity of *ef*, by which *fg* is connected with it. At *i* also is a hinge, uniting the radius *ig* to the middle of *ef*; and then, since *g* moves in a semicircle *egf*, a line joining *e* and *g* would be perpendicular to *fg*.

The piece *cd* has a cavity in the middle of it, so that, when any substance is applied to the middle of the prism P, it may continue to rest horizontally on its extremities. When *ed* has been so elevated that the yellow rays in the fringe of colours (observable where perfect reflection terminates) are seen through the sights, the point *g*, by means of a vernier which it carries, shows by inspection the length of the sine of refraction sought.

The advantages which this method possesses above the usual mode of examining refractive powers, are greater than they may at first sight appear. The usual practice has been, to form two surfaces of the substance under examination, so inclined to each other that the deviation occasioned by them might be measured. The inclination of these surfaces to each other must also be known; and thence the refractive power might be computed. But, in the method here proposed, it is sufficient to have only one surface, and the result is obtained at once, without computation.

The facility of determining refractive powers, is consequently such as to render this property of bodies a very convenient test in many philosophical inquiries. For discovering the purity of essential oils, such an examination may be of considerable utility, on account of the smallness of the quantity requisite for trial. In oil of cloves, for instance, I have met with a wide difference. The refractive power of genuine oil of cloves, is as high as 1,535; but I have also purchased oil by this name, which did not exceed 1,498, and which had probably been adulterated by some less refractive oil.

For such purposes, the refractive power of opaque substances may often be deserving of inquiry, which could not be learned by any means at present in use. For, in the usual mode, a certain degree of transparency is absolutely necessary; but, for trial by contact, the most perfect opacity does not occasion the least impediment.

Among other instances in which I have taken advantage of this circumstance, I may mention a substance that had been found in one of the islands of the North Pacific Ocean, which, to all outward appearance and by various trials, seemed to be perfect bees-wax, although it is supposed that there are no bees in the island from which it was brought. On placing it by the side of a piece of bees-wax, in contact with a prism, the perfect equality of their refractive powers afforded a strong confirmation of the opinion before formed of their identity.

For the examination also of media of which the refractive density is not uniform, the general method of trial by deviation wholly fails; on the contrary, by placing a varied medium in contact with a prism, all its gradations of density, from greatest to least, become at once the object of mere inspection. An instance of this may very readily be seen with a piece of gum, the surface of which has been moistened for a few minutes; when, by close application to a prism, a refractive power may be discerned, varying from that of the water on the surface, 1,336, to nearly 1,51, the refractive power of gum arabic.

I should not so much insist on this advantage, were it not for the opportunity hereby afforded of examining the crystalline lens of the eye, which is now known to be generally more dense in the centre than at its surface.

Mr. Hauksbee, who was not aware of this difference, has estimated the refractive power of the crystalline lens, by forming it into a wedge by plates of glass, somewhat higher than I find it to be; but, with his accustomed accuracy, he remarked the apparent enlargement of an object, occasioned by the variations of its density, which he was unable to explain.

In the table that follows, I have set down, not only the limits of refractive power in a crystalline lens of an ox, ascertained by trial, but also an average, computed from the refractive density of a dried crystalline of an ox, of which the weight had been first taken in the recent state, and the quantity of water lost by drying also measured.

The table exhibits a series of substances, arranged according to their refractive powers. That of the diamond is copied from Sir ISAAC NEWTON; of other bodies to which (on account of their being more dense than glass) the machine for measurement would not apply, the refractive powers have been found by other means, for the sake of furnishing a more continued series of subjects for comparative experiments. The rest have been compared by this method; and their power, when expressed in numbers, actually measured.

TABLE I.

Diamond	2,44
Plumbago	——
Native sulphur (double)	2,04
Glass, consisting of lead 6 and sand 1	1,987
Glass of antimony	1,98
Jargon	1,95
Spinelle ruby	1,812
Arsenic	1,811
Muriate of antimony,	variable
White sapphire	1,768
Gum dragon	—
Iceland spar, strongest	1,657
Sulphate of barytes (double)	1,646
Balsam of Tolu	1,60
Guaiacum	1,596
Benzoin	—
Flint glass	1,586
Ditto	1,583
Horn	—
Phosphorus	1,579
Mica	—
Opium	—
Amber	1,547
Rock crystal (double)	1,547
Old plate glass	1,545
Colophony	1,543
Box-wood	—
Bees-wax	1,542
Oil of sassafras	1,536
Red sealing-wax	—
Spermaceti, cold	—
Sugar, after fusion	—
Arseniate of potash	—
Mastic	—
Elemi	—
White wax (cold)	—
Oil of cloves	1,535
Copal	1,535

Anime	1,535
Radcliffe crown glass	1,533
Pitch	—
Centre of crystalline of fish, and dry crystalline of an ox	1,530
Canada balsam	1,528
Crown glass, common	1,525
Selenite	1,525
Caoutchouc	1,524
Gum lac	—
Dutch plate glass	1,517
Human cuticle	—
Gum arabic	1,514
Balsam of capivi	1,507
Oil of amber	1,505
English plate glass	1,504
French plate glass	1,500
Oil of nutmeg	1,497
Sulphate of potash	1,495
Tallow, cold	1,49
Iceland spar, weakest	1,488
Camphor	1,487
Linseed oil	1,485
Butter, cold	1,480
Essence of lemon	1,476

Oil of turpentine, common	1,476
——————— rectified	1,470
Oil of almonds	—
—— olives	1,469
—— peppermint	1,468
—— lavender	1,467
Tallow, *melted*	1,460
Alum	1,457
Spermaceti, *melted*	1,446
Crystalline lens of an ox	1,447
to	1,380
Computed average of ditto	1,430
Sulphuric acid	1,435
Fluor spar	1,433
Nitric acid (sp. gr. 1,48)	1,410
Alcohol	1,37
White of an egg	1,36
Æther	1,358
Vitreous humour of an eye	1,336
Water	1,336
Atmospheric air (HAUKSBEE)	1,00032

ON THE DISPERSION OF LIGHT.

The method above described for investigating refractive powers, may also be employed with similar advantage for inquiries into the dispersion of light by different bodies, and the consequences that result from their combined action.

When a glass prism is placed in contact with water, and brought near the eye, in such a position that it reflects the light from a window, the extent of perfect reflection is seen to be bounded by a fringe of the prismatic colours, in the order of their refrangibility.* The violet rays, being in this case the most refrangible, appear strongest and lowest, on account of the less obliquity that is requisite for their reflection.

But it may happen that two media, which refract unequally at the same incidence, may disperse equally at that incidence. Under these circumstances, a pencil of rays passing from one of such media into the other, will be refracted, without dispersion of its colours. The boundary of prismatic reflection would then be found a well defined line, free from colour, if the surface at which the reflected light emerges from the prism were at right angles to its course.

When the disparity of the dispersive powers of the media is still greater, it may also happen, that the usual order of prismatic colours will be reversed; and then the red will appear strongest and lowest in the fringe, unless the colours so produced are counteracted by refraction at their emergence from the prism.

An instance in which the colours are so reversed, may be seen by application of oil of sassafras to a prism of flint glass.

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* NEWTON's Optics. Book i. part 2. Exp. 16.

So high is the dispersive power of this oil, that, in refractions from flint glass into it, the red rays are refracted more than the violet.

It must be observed that, in this experiment, when the angle of reflection within a triangular prism exceeds 60°, the angle of emergence is such as would alone occasion the red rays to appear lowermost; but, when the glass used is rectangular, the refraction at emergence has an opposite effect; any reversion of colour will therefore be in some degree corrected, and may not be seen, unless the dispersive power of the medium in contact much exceeds that of the glass.

A case of refraction with an inverted order of colours, has been observed by Dr. Blair,* in a compound object-glass, where crown-glass was in contact with oil of turpentine. From trials with lenses, he likewise inferred, that several other fluids have the same effect, when applied to that glass.

With this glass, and also with plate-glass, I have tried oil of turpentine, and many other fluids that afford a similar reversion of colours, as linseed-oil, olive-oil, the essential oils of bergamot, lemon, lavender, pennyroyal, and peppermint, strong nitric acid, and many artificial compounds that I shall presently have occasion to mention.

The dispersive power of fluor spar is the least of any substance yet examined; so that, although its refractive power is also remarkably low, (considering its great specific gravity,) a prism of fluor, in contact with water or alcohol, shows the prismatic colours to be refracted in an inverted order.

With heavy spar, the instances of reversion are very numerous, as its dispersive power is low, and is accompanied with

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* Edinb. Trans. Vol. III.

great refractive density. In the refractions from this spar into flint glass, and into all oils or resins, I believe, without exception, the colours are seen reversed.

Rock crystal likewise disperses so little, that it exhibits the colours reversed, when it is in contact with many substances of less refractive power than itself. I have tried it with Dutch plate-glass, with Canada balsam and balsam of capivi, with many oils essential and expressed, and have found the colours in all these cases reversed.

By solutions of metallic salts, a great variety of such appearances may be produced. Most of these compounds have a highly dispersive power; and many of them may be rendered sufficiently dense to occasion reversion, even when applied to flint-glass. In a more dilute state, they may be used with crown-glass, or plate-glass, to produce the same effect. And since, when further diluted by a less dispersive medium, they will also present an appearance of colourless refraction, we may, by examining the degree of dilution necessary for that purpose, compare the dispersive powers of any ingredients contained in them, and may gradually extend our knowledge of this property to the elements of any bodies, however compounded.

As a specimen of the method, I have in this way compared a few solutions of metals, and of other substances, that were each diluted till the limit of reflection appeared void of colour, when they were in contact with a rectangular piece of plate-glass; and, in the table which follows, I have expressed their refractive powers in that state of dilution, as nearly as the eye can discern the disappearance of colour.

TABLE II.

	In Water.	In Alcohol.
Nitro-muriate of gold - -	1,364	1,390
Nitro-muriate of platina - -	1,370	
Nitrate of iron - - - -	1,375	
Sulphuret of potash - - -	1,375	
Red muriate of iron - - -	1,385	
Nitrate of magnesia		
Nitric acid - - - -	1,395	
Nitrate of jargon		
Balsam of Tolu - - - -	—	1,400
Acetite of litharge (extract of lead) -	1,400	
Nitrate of silver		
Nitrate of copper		
Oil of sassafras - - -	—	1,405
Muriate of antimony - - -	—	1,410
Nitrate of lime - - -	1,410	1,422
Nitrate of zinc		
Green muriate of iron - - -	1,415	
Muriate of magnesia - - -	1,416	
——— of lime - - - -	1,425	1,440
——— of zinc - - -	1,425	
Essence of lemon - - - -	—	1,430
Balsam of capivi - - - -	—	1,440

It may here be seen, that several of the metals increase the dispersive powers of nitric and muriatic acids, and consequently exceed them in that respect. Of all these substances that I have yet tried, gold and platina are the most dispersive. The least dispersive of the metals is zinc.

The earths also are found to possess this property in very different degrees: that of the jargon and magnesia differ but little from nitric acid in dispersive power; but siliceous earth, on the contrary, is inferior to water.

By comparing the salts formed with the nitric and muriatic acids, it appeared probable that the former had the higher dispersive power; but a more direct comparison could not be made by means of the rectangular piece of plate-glass, as muriatic acid could not be rendered sufficiently dense for such a trial; I therefore made use of a triangular prism of crown-glass, which is in itself less dispersive than any plate-glass, and, from the relative position of its surfaces, occasioned less correction of the colours. With this prism, I found that strong muriatic acid (having a refractive power 1,394) exhibited the colours reversed; and that, when it was diluted till the limit of reflection appeared void of colour, its refractive power was reduced to 1,382. But the dispersive power of nitric acid, when tried by the same prism, proved to be greater; for this acid required to be diluted till its refractive power did not exceed 1,375, before the colour was wholly destroyed.

In the table it may be observed, that the red and green muriates of iron, though consisting of the same metal and acid, differ very much in dispersive power; and, consequently, that some caution will be necessary, in attempting to compare the different metals with each other by means of the salts containing them, as any difference observed may be owing in part to a difference in the quantity of acid to which they are united, and in part to their different proportion of oxygen.

A striking instance of the latter is manifest, from a comparison of sulphur with the sulphuric acid; for, while the former appears to exceed the metallic oxides in dispersive power, the latter is inferior even to water.

As I have likewise, at various times, made many experiments on dispersion by means of wedges, in a manner nearly similar to that employed by Mr. DOLLOND, Dr. BLAIR, and others, I have endeavoured to reduce the several substances thus examined to one table; but, as the limits of colour are in few instances sufficiently well defined for accurate mensuration, I have not attempted to add any numerical estimate of their powers, but have merely ascertained the order in which they succeed each other; and, in the following table, have arranged them according to the excess of their effect on violet above red light, at a given angle of deviation.

TABLE III.

Order of dispersive Powers.	Refr. Power.
Sulphur - -	2,04
Glass of lead ($\frac{1}{7}$ sand)	1,987
Balsam of Tolu -	1,60
Oil of sassafras - -	1,536
Muriate of antimony	
Guaiacum - -	1,596
Oil of cloves - -	1,535
Flint-glass - -	1,586
Colophony - -	1,543
Canada balsam -	1,528
Oil of amber - -	1,505
Jargon - -	1,95
Oil of turpentine -	1,47
Copal - - -	1,535
Balsam of capivi -	1,507
Anime - -	1,535
Iceland spar - -	1,657
Amber - -	1,547
Diamond - -	2,44
Alum - - -	1,457
Plate-glass, Dutch -	1,517
Ditto, English -	1,504
Crown glass - -	1,533
Ruby (spinelle) -	1,812
Water - - -	1.336
Sulphuric acid -	1,435
Alcohol - - -	1,37
Sulphate of barytes -	1,046
Selenite - -	1,525
Rock crystal - -	1,547
Sulphate of potash -	1,495
White sapphire -	1,768
Fluor spar - -	1,433

By comparison of this table with the order of refractive powers, as contained in the first table, it will be seen how little correspondence there is between them; and, accordingly, how numerous are the combinations by means of which a pencil of rays that passes through two media, may be made to deviate without dispersion of its colours.

I cannot conclude these observations on dispersion, without remarking that the colours into which a beam of white light is separable by refraction, appear to me to be neither 7, as they usually are seen in the rainbow, nor reducible by any means (that I can find) to 3, as some persons have conceived; but that, by employing a very narrow pencil of light, 4 primary divisions of the prismatic spectrum may be seen, with a degree of distinctness that, I believe, has not been described nor observed before.

If a beam of day-light be admitted into a dark room by a crevice $\frac{1}{20}$ of an inch broad, and received by the eye at the distance of 10 or 12 feet, through a prism of flint-glass, *free from veins*, held near the eye, the beam is seen to be separated into the four following colours only, red, yellowish green, blue, and violet; in the proportions represented in Fig. 3.

The line A that bounds the red side of the spectrum is somewhat confused, which seems in part owing to want of power in the eye to converge red light. The line B, between red and green, in a certain position of the prism, is perfectly distinct; so also are D and E, the two limits of violet. But C, the limit of green and blue, is not so clearly marked as the rest; and there are also, on each side of this limit, other distinct dark lines, *f* and *g*, either of which, in an imperfect experiment, might be mistaken for the boundary of these colours.

The position of the prism in which the colours are most clearly divided, is when the incident light makes about equal angles with two of its sides. I then found that the spaces AB, BC, CD, DE, occupied by them, were nearly as the numbers 16, 23, 36, 25.

Since the proportions of these colours to each other have been supposed by Dr. BLAIR to vary according to the medium by which they are produced, I have compared with this appearance, the coloured images caused by prismatic vessels containing substances supposed by him to differ most in this respect, such as strong but colourless nitric acid, rectified oil of turpentine, very pale oil of sassafras, and Canada balsam, also nearly colourless. With each of these, I have found the same arrangement of these 4 colours, and, in similar positions of the prisms, as nearly as I could judge, the same proportions of them.

But, when the inclination of any prism is altered so as to increase the dispersion of the colours, the proportions of them to each other are then also changed, so that the spaces AC and CE, instead of being as before 39 and 61, may be found altered as far as 42 and 58.*

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* Although what I have above described comprises the whole of the prismatic spectrum that can be rendered visible, there also pass on each side of it other rays, whereof the eye is not sensible. From Dr. HERSCHEL's experiments (Phil. Trans. for 1800) we learn, that on one side there are invisible rays occasioning heat, that are less refrangible than red light; and on the other I have myself observed, (and the same remark has been made by Mr. RITTER,) that there are likewise invisible rays of another kind, that are more refracted than the violet. It is by their chemical effects alone that the existence of these can be discovered; and, by far the most delicate test of their presence is the white muriate of silver.

To SCHEELE, among many valuable discoveries, we are indebted for having first duly distinguished between radiant heat and light; (*Traité de l'Air et du Feu*, § 56, 57;) and to him also we owe the observation, that when muriate of silver is exposed

By candle-light, a different set of appearances may be distinguished. When a very narrow line of the blue light at the lower part of the flame is examined alone, in the same manner, through a prism, the spectrum, instead of appearing a series of lights of different hues contiguous, may be seen divided into 5 images, at a distance from each other. The 1st is broad red, terminated by a bright line of yellow; the 2d and 3d are both green; the 4th and 5th are blue, the last of which appears to correspond with the division of blue and violet in the solar spectrum, or the line D of Fig. 3.

When the object viewed is a blue line of electric light, I have found the spectrum to be also separated into several images; but the phenomena are somewhat different from the preceding. It is, however, needless to describe minutely, appearances which vary according to the brilliancy of the light, and which I cannot undertake to explain.

to the common prismatic spectrum, it is blackened more in the violet than in any other kind of light. (§ 66.) In repeating this experiment, I found that the blackness extended not only through the space occupied by the violet, but to an equal degree, and to about an equal distance, beyond the visible spectrum; and that, by narrowing the pencil of light received on the prism, the discoloration may be made to fall almost entirely beyond the violet.

It would appear therefore, that this and other effects usually attributed to light, are not in fact owing to any of the rays usually perceived, but to invisible rays that accompany them; and that, if we include two kinds that are invisible, we may distinguish, upon the whole, six species of rays into which a sun-beam is divisible by refraction.

XIII. *On the oblique Refraction of Iceland Crystal. By* William Hyde Wollaston, *M. D. F. R. S.*

Read June 24, 1802.

In the preceding communication, I have inserted two different measures of refractive powers, distinctly observable in the Iceland crystal, as well as an estimate of its dispersive power; but have reserved for a separate treatise, some remarks which the same mode of investigation has enabled me to make on its oblique refraction.

The optical properties of this body have been so amply described by Huygens, in his *Traité de la Lumière*, that it could answer little purpose to attempt to make any addition to those which he has enumerated. But, as the law to which he has reduced the oblique refractions occasioned by it, could not be verified by former methods of measurement, without considerable difficulty, it may be worth while to offer a new and easy proof of the justness of his conclusions. For, since the theory by which he was guided in his inquiries, affords (as has lately been shown by Dr. Young[*]) a simple explanation of several phenomena not yet accounted for by any other hypothesis, it must be admitted that it is entitled to a higher degree of consideration than it has in general received.

According to that hypothesis, light proceeding from any luminous centre, is propagated by vibrations of a medium highly

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[*] Bakerian Lecture. Phil. Trans. for 1801.

elastic, that pervades all space. In ordinary cases, the incipient undulations are of a spherical form; but, in the Iceland crystal, light appeared to Huygens to proceed as if the undulations were portions of an oblate spheroid, of which the axis is parallel to the short diagonal of an equilateral piece of the crystal, and its centre the point of incidence of the ray.

From this spheroidical form of the undulations, he deduces the obliquity of refraction; and lays down a law, observable in all refractions, at any surface of the spar, whether natural or artificial, which bears the closest analogy to that which obtains universally at other refracting surfaces; for as, in other cases, the ratio is given between the sine of incidence and sine of refraction, (or ordinate of the *spherical* undulation propagated,) so, in the Iceland crystal, the ratio between the sine of incidence and ordinate of refraction (in any one section of the *spheroidical* undulation) is a given ratio.

If ABD (Fig. 1, Plate XV.) be any surface of the spar, let FHOK be a section of the spheroid through its centre C, and RC any ray of light falling on that surface; draw FO a diameter of the spheroid, in the plane of incidence RVO, and CT, its semiconjugate diameter, in the plane of refraction FTO. Then, if CI be the refracted ray, VR, the sine of incidence, shall be to EI, the ordinate of refraction parallel to FC, in the constant ratio of a given line N to the semidiameter FC.

In any other plane of incidence, the ratio of sine to ordinate is also constant; but it is a different ratio, according to the magnitude of that diameter in which the plane of incidence intersects the ellipse FHOK.

When the incidence of a ray passing from any medium of greater density upon a surface of this spar, is such that the emergent ray becomes parallel to the surface, the ordinate of refraction is then a semidiameter of the spheroid; and, accordingly, the refractive power of this spar, when examined by means of a prism in different directions, should be found to vary as that semidiameter which coincides with the plane of incidence and refracting surface.

The observations that I have made on this substance, accord throughout with this hypothesis of HUYGENS; the measures that I have taken, correspond more nearly than could well happen to a false theory, and are the more to be depended on, as all my experiments, excepting the last, were made prior to my acquaintance with the theory, and their agreement was deduced by subsequent computation.

Exp. 1. The oblique refraction of this spar is rendered visible, by cementing a surface of it to a prism of flint-glass, with a little balsam of Tolu. When the line of sight bisects an acute angle of a natural surface of the spar, the refractive power is seen to be less than in any other direction, and may be expressed by the sine 1,488, or its reciprocal 0,67204.

Exp. 2. When the plane of incidence is parallel to one of the sides, the power is 1,518, of which the reciprocal is 0,6587.

Exp. 3. In a direction at right angles with either side, it is found still higher, being 1,537, or its reciprocal 0,6506.

Exp. 4. And, in the plane bisecting an obtuse angle, the refractive power of the natural surface appears greatest, and is expressed by the sine 1,571, or its reciprocal 0,6365.

Exp. 5. When either of the two greatest solid angles of the spar contained under three obtuse angles, is cut off by a polished surface making equal angles with each of its sides, the same refractive power 1,488 is found in all directions. By the theory also, the section of the spheroid is in this case a circle, and every semidiameter (FC) the same, since the plane is at right angles to the minor axis.

Exp. 6. If a plane surface be formed bisecting an obtuse angle of the spar, and applied to a prism, the same minimum of refraction 1,488, is found in a direction that coincides with the preceding plane, and therefore with the major axis of the generating ellipse; but, as the direction is varied, it increases so rapidly as soon to exceed the power of glass, and to be no longer ascertainable by the angle of incipient reflection.

Exp. 7. The regular refraction of this spar is also too great for examination by means of any prism, for want of a medium of union of sufficient density; but, by trial in the usual method, it measured, on an average of several experiments, 1,657, or its reciprocal 0,6035.

By assuming, as HUYGENS has done, the equality of this power with the maximum of the oblique refraction, we have sufficient data for construction of the spheroid by which the refractions are regulated; for we have 0,67204 (*Exp.* 1.) as major axis of the generating ellipse, and 0,6035 (*Exp.* 7.) will be the minor axis, parallel in position to the short axis of the spar.

The angle of inclination of this axis to the surfaces of the spar, if supposed to be equilateral, may be computed by spherical trigonometry, from any other angle that has been ascertained by measurement.

The measures that I have taken are not exactly those of HUYGENS; but I nevertheless hold them in equal estimation, from the conformity which I find they bear to each other, by assistance of his theory.

Exp. 8. I measured with care, an angle at which two surfaces of the spar are inclined to each other, and found it to be 105° 5′. Hence, the greater angle of the surfaces themselves may be computed to be 101° 55′; and the angle which the short axis makes with each plane surface is 45° 23′ 25″.

If GSMP (Fig. 2.) be a plane bisecting an obtuse angle of the spar, the section of the spheroid in that plane passes through the axis CS, and therefore is the generating ellipse. By calculating from the known dimensions of its major axis CP 0,67204, its minor axis CS 0,6035, and the angle GCS = 45° 23′ 25″, CG will be found* to be 0,6365, of which the reciprocal is 1,5736, differing but little from 1,571, as it appeared by measurement. (*Exp.* 4.)

Again, if ABDE (Fig. 4.) be one of the natural surfaces, and PG*p* the ellipse formed by that section of the spheroid, PC being as before 0,67204, and CG 0,6365, the reciprocal of 1,571 found by measurement, (*Exp.* 4.) then the semidiameter CT, parallel to the side AE, which makes an angle TCP 39° 2½′, will be found to be 0,6573, instead of 0,6587, and its reciprocal 1,5215, instead of 1,518. (*Exp.* 2.)

The semidiameter also, in the direction of CL, perpendicular to the side, at an angle LCP 50° 57½′, is found by calculation 0,650, and its reciprocal 1,539, instead of 0,6506 and 1,537. (*Exp.* 3.)

From the foregoing data, the course of a ray perpendicular to the surface of the spar may likewise be computed; for, since the sine of incidence is then nothing, the ordinate of refraction must be also nothing, and the ray will be refracted along the semiconjugate diameter CM. (Fig. 2.)

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* (Fig. 3.) CS : CP :: tang. PCG : tang. PC*p*.
sec. PC*p* : sec. PCG :: CP : CG.

By calculation,* the angle which this conjugate makes with the perpendicular is 6° 7½′. But, by the following measurement, it appears to be 6° 16′.

Exp. 9. A piece of spar that measured 1,145 inch in thickness, was laid upon a line, and showed two images that were removed from each other $\frac{126}{1000}$ of an inch. Then, as 1,145 : 0,126 : : radius : tang. of 6° 16′.

The different results deduced from theory and from observation, will be seen at one view in the following statement.

In *Exp.* 2d,	observed	1,518 ;	calculated	1,5215.
3d,	———	1,537	———	1,539.
4th,	———	1,571	———	1,5736.
9th, angle observed		6° 16′	- -	6° 7½′.

The angle observed differs from that obtained by computation, in a greater degree than any of the former measures; but, when the difficulty of measuring this angle with accuracy is considered, and also the greater effect of any incorrectness in the data from which a semiconjugate is computed, I think the result of this, as well of the preceding comparisons, must be admitted to be highly favourable to the HUYGENIAN theory; and, although the existence of two refractions at the same time, in the same substance, be not well accounted for, and still less their interchange with each other, when a ray of light is made to pass through a second piece of spar situated transversely to the first, yet the oblique refraction, when considered alone, seems nearly as well explained as any other optical phenomenon.

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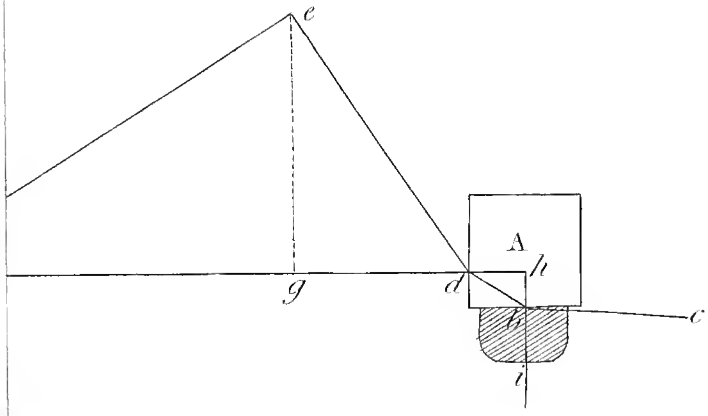
* (Fig. 5.) CS : CP : : tang. PCG : tang. *p*CO or co-tang. PCQ;
then CP : CS : : tang. PCQ : tang. PCM;
and LCP — PCM = MCL.



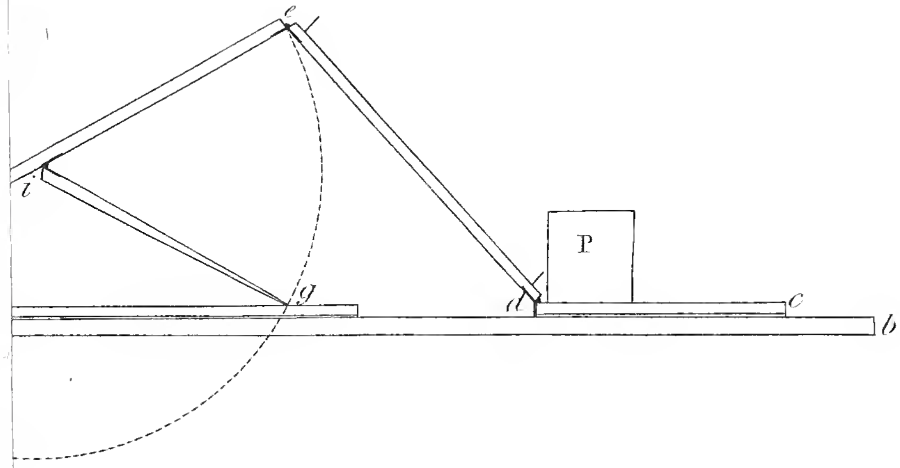
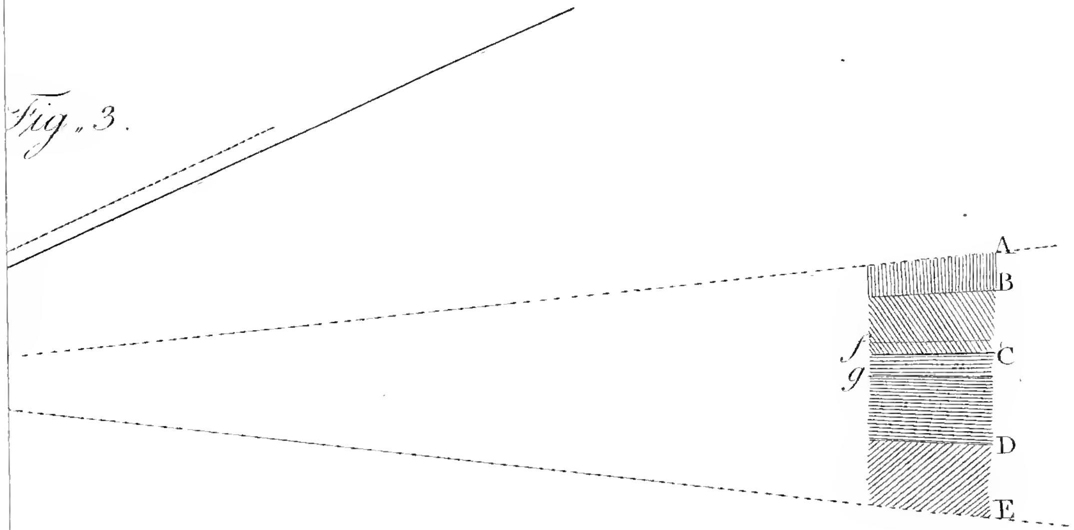
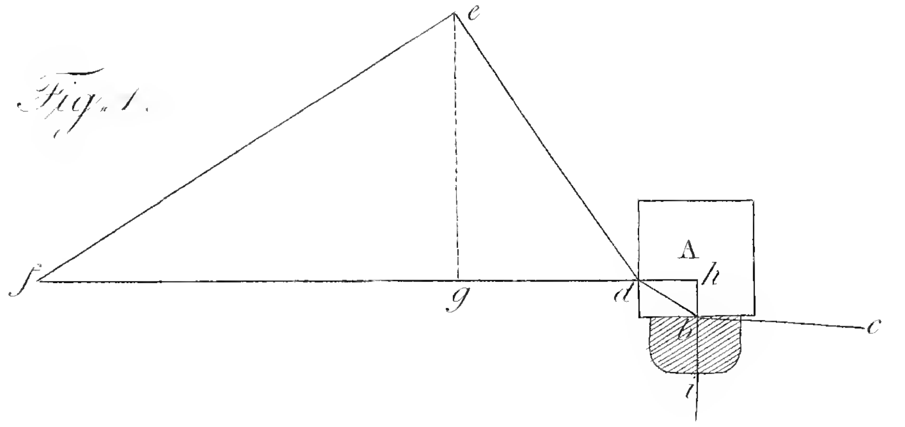


Fig. 3.



Basire sc.

Fig. 1.



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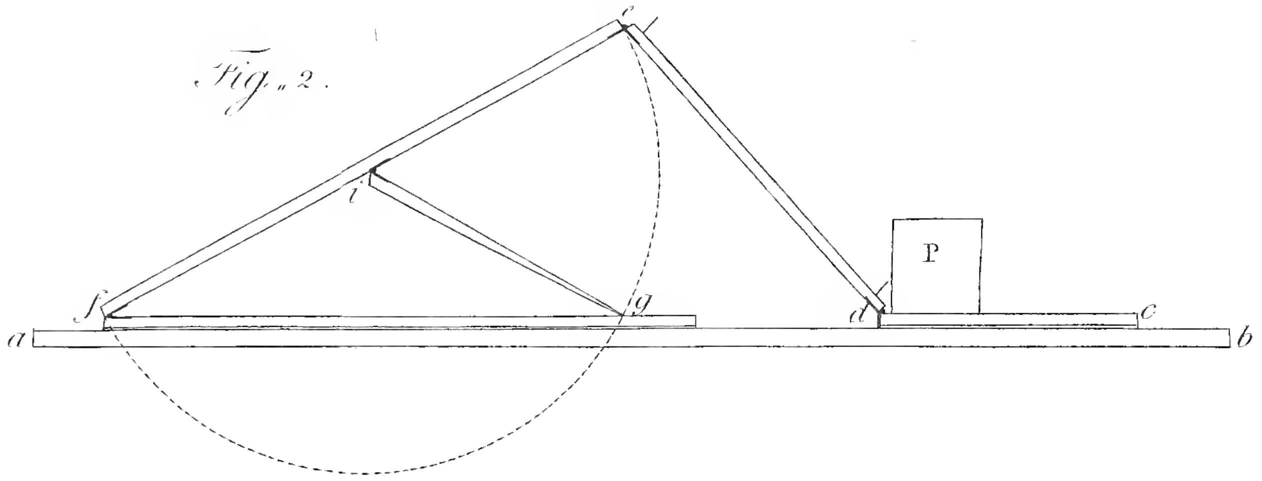

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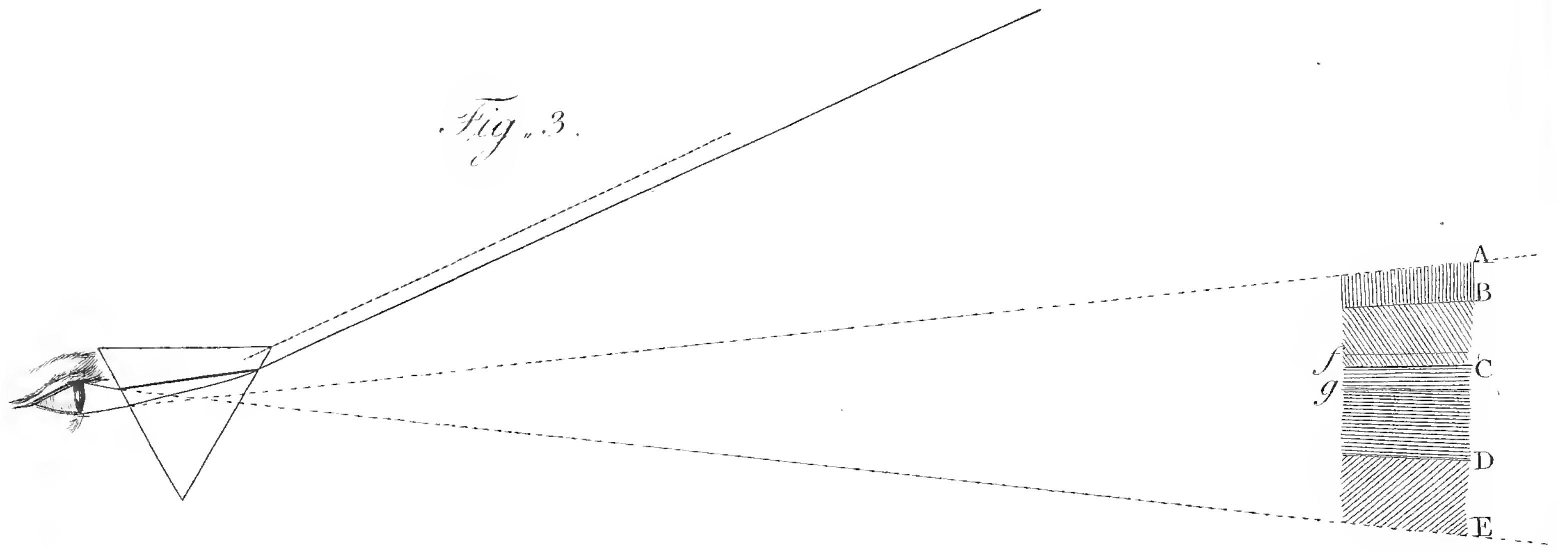

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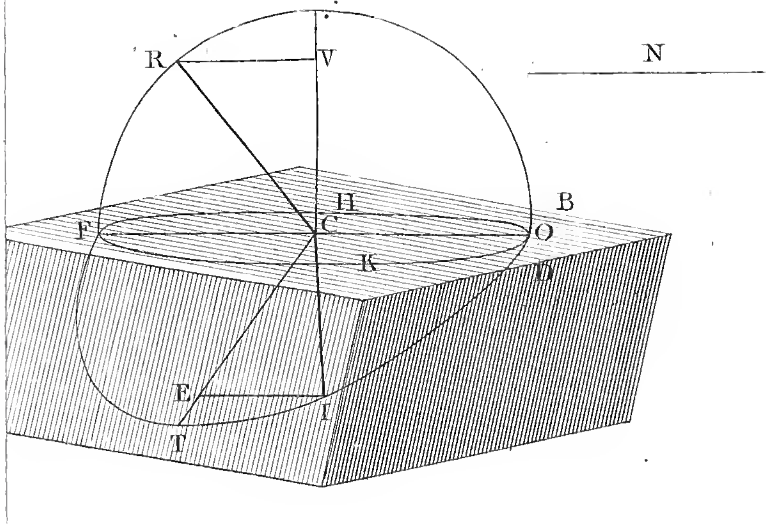
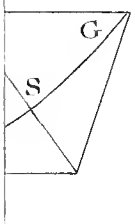




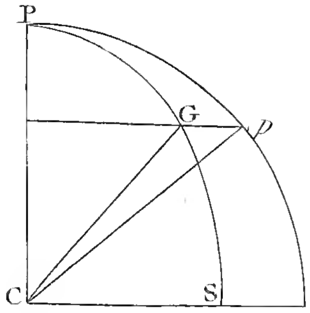
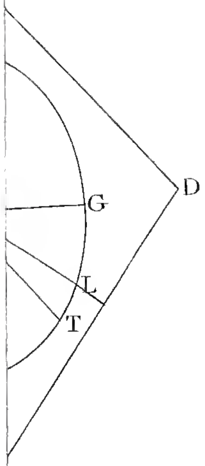


Fig. 3.



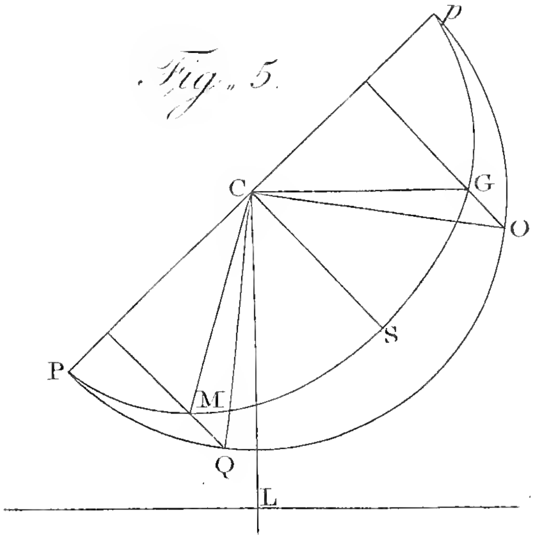


Fig. 5.

Fig. 1.

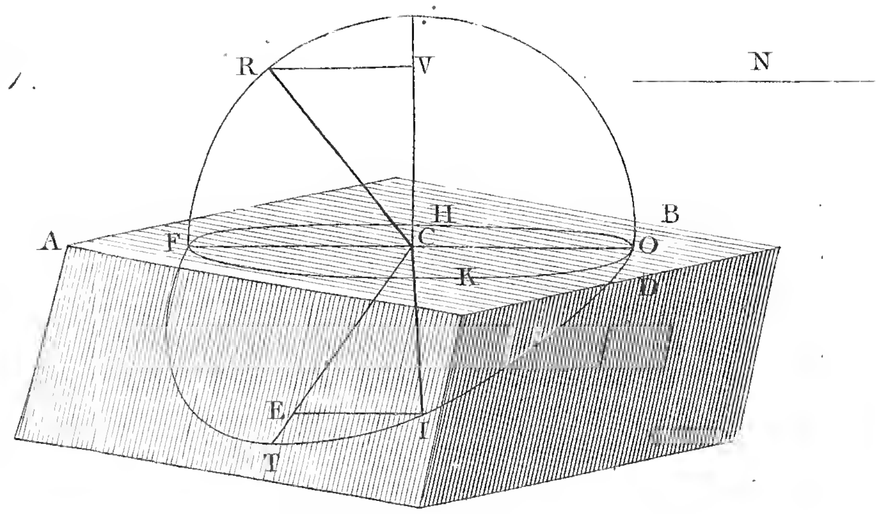
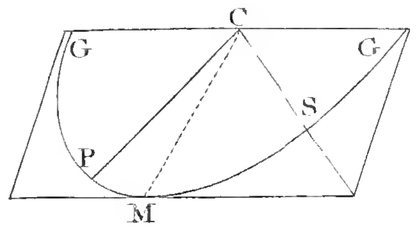


Fig. 2.



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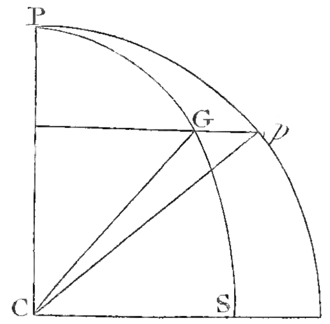


Fig. 4.

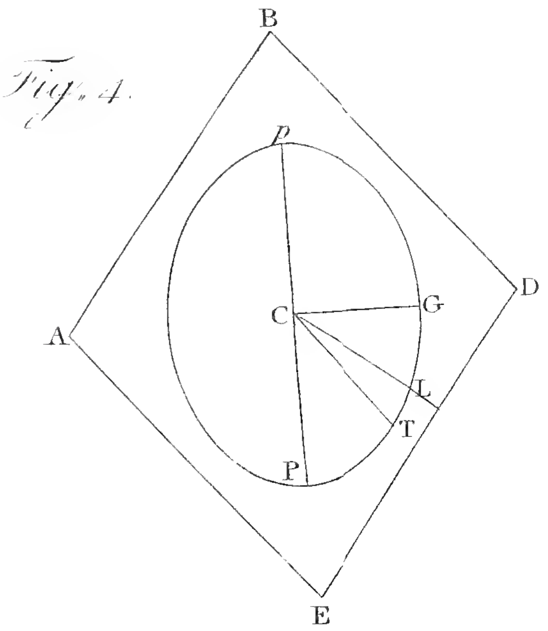
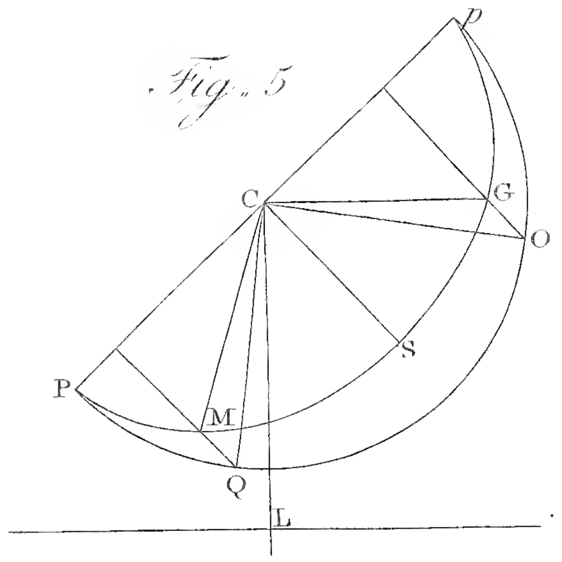


Fig. 5.



XIV. *An Account of some Cases of the Production of Colours, not hitherto described. By* Thomas Young, *M. D. F. R. S. F. L. S. Professor of Natural Philosophy in the Royal Institution.*

Read July 1, 1802.

WHATEVER opinion may be entertained of the theory of light and colours which I have lately had the honour of submitting to the Royal Society, it must at any rate be allowed that it has given birth to the discovery of a simple and general law, capable of explaining a number of the phenomena of coloured light, which, without this law, would remain insulated and unintelligible. The law is, that "wherever two portions of the same "light arrive at the eye by different routes, either exactly or "very nearly in the same direction, the light becomes most "intense when the difference of the routes is any multiple of a "certain length, and least intense in the intermediate state of "the interfering portions; and this length is different for light "of different colours."

I have already shown in detail, the sufficiency of this law for explaining all the phenomena described in the second and third books of NEWTON's Optics, as well as some others not mentioned by NEWTON. But it is still more satisfactory to observe its conformity to other facts, which constitute new and distinct classes of phenomena, and which could scarcely have agreed so well with any anterior law, if that law had been erroneous or imaginary: these are, the colours of fibres, and the colours of mixed plates.

As I was observing the appearance of the fine parallel lines of light which are seen upon the margin of an object held near the eye, so as to intercept the greater part of the light of a distant luminous object, and which are produced by the fringes caused by the inflection of light already known, I observed that they were sometimes accompanied by coloured fringes, much broader and more distinct; and I soon found, that these broader fringes were occasioned by the accidental interposition of a hair. In order to make them more distinct, I employed a horse-hair; but they were then no longer visible. With a fibre of wool, on the contrary, they became very large and conspicuous: and, with a single silk-worm's thread, their magnitude was so much increased, that two or three of them seemed to occupy the whole field of view. They appeared to extend on each side of the candle, in the same order as the colours of thin plates, seen by transmitted light. It occurred to me, that their cause must be sought in the interference of two portions of light, one reflected from the fibre, the other bending round its opposite side, and at last coinciding nearly in direction with the former portion; that, accordingly as both portions deviated more from a rectilinear direction, the difference of the length of their paths would become gradually greater and greater, and would consequently produce the appearances of colour usual in such cases; that, supposing them to be inflected at right angles, the difference would amount nearly to the diameter of the fibre, and that this difference must consequently be smaller as the fibre became smaller; and, the number of fringes in a right angle becoming smaller, that their angular distances would consequently become greater, and the whole appearance would be dilated. It was easy to calculate, that for the light least inflected, the difference of the paths would be to the diameter of the fibre, very nearly as the deviation of the ray, at any point, from the rectilinear direction, to its distance from the fibre.

I therefore made a rectangular hole in a card, and bent its ends so as to support a hair parallel to the sides of the hole: then, upon applying the eye near the hole, the hair of course appeared dilated by indistinct vision into a surface, of which the breadth was determined by the distance of the hair and the magnitude of the hole, independently of the temporary aperture of the pupil. When the hair approached so near to the direction of the margin of a candle that the inflected light was sufficiently copious to produce a sensible effect, the fringes began to appear; and it was easy to estimate the proportion of their breadth to the apparent breadth of the hair, across the image of which they extended. I found that six of the brightest red fringes, nearly at equal distances, occupied the whole of that image. The breadth of the aperture was $\frac{66}{1000}$, and its distance from the hair $\frac{8}{10}$ of an inch: the diameter of the hair was less than $\frac{1}{500}$ of an inch; as nearly as I could ascertain, it was $\frac{1}{600}$. Hence, we have $\frac{11}{1000}$ for the deviation of the first red fringe at the distance $\frac{8}{10}$; and, as $\frac{8}{10} : \frac{11}{1000} :: \frac{1}{600} : \frac{11}{480000}$, or $\frac{1}{43636}$ for the difference of the routes of the red light where it was most intense. The measure deduced from Newton's experiments is $\frac{1}{39200}$. I thought this coincidence, with only an error of one-ninth of so minute a quantity, sufficiently perfect to warrant completely the explanation of the phenomenon, and even to render a repetition of the experiment unnecessary; for there are several circumstances which make it difficult to calculate much more precisely what ought to be the result of the measurement.

When a number of fibres of the same kind, for instance, a uniform lock of wool, are held near to the eye, we see an appearance of halos surrounding a distant candle; but their brilliancy, and even their existence, depends on the uniformity of the dimensions of the fibres; and they are larger as the fibres are smaller. It is obvious that they are the immediate consequences of the coincidence of a number of fringes of the same size, which, as the fibres are arranged in all imaginable directions, must necessarily surround the luminous object at equal distances on all sides, and constitute circular fringes.

There can be little doubt that the coloured atmospherical halos are of the same kind: their appearance must depend on the existence of a number of particles of water, of equal dimensions, and in a proper position, with respect to the luminary and to the eye. As there is no natural limit to the magnitude of the spherules of water, we may expect these halos to vary without limit in their diameters; and, accordingly, Mr. JORDAN has observed that their dimensions are exceedingly various, and has remarked that they frequently change during the time of observation.

I first noticed the colours of mixed plates, in looking at a candle through two pieces of plate-glass, with a little moisture between them. I observed an appearance of fringes resembling the common colours of thin plates; and, upon looking for the fringes by reflection, I found that these new fringes were always in the same direction as the other fringes, but many times larger. By examining the glasses with a magnifier, I perceived that wherever these fringes were visible, the moisture was intermixed with portions of air, producing an appearance similar to dew. I then supposed that the origin of the colours was the same as that of the colours of halos; but, on a more minute examination, I found that the magnitude of the portions of air and water was by no means uniform, and that the explanation was therefore inadmissible. It was, however, easy to find two portions of light sufficient for the production of these fringes; for, the light transmitted through the water, moving in it with a velocity different from that of the light passing through the interstices filled only with air, the two portions would interfere with each other, and produce effects of colour according to the general law. The ratio of the velocities in water and in air, is that of 3 to 4; the fringes ought therefore to appear where the thickness is 6 times as great as that which corresponds to the same colour in the common case of thin plates; and, upon making the experiment with a plane glass and a lens slightly convex, I found the sixth dark circle actually of the same diameter as the first in the new fringes. The colours are also very easily produced, when butter or tallow is substituted for water; and the rings then become smaller, on account of the greater refractive density of the oils: but, when water is added, so as to fill up the interstices of the oil, the rings are very much enlarged; for here the difference only of the velocities in water and in oil is to be considered, and this is much smaller than the difference between air and water. All these circumstances are sufficient to satisfy us with respect to the truth of the explanation; and it is still more confirmed by the effect of inclining the plates to the direction of the light; for then, instead of dilating, like the colours of thin plates, these rings contract: and this is the obvious consequence of an increase of the length of the paths of the light, which now traverses both mediums obliquely; and the effect is every where the same as that of a thicker plate.

It must however be observed, that the colours are not produced in the whole light that is transmitted through the mediums: a small portion only of each pencil, passing through the water contiguous to the edges of the particle, is sufficiently coincident with the light transmitted by the neighbouring portions of air, to produce the necessary interference; and it is easy to show that, on account of the natural concavity of the surface of each portion of the fluid adhering to the two pieces of glass, a considerable portion of the light which is beginning to pass through the water will be dissipated laterally by reflection at its entrance, and that much of the light passing through the air will be scattered by refraction at the second surface. For these reasons, the fringes are seen when the plates are not directly interposed between the eye and the luminous object; and, on account of the absence of foreign light, even more distinctly than when they are in the same right line with that object. And, if we remove the plates to a considerable distance out of this line, the rings are still visible, and become larger than before; for here the actual route of the light passing through the air, is longer than that of the light passing more obliquely through the water, and the difference in the times of passage is lessened. It is however impossible to be quite confident with respect to the causes of these minute variations, without some means of ascertaining accurately the forms of the dissipating surfaces.

In applying the general law of interference to these colours, as well as to those of thin plates already known, I must confess that it is impossible to avoid another supposition, which is a part of the undulatory theory, that is, that the velocity of light is the greater, the rarer the medium; and that there is also a condition annexed to the explanation of the colours of thin plates, which involves another part of the same theory, that is, that where one of the portions of light has been reflected at the surface of a rarer medium, it must be supposed to be retarded one half of the appropriate interval, for instance, in the central black spot of a soap-bubble, where the actual lengths of the paths very nearly coincide, but the effect is the same as if one of the portions had been so retarded as to destroy the other. From considering the nature of this circumstance, I ventured to predict, that if the two reflections were of the same kind, made at the surfaces of a thin plate, of a density intermediate between the densities of the mediums containing it, the effect would be reversed, and the central spot, instead of black, would become white; and I have now the pleasure of stating, that I have fully verified this prediction, by interposing a drop of oil of sassafras between a prism of flint-glass and a lens of crown glass: the central spot seen by reflected light was white, and surrounded by a dark ring. It was however necessary to use some force, in order to produce a contact sufficiently intimate; and the white spot differed, even at last, in the same degree from perfect whiteness, as the black spot usually does from perfect blackness.

The colours of mixed plates suggested to me an idea which appears to lead to an explanation of the dispersion of colours by refraction, more simple and satisfactory than that which I advanced in the last BAKERIAN lecture. We may suppose that every refractive medium transmits the undulations constituting light in two separate portions, one passing through its ultimate particles, and the other through its pores; and that these portions re-unite continually, after each successive separation, the one having preceded the other by a very minute but constant interval, depending on the regular arrangement of the particles of a homogeneous medium. Now, if these two portions were always equal, each point of the undulations resulting from their re-union, would always be found half way between the places of the corresponding point in the separate portions; but, supposing the preceding portion to be the smaller, the newly combined undulation will be less advanced than if both had been equal, and the difference of its place will depend, not only on the difference of the length of the two routes, which will be constant for all the undulations, but also on the law and magnitude of those undulations; so that the larger undulations will be somewhat further advanced after each re-union than the smaller ones, and, the same operation recurring at every particle of the medium, the whole progress of the larger undulations will be more rapid than that of the smaller; hence the deviation, in consequence of the retardation of the motion of light in a denser medium, will of course be greater for the smaller than for the larger undulations. Assuming the law of the harmonic curve for the motions of the particles, we might without much difficulty reduce this conjecture to a comparison with experiment; but it would be necessary, in order to warrant our conclusions, to be provided with very accurate measures of the refractive and dispersive powers of various substances, for rays of all descriptions.

Dr. WOLLASTON's very interesting observations would furnish great assistance in this inquiry, when compared with the separation of colours by thin plates. I have repeated his experiments on the spectrum with perfect success, and have made some attempts to procure comparative measures from thin plates; and I have found that, as Sir ISAAC NEWTON has already observed, the blue and violet light is more dispersed by refraction, than in proportion to the difference of the appropriate dimensions deduced from the phenomena of thin plates. Hence it happens, that when a line of the light proceeding to form an image of the rings of colours of thin plates, is intercepted by a prism, and an actual picture is formed, resembling the scale delineated by NEWTON from theory, for estimating the colours of particles of given dimensions, the oblique spectrums, formed by the different colours of each series, are not straight, but curved, the lateral refraction of the prism separating the violet end more widely than the red. The thickness corresponding to the extreme red, the line of yellow, bright green, bright blue, and extreme violet, I found to be inversely as the numbers 27, 30, 35, 40, and 45, respectively. In consequence of Dr. WOLLASTON'S correction of the description of the prismatic spectrum, compared with these observations, it becomes necessary to modify the supposition that I advanced in the last BAKERIAN lecture, respecting the proportions of the sympathetic fibres of the retina; substituting red, green, and violet, for red, yellow, and blue, and the numbers 7, 6, and 5, for 8, 7, and 6.

The same prismatic analysis of the colours of thin plates, appears to furnish a satisfactory explanation of the subdivision of the light of the lower part of a candle: for, in fact, the light transmitted through every part of a thin plate, is divided in a similar manner into distinct portions, increasing in number with the thickness of the plate, until they become too minute to be visible. At the thickness corresponding to the ninth or tenth portion of red light, the number of portions of different colours is five; and their proportions, as exhibited by refraction, are nearly the same as in the light of a candle, the violet being the broadest. We have only to suppose each particle of tallow to be, at its first evaporation, of such dimensions as to produce the same effect as the thin plate of air at this point, where it is about $\frac{1}{10000}$ of an inch in thickness, and to reflect, or perhaps rather to transmit, the mixed light produced by the incipient combustion around it, and we shall have a light completely resembling that which Dr. WOLLASTON has observed. There appears to be also a fine line of strong yellow light, separate from the general spectrum, principally derived from the most superficial combustion at the margin of the flame, and increasing in quantity as the flame ascends. Similar circumstances might undoubtedly be found in other cases of the production or modification of light; and experiments upon this subject might tend greatly to establish the NEWTONIAN opinion, that the colours of all natural bodies are similar in their origin to those of thin plates; an opinion which appears to do the highest honour to the sagacity of its author, and indeed to form a very considerable step in our advances towards an acquaintance with the intimate constitution and arrangement of material substances.

I have lately had an opportunity of confirming my former observations on the dispersive powers of the eye. I find that, at the respective distances of 10 and 15 inches, the extreme red and extreme violet rays are similarly refracted, the difference being expressed by a focal length of 30 inches. Now the interval between red and yellow is about one-fourth of the whole spectrum; consequently, a focal length of 120 inches expresses a power equivalent to the dispersion of the red and yellow, and this differs but little from 132, which was the result of the observation already described. I do not know that these experiments are more accurate than the former one; but I have repeated them several times under different circumstances, and I have no doubt that the dispersion of coloured light in the human eye is nearly such as I have stated it. How it happens to be no greater, I cannot at present undertake to explain.

CORRECTION OF A FORMER PAPER.

In the Philosophical Transactions for 1800,
  P. 146, line 12, for 83810, read 84197;
  ——— line 15, for .0011562, read .0010116.
In Fig. 53, (Plate VII.) the E*b* (Q) is too near D;
  and the E*b* (Y) should be above, instead of below it.

XV. *On the Composition of Emery. By* Smithson Tennant, *Esq. F. R. S.*

Read July 1, 1802.

The substance called emery, which, from its great hardness, has been long used in various manufactures, for grinding and polishing other bodies, has not, it appears, been hitherto correctly analyzed. In books of mineralogy, it is considered as an ore of iron; an opinion probably derived from its great specific gravity, as well as from the iron which it frequently contains. But, where this metal is most abundant, it could not be extracted from it with advantage, and ought rather to be regarded as an impurity, as it does not contribute to produce the peculiar hardness for which this substance is distinguished. In Mr. Kirwan's mineralogy, he mentions an examination of emery made by Mr. Wiegleb, from which he inferred that 100 parts consisted of 95,6 of silex, and 4,4 of iron. Mr. Kirwan, however, justly suspects the correctness of this account, and observes that he had no doubt but some other stone was imposed upon Mr. Wiegleb for emery.

When powder of emery is boiled in acids, it becomes of a lighter colour, from the loss of part of the iron; after which, it does not seem to undergo any further alteration. As acids produce so little effect on it, I exposed it to a pretty strong red heat, with carbonate of soda, in a crucible of platina. On adding water to the mass contained in the crucible, the greater part of the emery was found in powder; having only become of a light colour, from the extraction of part of the iron. Though this process was twice repeated with the remaining powder, and in a stronger heat, a great proportion of it remained undissolved.

The alkaline solution, after a red calx of iron had subsided from it, was saturated with acid; and gave a precipitate of a white earth, which I found to be almost purely argillaceous.

The result of these experiments, was so similar to those of Mr. KLAPROTH on diamond spar, as to render it very probable that emery was in reality the same substance, though usually mixed with a larger proportion of iron; and the subsequent experiments appear to confirm this opinion.

In order to obtain a quantity of emery as free from iron as I could, I reduced to a coarse powder, a piece which consisted of different strata, some of which were of much lighter colour than others; and afterwards separated, by a magnet, the particles which were attracted by it. The part which was not attracted by the magnet, I observed to have the usual degree of hardness, (by the scratches which might be made with it on flint.) I then reduced it to a finer powder, in an agate mortar; and, as this was principally done by pressure, and not by grinding, hardly any sensible addition was made to its weight. In the same manner, I found that diamond spar might be powdered to the same degree of fineness, without any material increase of weight from the mortar.

Of the emery powder thus prepared, 20 grains were taken, and heated in the manner before described, with 120 grains of soda, which had been previously deprived of carbonic acid, and boiled to dryness in a silver pan. By nearly the same process as that used by Mr. KLAPROTH, I obtained about 16,0 grains of argillaceous earth, ,6 of siliceous earth, ,8 or ,9 of iron, and ,6 of a grain remained undissolved. These numbers, reduced to parts of a hundred, are therefore,

Argillaceous earth - - -	80
Silex - - - - -	3
Iron - - - - -	4
Undissolved - - - -	3
	90.

Mr. KLAPROTH obtained from the Chinese corundum, after separating from it the particles which were attracted by the magnet,

Argillaceous earth - - -	84
Silex - - - -	6,5
Iron - - - - -	7,5
	98.

As this analysis was no doubt conducted with greater care than mine, the loss of weight was less; but the proportion of the ingredients is sufficiently near to show that the substances are essentially the same.

From 25 grains of emery which appeared the most impregnated with iron, and yet retained its usual hardness, I obtained, argillaceous earth 12,5, silex 2, iron 8, and one grain was not dissolved; or, per cent.

Argillaceous earth - - -	50
Silex - - - - -	8
Iron - - - - -	32
Undissolved - - - -	4
	94.

As such emery can easily be had of uniform quality in large pieces, I procured the powder employed in this experiment, by rubbing two pieces against each other.

From 25 grains of emery, similar in appearance to the preceding, but which had been digested with marine acid previous to the action of the alkali, I had,

		per cent.
Of argillaceous earth - -	16,4	65,6
Siliceous earth - - -	,8	3,2
Iron - - - -	2,	8,
Not dissolved - - -	4,5	18,0
	23,7	94,8.

The hardness of emery, as far as I could judge by its cutting rock crystal and flint, appeared to be equal to that of diamond spar. The latter could not be scratched by the former; but, as emery has not a surface sufficiently polished to render a mark visible, the converse of this could not be tried.

All the emery which is used in England, is said to be brought from the Islands of the Archipelago, and principally from Naxos. In those places, it is probably very abundant; as the price of it in London, which I was told was 8 or 10 shillings the hundred weight, appears little more than sufficient for the charges of carriage. Though I saw a very large quantity in one place, (more than a thousand hundred weight,) I could not find any pieces of a crystallized form; possibly the great proportion of iron usually mixed with it, may prevent its crystallization. The whole consisted of angular blocks incrusted with iron ore, sometimes of an octaedral form, with pyrites, and very often with mica. The latter frequently penetrates the whole substance of the mass, giving it, when broken, a silvery appearance, if seen in the direction in which the flat surfaces present themselves to the eye. As these substances have no chemical relation to the emery itself, it is remarkable that they should also accompany the diamond spar from China; for Mr. KLAPROTH observes, "that its lateral facets are mostly coated with a "firmly-adhering crust of micaceous scales, of a silvery lustre:" he also mentions, besides felspar, pyrites, and grains of magnetic iron ore.

XVI. *Quelques Remarques sur la Chaleur, et sur l'Action des Corps qui l'interceptent.* *Par* P. Prevost, *Professeur de Philosophie à Genève, &c.* *Communicated by* Thomas Young, *M. D. F. R. S.*

Read July 1, 1802.

Partie I.

§ 1. Le Dr. Herschel, voulant estimer la quantité de lumière transmise par divers corps, a employé un appareil dont il donne la description détaillée.* Aû moyen de cet appareil, il apprécie l'éffet d'une même source de chaleur, agissant d'un côté sans obstacle, et de l'autre à travers une lame qui l'arréte en partie. Qu'on se représente un rayon solaire tombant sur deux thermométres pareils, sur l'un directement, et sur l'autre à travers un verre; qu'on écarte soigneusement toutes les causes étrangères qui pourroient influer ici; et l'on aura une idée de cet appareil, construit avec tout le soin et toute la sagacité qu'on a droit d'attendre d'un excellent observateur.

Ce physicien a fait avec cet appareil un grand nombre d'expériences, toutes de même forme. Chacune d'elles offre six observations, pour chacun des deux thermométres. La 1re observation indique le dégré au commencement de l'expérience, et avant que la source de chaleur ait pu agir; les autres indiquent successivement, de minute en minute, les dégrés de la chaleur croissante, jusqu'à la 5me minute, époque où finit

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* Phil. Trans. for 1800. p. 446.

l'expérience.[*] Ces nombreuses expériences ne différent entr'elles, que par la nature du corps dont est formée la lame interceptante, ou par la nature de la source de chaleur qui est employée.

A la fin de chaque expérience, l'auteur en donne le résultat. Pour cet effet, il retranche le dégré initial du dégré final, et, faisant séparément cette soustraction pour chacun des deux thermométres, il se borne à comparer les restes, pour en conclure la transmission.

Voici le détail de la 1re des expériences de ce genre, qui est la 24me de l'ouvrage.

	Au soleil direct.			A travers un verre blanc bleuâtre.
0′	67°	-	-	67°
1	$68\frac{3}{4}$	-	-	$68\frac{1}{8}$
2	$70\frac{1}{8}$	-	-	$69\frac{1}{8}$
3	$71\frac{3}{8}$	-	-	70
4	$72\frac{3}{8}$	-	-	$70\frac{7}{8}$
5	73	-	-	$71\frac{1}{2}$.

Soustrayant donc le dégré initial du final, on a au soleil direct 6° de chaleur acquise; tandis qu'à travers le verre on n'en a que $4\frac{1}{2}$. Le rapport de ce dernier nombre au premier, répresente la transmission par le verre. C'est en milliėmes 0,750; dont le complément 0,250 exprime les rayons interceptés.

§ 2. Les expériences multipliées dont je viens de donner sommairement l'idée, ont été faites surement avec toute l'exactitude qui peut leur donner du prix: elles ouvrent un nouveau champ de spéculation, et font espérer des résultats intéressans. Mais celui que l'auteur a eu en vue, je veux dire, l'appréciation

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[*] Une partie de ces expériences n'a duré que trois minutes: je ne citerai pas cette classe d'experiences, c'est pourquoi j'emploie ici une expression générale.

de la faculté interceptante de la lame mise en expérience, n'est pas aussi simple qu'il le paroît au premier coup-d'œil, et exige une nouvelle recherche.

Et d'abord, si de l'expérience que j'ai citée et transcrite ci-dessus, on pouvoit inférer d'une manière générale, que le nombre des rayons interceptés par le verre blanc bleuâtre est exprimé par la fraction 0,250, cela ne devroit pas être particulier au temps de la durée de l'experience. Si, par exemple, elle avoit duré six ou sept minutes, on devroit trouver le même résultat; le même encore, si elle n'avoit duré que trois ou quatre minutes. Nous ne pouvons parler expérimentalement que de ce dernier cas, qui est consigné dans le régistre de l'expérience. Or, il est facile de voir, que si l'auteur s'étoit arrêté à la 4me minute, il auroit trouvé la transmission exprimée par le rapport $3\frac{7}{8} : 5\frac{3}{8} = 0{,}720$, et par conséquent la faculté interceptante $=$ 0,280. S'il se fût arrêté à la 3me minute, l'interception eût été 0,314; a la 2de minute, 0,320; et à la 1re minute, 0,357. Ensorte qu'on devroit croire, en suivant la marche tenue ici, qu'à la 1re minute le verre interceptoit plus de rayons qu'à la 2de; plus à la 2de qu'à la 3me; à la 3me qu'à la 4me; à la 4me qu'à la 5me.

Le même résultat, à une seule irrégularité près,* peut se déduire de l'expérience suivante, (où l'auteur a substitué une lame de *flint glass* à celle de verre blanc bleuâtre de la précédente, et dont il donne le détail,) que je transcrirai ci-dessous. (§ 9.) L'auteur dit lui-même, que ce résultat a été commun à toutes les expériences analogues, à l'exception d'une seule, qui lui a paru anomale à d'autres égards.† Il emploie même ce fait

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* A la fin de la 3me minute, la transmission a été excédente.

† P. 479. Exp. 122. Je rapporterai bientôt cette expérience en détail. (§ 11.)

comme un argument, pour prouver que les rayons chauds et lumineux sont différens.* On peut donc envisager ce résultat comme général et constant. Ensorte que, si la mesure de la transmission adoptée ici est juste, on doit croire que la faculté interceptante des corps va toujours en croissant, de minute en minute, au moins jusqu'à la 5me ; et, si jamais un raisonnement analogique est admissible, on doit croire que cette progression croissante dureroit après les cinq minutes écoulées ; si bien qu'en prolongeant l'expérience, on fait croître la faculté interceptante de la lame, et la chaleur transmise doit à la longue diminuer beaucoup, ou même enfin se réduire à rien.

Cette conséquence, qui est inévitable dans cette méthode d'estimation, doit peut-être inspirer du doute sur le principe dont elle dérive ; car on ne sauroit concevoir aucune raison probable, pour laquelle la faculté de transmettre ou d'intercepter la chaleur doive varier dans un corps, parcequ'il y a plus ou moins long-temps qu'il la transmet ou l'intercepte.

Cette difficulté nous ramène à la théorie de la chaleur, et en particulièr de la communication de la chaleur, de son passage d'un lieu dans un autre, ou d'un corps dans un autre corps. En effet on voit ici, de part et d'autre, la boule d'un thermométre plongée dans une source de chaleur, telle qu'un rayon solaire, par exemple ; on voit la chaleur passer de la source dans la boule, et amener celle-ci, peu-à-peu, à une température plus haute que celle dont elle jouissoit ; on voit qu'il s'écoule plusieurs

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* P. 522. " L'Interception de la chaleur solaire," dit-il, " a constamment été plus " grande près du commencement des cinq minutes que vers la fin ; or, cela n'a pas " lieu dans la transmission de la lumière, qui est sensiblement instantanée. Cela " indique que la loi de transmission n'est pas la même pour la lumière que pour la " chaleur." Ce sont là ses expressions abrégées, et réduites au seul objet que j'ai en vue.

minutes avant que cet accroissement de température vienne à cesser; et il ne paroît pas qu'on ait atteint le terme de son maximum. Il y auroit donc de l'importance à connoître la loi de cet accroissement; car il est facile de comprendre que, selon la nature de cette loi, l'accroissement partiel produit pendant un temps limité (tel que 5′) sera, ou ne sera pas, proportionnel à la chaleur de la source. Or, c'est cette chaleur qu'il s'agit d'estimer, d'une et d'autre part, pour pouvoir comparer la chaleur transmise par le verre avec la chaleur entière qui passe sans obstacle. Tournons donc notre attention vers un objet si évidemment requis.

§ 3. La loi dont nous avons besoin, a été reconnue et déterminée par des expériences directes. Il résulte de celles de M M. KRAFT et RICHMANN,* que *dans un milieu d'une température constante*, un corps s'échauffe ou se réfroidit de sorte que *les différences de sa chaleur à celle du milieu sont en progression géométrique, tandis que les temps* de l'échauffement ou du réfroidissement *sont en progression arithmétique.* Cette loi, déduite, je le répéte, d'expériences directes et faites avec soin, est parfaitement d'accord avec la théorie générale de la chaleur, qui se fonde sur d'autres faits, et dont je dirai un mot en finissant ce mémoire. En ce moment, je laisse cette loi isolée, et je l'admets simplement comme une vérité particulière, que l'expérience a démontrée.

§ 4. Il résulte de cette loi d'accroissement, que si deux corps de même température sont plongés dans deux milieux de température constante, mais inégale, les accroissemens opérés en temps égaux ne seront point, en général, proportionnels à la température de ces milieux, puisqu'il n'y a que quelques cas

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* *Nov. Comm. Acad. Petrop.* Tom. I. p. 195.

très-particuliers et très-rares où cette proportion puisse avoir lieu, comme il est facile de s'en assurer; par conséquent, les deux thermométres des expériences précédentes n'indiquent pas, par le rapport de leurs mouvemens en cinq minutes, le rapport de la chaleur totale à la partie de cette chaleur qui a été transmise par la lame interposée. Il faut s'y prendre d'une autre manière, pour faire cette estimation. La plus simple, peut-être, eût été de n'avoir aucun égard au temps, et de laisser chaque thermométre atteindre la température de la source de chaleur dans laquelle il est plongé; mais la durée de l'expérience entreprise sur ce principe offre peut-être des inconvéniens, et l'on verra d'ailleurs, par ce qui va suivre, que cette méthode même exigeroit encore une analyse ultérieure. Quoiqu'il en soit, on peut encore tirer des conséquences légitimes, des expériences qui ont été faites dans un temps limité. Je vais m'appliquer a tirer ces conséquences, du moins les principales; et l'auteur de cette belle suite d'observations verra, j'espère, avec plaisir, que les resultats qu'elles offrent sous cette nouvelle forme deviennent plus réguliers et plus probables.

§ 5. Je commence par discuter l'expérience 24me de l'ouvrage de M. HERSCHEL; c'est celle dont j'ai transcrit le détail ci-dessus. (§ 1.) Les temps croissant en proportion arithmétique, 0, 1, 2, 3, 4, 5, les différences de chaleur du thermométre et du milieu doivent décroitre en progression géométrique. Les dégrés observés au thermométre exposé à la chaleur libre du soleil sont, au commencement des trois premières minutes, 67, $68\frac{3}{4}$, $70\frac{1}{8}$, ou en huitièmes de dégré, 536, 550, 561. Maintenant, si l'on suppose que la température du rayon solaire ait été (en huitièmes de dégré) = 601, on trouvera que les différences de la chaleur du thermométre à celle du milieu, savoir, 65, 51, 40, sont en progression géométrique; ce qu'on n'obtiendra par aucun autre nombre. La loi prescrite nous force donc d'admettre ce nombre, pour l'expression de la chaleur du milieu où étoit placé le thermométre. Cela étant, nous calculerons les termes suivans de la progression, nous en conclurrons les dégrés du thermométre pour les minutes suivantes, et nous les comparerons aux dégrés observés. C'est l'objet de la petite table suivante, où tous les nombres expriment des huitièmes de dégré.

Chaleur du milieu, conclue des 3 premiers termes . . . . 601.

	Dégrés observés.	Dégrés calculés.	Différences en progr. géom.
0′	536	536	65
1	550	550	51
2	561	561	40
3	571	570	31
4	579	576	25
5	584	582	19

On peut observer, que les trois derniers dégrés calculés sont d'accord avec les dégrés observés, avec un écart de moins de trois huitièmes de dégré.

§ 6. Maintenant nous allons faire la même opération pour les observations collatérales, faites avec le thermométre que garantissoit un peu une lame de verre blanc bleuâtre. Mais il y a ici une remarque à faire: la progression des différences du premier thermométre et du milieu a pour quotient $\frac{65}{51}$; il paroît que celle du second thermométre doit avoir le même quotient, car il part du même point, son échelle d'échauffement est comprise en entier dans celle du premier thermométre, et ces deux thermométres ont été choisis avec une attention scrupuleuse, de manière à avoir précisément la même sensibilité; ainsi, par un même accroissement de chaleur, chacun d'eux, en même temps, se meut d'une même quantité. Si, par exemple, la température du milieu excéde, de part et d'autre, celle du thermométre de 65 huitièmes de dégré, on doit s'attendre que l'un et l'autre en une minute en acquerra 14, et ne différera plus de la source que de 51 huitièmes de dégré; mais, en chaque thermométre, cette proportion étant constante dans les échauffemens subséquens, (d'après la loi,) il est clair que le quotient est le même pour les deux thermométres, dans toute l'étendue de la progression.

Il n'en seroit pas ainsi, si les thermométres n'étoient pas également sensibles; par conséquent, en passant d'une expérience à l'autre, il conviendra de remarquer si les thermométres ont changé; et, en ce cas, de chercher de nouveau le quotient de la progression.

§ 7. Je viens à la partie de l'expérience qui nous reste à examiner; il s'agit du thermométre garanti de l'action du soleil, par une lame de verre blanc bleuâtre. Prenant donc les deux premiers nombres donnés par l'observation, savoir, ceux qui repondent au commencement et à la fin de la 1re minute de l'expérience, nous déterminerons celui qui a dû exprimer la chaleur du milieu, pour que les différences des deux premiers nombres à celui-ci soient entr'eux comme 65 est à 51; et, formant successivement les autres termes de cette progression, nous en conclurons les dégrés pour les quatres minutes suivantes, afin de les comparer aux dégrés observés. C'est l'objet de la petite table suivante, en huitièmes de dégré.

Chaleur du milieu 578.

	Dégrés observés.	Dégrés calculés.	Différences en progr. geom.
0'	536	536	42
1	545	545	33
2	553	552	26
3	560	558	20
4	567	562	16
5	572	565	13

Les nombres calculés et observés différent ici de 1 jusqu'à 7 huitièmes de dégré. Je dirai plus bas à quoi j'attribue cet écart. (§. 12.)

§. 8. Supposant maintenant que la chaleur de l'un et de l'autre milieu (celle du rayon libre et celle du courant qui agit sous le verre) ait été bien appréciée, il ne reste plus qu'à les comparer; leur rapport est celui de 601 à 578; et par conséquent la quantitè interceptée = 0,038.

§. 9. Passons à l'expérience suivante, qui est la 25me de l'ouvrage. Celle-ci a été faite avec les mêmes thermométres que la précédente. Le corps mis en expérience étoit une lame de *flint glass;* et en voici le résultat, tel que le donne l'auteur.

	Dégrés observés.	
	Au soleil libre.	A travers le *flint glass.*
0'	$69\frac{3}{4}$	$69\frac{3}{4}$
1	$71\frac{1}{4}$	71
2	$72\frac{5}{8}$	$72\frac{1}{8}$
3	$74\frac{1}{8}$	$73\frac{7}{8}$
4	$74\frac{7}{8}$	74
5	$75\frac{1}{4}$	$74\frac{3}{4}$ ..... $5\frac{1}{2} : 5 = 0{,}909$

En calculant cette expérience comme la précédente, et en prenant $\frac{65}{51}$ pour le quotient de la progression des différences, on aura, en huitièmes de dégré, les resultats comparés qu'indique la table suivante.

	Au soleil libre. *Chaleur du milieu* 614.			*A travers le* flint glass. *Chaleur du milieu* 604.		
	Dégrés observés.	Dégrés calculés.	Differ. en progr. geom.	Dégrés observés.	Dégrés calculés.	Differ. en progr. geom.
0′	558	558	56	558	558	46
1	570	570	44	568	568	36
2	581	579	35	577	576	28
3	593	587	27	591	582	22
4	599	593	21	592	587	17
5	602	597	17	598	590	14

Rapport des deux chaleurs 604 : 614.
Interception - - 0,015.

§. 10. En jettant les yeux sur cette table, on voit que les deux thermométres ont montré un échauffement plus rapide dans les dernières minutes que le calcul ne l'annonçoit. Le thermométre au soleil libre, présente un excès de 1 jusqu'à 6 huitièmes de dégré, qui diminue à la fin, et se reduit à 5 huitièmes. Le thermométre garanti par le *flint glass,* présente un excès qui varie irrégulièrement de 1 à 9 huitièmes. Cet écart, pour le thermométre garanti, est dans le même sens que celui de l'expérience précédente, et sera expliqué de même ci-dessous. (§. 12.) L'écart du thermométre exposé au soleil libre, ne peut s'expliquer qu'en supposant quelque cause particulière d'irregularité.

§. 11. Il y a une troisième expérience dont l'observateur donne le detail, et qu'il nous reste à examiner; c'est la 122me de l'ouvrage; elle a été faite avec une lame de talc, sous l'influence de la chaleur d'un feu de charbon bien ménagé. En voici les résultats donnés par l'auteur.

	Dégrés observés.	
	Au feu libre.	A travers le talc.
0'	65	65
1	72	67
2	77	$68\frac{3}{4}$
3	$80\frac{1}{2}$	$69\frac{1}{2}$
4	83	70
5	85	$70\frac{3}{4}$ . . . . . $20 : 5\frac{3}{4} = 0{,}287$.

Ici les thermométres ne sont plus les mêmes que ceux qui ont été employés dans les deux expériences que nous avons discutées; ceux-ci étoient désignés No. 5, et No. 1; ceux-là sont distingués par les lettres D, C; il faut donc chercher de nouveau, pour ces deux thermométres, également sensibles entr'eux, (mais peut-être différens en sensibilité des precedens,) selon quelle progression s'est fait l'échauffement au soleil libre, pendant le cours des deux premières minutes, (§ 6,) afin d'en conclure les dégrés suivans. Il résultera de ce calcul, et de l'emploi de la progression ainsi déterminée pour le thermométre garanti, la table suivante, toujours en huitièmes de dégré.

	Au feu libre. *Chaleur du feu 716.*			*A travers le talc.* *Chaleur du milieu 576.*		
	Dégrés observés.	Dégrés calculés.	Différ. en progr. geom.	Dégrés observés.	Dégrés calculés.	Différ. en progr. geom.
0′	520	520	196	520	520	56
1	576	576	140	536	536	40
2	616	616	100	550	547	29
3	644	645	71	556	555	21
4	664	665	51	560	561	15
5	680	680	36	566	566	10

La progression des différences a ici pour quotient $\frac{7}{5}$, au lieu de $\frac{65}{51}$: ainsi les thermométres recevoient, en temps égal, de la source calorifique, une aliquot de chaleur un peu moindre que les précédens ; cependant, la différence n'est pas très considérable ; du reste, on peut bien dire, que dans cette expérience le calcul et l'observation sont parfaitement d'accord. Ce n'est pas la peine de remarquer des différences aussi petites, et qui seroient encore plus insensibles, si j'avois tenu compte des fractions de dégré inférieures à une huitième, ce que je n'ai pas cru devoir faire. Cet accord est d'autant plus remarquable, que c'est précisément ici l'expérience qui a offert quelque chose de particulier, qui auroit dû, à ce qu'il semble, introduire de l'irrégularité dans les résultats. Le talc s'est calciné par l'action du feu, dans le cours de l'éxpérience, et de transparent qu'il étoit, il est devenu parfaitement opaque ; néanmoins, il paroit que l'action de la chaleur sous le talc, de minute en minute, a suivi un cours parfaitement régulier et uniforme. En voici le calcul.

Rapport des deux chaleurs 576 : 716.
Interception - - 0,196.

§ 12. Tels sont les résultats que nous offrent les trois expériences dont l'auteur a consigné le détail dans son ouvrage. Il est temps de dire un mot de la cause à laquelle j'attribue, dans les deux premières expériences, l'excès d'échauffement qui a été observé au thermométre garanti, dans les dernières minutes de leur durée. (§§ 7 et 10.) Je crois qu'il depend de la chaleur accumulée dans le corps interceptant. A l'instant où ce corps s'échauffe, il contribue à faire monter le thermométre voisin. Si la marche de cette accumulation de chaleur étoit très régulière, son effet se confondroit avec celui des rayons transmis; (c'est, je pense, ce qui a eu lieu dans la 3me expérience, où la progression des différences n'est guères moins exacte pour le thermométre garanti que pour l'autre;) mais, si l'accumulation est accélérée, (c'est-à-dire, si le rapport des rayons accumulés aux transmis est plus grand en même temps vers la fin de l'expérience qu'au commencement,) son effet croissant se fera sentir au thermométre, qui se mouvra comme il s'est mû dans les deux premières expériences. A quoi donc peut tenir une pareille accélération, et quelle raison peut on imaginer pour qu'elle ait lieu dans un cas, et non dans l'autre? On ne sauroit, je crois, l'imputer à aucune cause plus probable qu'à l'épaisseur de la lame, ou à la foiblesse de la source de chaleur.

§ 13. Supposons qu'on présente un verre épais à un foyer de chaleur; il s'échauffera du côté du feu, et, conduisant mal la chaleur, il restera quelque temps froid du côté opposé; ainsi, pendant la 1re minute, peut-être, un thermométre placé de ce dernièr côté n'accuseroit aucun échauffement; mais, peu-à-peu, dans les suivantes, cet échauffement se feroit sentir. Je présume que c'est ainsi que les choses se sont passées dans les deux premières expériences, et en particulier dans la seconde; (la 25me de l'ouvrage;) dans celle-ci, la lame de *flint glass* avoit environ trois lignes d'épaisseur. L'observateur donne cette mesure, tandis qu'il ne dit rien de l'épaisseur des autres lames. Il est probable que celles-ci étoient plus minces, en particulier celle de talc; et cela pourroit expliquer la régularité de l'une de ces expériences, et l'irregularité de l'autre.

Joignez à cela, que dans la troisième des expériences que j'ai analysées, (la 122me de l'ouvrage), la source de chaleur (le feu de charbon) avoit plus d'intensité, ou d'activité, que celles (les rayons solaires) qui agissoient dans les deux autres; puisque, dans le même espace de cinq minutes, elle a amené le thermométre libre de 65° à 85; tandis que le thermométre libre dans les deux autres expériences, n'a monté que de 5 ou 6 dégrés, compris entre ces extrêmes. Or, il est probable, que si deux lames sont de même nature et de mème épaisseur, mais que l'une soit exposée à une chaleur forte et l'autre à une chaleur foible, la première sera traversée plutôt que la seconde, par la chaleur accumulée; ensorte que, touchant, à la fin de la 1re minute, par exemple, la face non exposée de chacune des deux lames, il se pourra faire qu'on sente l'une froide et l'autre chaude.

Par deux raisons donc, l'expérience 122me a dû offrir des résultats réguliers; 1. parceque probablement la lame étoit mince; 2. parceque la source de chaleur étoit grande; d'où il résultoit, que la chaleur accumulée l'avoit traversée dès la fin de la 1re minute; ensorte que l'accumulation, et le rayonnement qui en est la suite, croissoient, de minute en minute, selon la même loi d'échauffement selon laquelle s'échauffoit d'ailleurs la boule du thermométre, si quelque chaleur étoit transmise sans obstacle.

Et si la 2de expérience (la 25me de l'ouvrage) offre plus d'irregularités que la première, (la 24me de l'ouvrage,) cela pourroit bien tenir en partie à la plus grande épaisseur du *flint* *glass.* Cependant, d'un côté nous ne pouvons rien affirmer sur l'épaisseur du verre blanc bleuâtre, qui n'est pas indiquée; et de l'autre, l'échauffement au soleil libre offre, dans cette même expérience, (la 25me,) des écarts qui vont jusqu'à $\frac{6}{8}$mes de dégré. Pourroit on les attribuer à quelque légére variation dans la source même de la chaleur, pendant le cours de l'expérience?

Je pense en avoir dit assez, pour rendre probable la cause à laquelle j'attribue cette espèce d'irrégularité apparente, qui consiste dans l'accélération de l'échauffement du thermométre garanti; cette cause doit avoir été, l'inégale action de la chaleur accumulée sur le corps interceptant, au commencement et à la fin de l'expérience.

§ 14. Il résulte de ces considérations, et de la distinction entre les deux chaleurs, transmise et accumulée, que l'interception calculée ci-dessus, dans chacune des trois expériences que nous avons rapportées, n'est, à proprement parler, qu'une limite en dessous, et laisse indéterminée la limite supérieure. Car, comme nous ne savons point le rapport des deux chaleurs, (transmise et accumulée,) nous ne pouvons point affirmer l'influence de chacune d'elles sur le résultat. Si la chaleur librement transmise agissoit seule, nous aurions une progression régulière de différences, (comme on l'a au soleil libre,) et les dégrés calculés s'accorderoient aussi bien avec ceux qu'a donnés l'observation. Mais il y a excès dans les derniers termes; et cet excès doit provenir de la chaleur accumulée; celle-ci a donc agi, et manifesté son influence. D'un autre côté, la transmission libre peut avoir été fort petite; on pourroit même la supposer nulle, et attribuer à la chaleur accumulée, tout l'effet observé sur le thermométre garanti. Ainsi l'on peut bien dire, que la transmission réelle n'a pas été plus grande que la calculée, puisque le calcul suppose tout l'effet produit par cette chaleur; mais elle peut très bien avoir été moindre, puisque cet effet a certainement été produit, en partie au moins, et peut-être en totalité, par une autre cause. L'interception peut donc avoir été totale, ou très grande, mais jamais moindre que celle que le calcul nous a donnée. C'est en ce sens qu'il faut prendre tous nos résultats obtenus jusqu'ici, et tous ceux que nous allons rechercher encore.

§ 15. Il y auroit maintenant quelque intérêt à examiner, d'après les calculs précédens, combien auroit dû durer chaque expérience, pour que le thermométre atteignît le maximum d'échauffement, c'est-à-dire, la température de la source, ou du milieu dans lequel il étoit plongé; car c'est à cette époque qu'on auroit pu comparer immédiatement les dégrés des deux thermométres, exposés, l'un à la chaleur libre, et l'autre à la chaleur génée par l'interception. Cependant, une difficulté se présente. Il est facile de continuer les termes de la progression au soleil libre, et d'en conclure les dégrés qu'on auroit observés dans les minutes suivantes; mais, pour le thermométre garanti, comment tenir compte de l'effet inégal de la chaleur accumulée dans la lame interceptante? Arrivée à un certain point, cette chaleur accumulée, n'en développera-t-elle point même de nouvelle, comme il semble que cela a lieu dans les boules d'argile échauffées au feu d'un foyer? Quoiqu'il en soit, comme ceci n'intéresse point l'échauffement au soleil libre, nous pouvons du moins examiner ce cas. J'y joindrai le calcul de l'échauffement sous le talc, à cause de sa régularité, qui semble indiquer que, dans les termes suivans, la progression auroit été constante.

Comme l'observateur tient compte des huitièmes de dégré, et non d'aucune fraction moindre, l'échauffement paroîtra fini plutôt qu'il ne le sera réellement. Ainsi, vers la fin, on ne remarquera plus de différence sensible pendant une minute; mais, en attendant deux ou trois minutes, cet accroissement se fera remarquer. Je trouve que dans la 1re expérience, (la 24me de l'ouvrage,) au soleil libre, le thermométre auroit continué jusqu'à la 12me minute, d'accuser, de minute en minute, un accroissement de chaleur sensible: il auroit alors marqué 598 huitièmes de dégré. Il se seroit passé encore quelques minutes, avant que le thermométre eût acquis sensiblement (c'est-à-dire, à un huitième près) la chaleur totale de la source, qui, selon notre calcul, (§ 5,) étoit de 601 huitièmes de dégré.

Je laisse l'expérience faite avec le *flint glass*, (la 25me de l'ouvrage,) à cause de son irrégularité.

Celle où le talc a été employé (la 122me de l'ouvrage) nous fait voir, qu'au soleil libre il auroit aussi fallu 12′ pour amener le thermométre assez près de la température du milieu, pour que l'échauffement en une minute fût devenu insensible; (c'est-à-dire, moindre qu'un huitième de dégré;) à cette époque, il n'auroit différé que d'environ $\frac{3}{8}$mes de la température du milieu, qu'il auroit assez vîte atteint.

Dans cette même expérience, le thermométre couvert de la lame de talc n'auroit requis que 9′, pour arriver au terme auquel une minute de plus ne produit aucun effet sensible; à cette époque, la chaleur du thermométre auroit différé de celle du milieu d'un peu moins de $\frac{3}{8}$mes de dégré; et 3 minutes après, c'est-à-dire, à la 12me minute de l'expérience, ces deux chaleurs n'auroient pas différé sensiblement; je veux dire, qu'elles auroient différé d'une quantité moindre qu'un huitième de dégré, qui est la fraction la plus petite dont l'observateur ait tenu compte.

§ 16. Jusqu'ici je n'ai discuté que trois expériences, entre toutes celles du même genre, parceque ce sont les seules dont l'auteur donne le détail. Pour toutes les autres, il se contente de rapporter le dégré initial et le dégré final de chaque thermométre, parcequ'en effet ce sont les seuls qu'il emploie, pour en conclure, par sa méthode, la quantité des rayons transmis et interceptés. Il sera facile à l'auteur de vérifier ces remarques, par l'examen de ses régistres plus détaillés. Pour suppléer à cette recherche, qui n'est pas en mon pouvoir, j'ai essayé d'employer, d'une manière conforme aux principes exposés ci-dessus, quelques-uns des résultats abrégés, qui s'offrent à nous en grand nombre.

§ 17. On peut remarquer que le rapport de 13 à 10, est moyen entre ceux qui ont été employés comme quotients de la progression des différences, et que l'observation a déterminés. (§§ 5 et 11.) Je me tiendrai donc à ce rapport; et je déterminerai la chaleur constante du milieu par la proportion suivante. Les différences entre cette chaleur et chacun des nombres donnés par l'observation, (l'initial et le final,) sont entr'elles comme le 1er terme de la progression est au 6me, c'est-à-dire, comme les nombres 13 et 10 élévés à la cinquième puissance.

§ 18. Ainsi, prenant la 26me expérience de l'ouvrage, on l'y trouvera ainsi abrégée:

	Au soleil libre.	A travers du *crown glass* verdâtre.
0′	$66\frac{1}{4}$	$66\frac{1}{4}$
5	73	$71\frac{1}{4}$ . . . . . $6\frac{3}{4} : 5 = 0{,}741$

J'en conclus, (en partant du rapport de 13 à 10 pour la progression des différences,) que la chaleur constante du soleil libre étoit, en huitièmes de dégré, 604; et à travers le verre 584.

Rapport de ces chaleurs	0,967
Interception	0,033.

27*me*.

	Soleil.	*Coach glass* (verre de carrosse.)
o′	$68\frac{7}{8}$	$68\frac{7}{8}$
5	$75\frac{7}{8}$	$74\frac{3}{8}$ ...... $7 : 5\frac{1}{2} = 0{,}786$

Chaleur au soleil libre 267
Sous le verre - - 611
Rapport 0,974
Interception 0,026

28*me*.

	Soleil.	Cristal d'Islande.
o′	67	67
5	$72\frac{5}{8}$	$71\frac{1}{4}$ ..... $5\frac{5}{8} : 4\frac{1}{4} = 0{,}756$

Chaleur au soleil libre 598
Sous le cristal d'Islande 583
Rapport 0,975
Interception 0,025.

29*me*.

	Soleil.	Talc.
o′	$67\frac{1}{2}$	$67\frac{1}{2}$
5	72	$71\frac{3}{8}$ ..... $4\frac{1}{2} : 3\frac{7}{8} = 0{,}861$

Chaleur au soleil libre 590
Sous le talc - - 584
Rapport 0,990
Interception 0,010.

30*me*.

	Soleil.	Talc aisément calcinable.
o′	50	50
5	$54\frac{3}{4}$	$53\frac{7}{8}$ ..... $4\frac{3}{4} : 3\frac{7}{8} = 0{,}816$

Chaleur au soleil libre 453
Sous le talc calcinable 443
Rapport 0,978
Interception 0,022.

31me.

	Soleil.	Verre rouge très obscur.
o′	73	73
5	$79\frac{1}{4}$	$74\frac{1}{4}$ . . . . . $6\frac{1}{4} : 1\frac{1}{4} = 0{,}200$

Chaleur au soleil libre - 654
Sous le verre rouge obscur 598
Rapport 0,914
Interception 0,086.

40me.

	Soleil.	Verre indigo.
o′	$61\frac{3}{4}$	$61\frac{3}{4}$
5	$67\frac{7}{8}$	64 . . . . . $6\frac{1}{8} : 2\frac{1}{4} = 0{,}367$

Chaleur au soleil libre 562
Sous le verre indigo 519
Rapport 0,923
Interception 0,077.

§ 19. Je vais encore rapporter quelques expériences, et en tirer les résultats, comme ci-dessus. Mais je dois remarquer, que dans les suivantes, il arrive souvent que les thermométres ne sont pas d'accord au point de départ. J'ignore d'où cela peut dépendre.

44*me Expérience.*

	Soleil.	Les deux fonds de verre d'un tube fermé, long de 3 pouces.
o′	53	53
5	59	$55\frac{3}{4}$ . . . . . $6 : 2\frac{3}{4} = 0{,}458$

Chaleur au soleil libre - - 490
Sous les deux fonds de verre 454
Rapport 0,927
Interception 0,073.

45*me*.

	Soleil.	Eau, et les deux fonds de verre du même tube fermé qui la contient.	
0′	$52\frac{1}{4}$	$52\frac{1}{8}$	
5	$58\frac{3}{4}$	55	..... $6\frac{1}{2} : 2\frac{7}{8} = 0,442$

Chaleur au soleil libre - - - 490

Sous l'eau et les deux fonds de verre 449

Rapport 0,917

Interception 0,083.

47*me*.

	Soleil.	Esprit de vin, et les deux fonds de verre du même tube qui le contient.	
0′	$51\frac{5}{8}$	$51\frac{5}{8}$	
5	$57\frac{3}{4}$	54	..... $6\frac{1}{8} : 2\frac{3}{8} = 0,388$

Chaleur au soleil libre - - - - 494

Sous l'esprit de vin et les deux fonds de verre 439

Rapport 0,889

Interception 0,111.

48*me*.

	Soleil.	*Gin*, (liqueur spiritueuse,) et les deux fonds de verre.	
0′	52	52	
5	$57\frac{3}{4}$	$53\frac{1}{2}$	..... $5\frac{3}{4} : 1\frac{1}{2} = 0,261$

Chaleur au soleil libre - - - 480

Sous le *gin* et les deux fonds de verre 434

Rapport 0,904

Interception 0,096.

50*me*.

	Soleil.	*Crown glass* usé à l'émeri du côté exposé.	
0′	67	67	
5	74	$70\frac{3}{4}$	..... $7 : 3\frac{3}{4} = 0,536$

Chaleur au soleil libre 614

Sous le verre - - 578

Rapport 0,941

Interception 0,059.

51*me.*

	Soleil.	*Coach glass* (glace de carrosse) usé à l'émeri du côté opposé.	
o′	66½	66½	
5	73½	69½	..... 7 : 3 = 0,429

Chaleur au soleil libre 660
Sous le verre - 568
Rapport 0,861
Interception 0,139.

148*me.*

	Aux rayons invisibles du soleil libre.	A ces mêmes rayons à travers un verre blanc bleuâtre.	
o′	48	47	
5	49¾	48⅝	..... 1¾ : 1⅝ = 0,929

Chaleur aux rayons libres 404
Sous le verre - - 394
Rapport 0,975
Interception 0,025.

149*me.*

	Rayons invisibles.	*Flint glass.*	
o′	50¾	49⅞	
5	52	51⅛	..... 1¼ : 1¼ = 1,000

Chaleur aux rayons libres 420
Sous le verre - - 413
Rapport 0,983
Interception 0,017.

150*me.*

	Rayons invisibles.	*Crown glass.*	
o′	50½	49¾	
5	51⅞	50⅞	..... 1⅜ : 1⅛ = 0,818

Chaleur aux rayons libres 419
Sous le verre - - - 410
Rapport 0,978
Interception 0,022.

151*me.*

	Rayons invisibles.	*Coach glass* (verre de carrosse.)
0′	$54\frac{1}{2}$	$53\frac{7}{8}$
5	$55\frac{3}{8}$	$54\frac{5}{8}$ ..... $\frac{7}{8} : \frac{3}{4} = 0{,}857$

Chaleur aux rayons libres 446
Sous le verre - - - 439
Rapport 0,984
Interception 0,016.

152*me.*

	Rayons invisibles.	Talc calcinable.
0′	$51\frac{3}{8}$	$50\frac{3}{4}$
5	$52\frac{7}{8}$	$51\frac{7}{8}$ ..... $1\frac{1}{2} : 1\frac{1}{8} = 0{,}750$

Chaleur aux rayons libres 428
Sous le talc calcinable 419
Rapport 0,979
Interception 0,021.

§ 20. Cette comparaison, entre mes résultats et ceux que l'auteur a déduit des mêmes expériences, donne lieu à quelques remarques.

Première Remarque. Nous pouvons nous faire quelque idée de l'inexactitude de mes résultats, fondés sur les deux nombres extrêmes, en calculant ainsi les trois expériences que nous avons déjà calculés sur des données plus détaillées. Quant à la 122me, (§ 11,) comme la progression est très régulière, nous sommes assurés que les deux méthodes co-incident, et toute comparaison est inutile. Dans les deux autres, au contraire, nous sommes assurés d'avance, qu'elles ne co-incident pas; et c'est cet écart qui nous intéresse.

24me *Expérience.*

	Soleil.	Verre blanc bleuâtre.
0′	67	67
5	73	71½ . . . . . 6 : 4½ = 0,750

Chaleur au soleil libre 602
Sous le verre - - 588
Rapport 0,974
Interception 0,026.

Mon résultat précédent (§ 8) donnoit précisément, ou à un huitième près, la même chaleur au soleil libre. Sous le verre elle donnoit seulement 578 ; ce qui est bien naturel, puisque l'échauffement sous le verre a excédé la progression dans les dernièrs temps ; en conséquence, les rayons interceptés étoient exprimés par 0,038.

Le rapport des interceptions, déterminées par ces deux méthodes, est celui de 13 à 19, qui est très voisin de celui de 2 à 3. Ici donc, pour trouver l'interception résultant du calcul fondé sur toutes les données de l'expérience, il falloit augmenter l'interception déterminée par les deux nombres extrêmes, dans le rapport de 2 à 3.

25me *Expérience.*

	Soleil.	*Flint glass.*
0′	69¾	69¾
5	75¼	74¾ . . . . . 5½ : 5 = 0,909

Chaleur au soleil libre 619
Sous le verre - 613
Rapport 0,990
Interception 0,010.

Mon résultat précédent (§ 9) donnoit 614, au lieu de 619, pour la chaleur au soleil libre ; et 604, au lieu de 613, sous le verre ; et l'interception étoit 0,015, au lieu de 0,010. Ici donc encore, il auroit convenu d'augmenter l'interception, déterminée par deux nombres seulement, selon le rapport de 2 à 3, afin d'avoir l'interception résultant de toutes les données.

On doit présumer, qu'il en est de même de la plupart des autres expériences dont nous n'avons pas le détail. En appliquant cette correction à toutes celles qui sont dans ce cas, dont j'ai fait ci-dessus le calcul, (§§ 18 et 19,) il en résulteroit la table suivante, dans laquelle mes résultats sont rapprochés de ceux de l'observateur, tant pour la chaleur que pour la lumière ; et où l'on remarquera, que l'interception de la chaleur, calculée selon ma méthode, (d'après la loi du § 3,) est constamment moindre que l'interception de la lumière, dont elle est une fraction qui varie entre un et sept dixièmes.

Interception de la chaleur par différentes matières. *Sur* 1000 *rayons.*

Numéros des expériences.	*Au soleil.*	Interception selon la loi du § 3.	Interception selon l'observateur.	
			Chaleur.	Lumière.
24.	Verre blanc bleuâtre -	38	250	86
25.	*Flint glass* - -	15	91	34
26.	*Crown glass* verdâtre -	49	259	203
27.	*Coach glass* (glace de carrosse) - - - -	39	214	168
28.	Cristal d'Islande - -	38	244	150
29.	Talc - - -	15	139	90
30.	Talc aisément calcinable	33	184	288
31.	Verre rouge très obscur -	129	800	999,9
40.	Verre indigo - -	115	633	999,7
44.	Les deux fonds de verre d'un tube fermé, long de 3 pouces	109	542	204
45.	Les deux fonds de verre, et l'eau que le tube contient	124	558	211
47.	Les deux fonds de verre, et l'esprit de vin contenu	166	612	224
48.	Les deux fonds, et le *gin* contenu - -	144	739	626
50.	*Crown glass* usé à l'émeri du côté exposé - -	88	464	854
51.	*Coach glass* (glace de carrosse) usé à l'émeri du côté exposé - -	208	571	885
	Aux rayons invisibles.			
148.	Verre blanc bleuâtre -	38	71	—
149.	*Flint glass* - - -	25	—	—
150.	*Crown glass* - - -	33	182	—
151.	*Coach glass* (glace de carrosse) - - -	24	143	—
152.	Talc calcinable - -	31	250	—
	Au feu de charbon.			
122.	Talc calciné pendant l'expérience - -	196	713	288

Mais, outre qu'il y a probablement des cas auxquels la correction aura été appliquée mal-à-propos, (cas qu'il m'est impossible de déterminer,) je crois, qu'avant de prononcer d'une manière générale sur la qualité de chaque corps, il conviendroit de les réduire tous en lames d'une égale épaisseur, et de les exposer à des chaleurs égales, par les raisons que j'ai exposé ci-dessus. (§ 13.) Mais il est probable, que ces causes d'erreur ne masquent pas entièrement la vérité.

§ 21. 2*de Remarque.* La faculté interceptante de cinq substances, rélativement aux rayons invisibles, conclue des expériences 148me et suivantes, par les deux observations extrêmes, est, selon mon résultat, (fondé sur la loi du § 3,) fort rapprochée de celle de ces mêmes substances, rélativement à tout le rayon solaire. (*Exp.* 24, et suivantes.)

Selon le résultat de l'observateur, la différence est plus considérable; elle est même infinie par rapport au *flint glass*, puisque, selon cette manière d'apprécier la transmission, les rayons invisibles ont tous traversé le *flint glass*, et n'ont point été interceptés. Ce résultat, qui paroît invraisemblable, surtout lorsqu'on a sous les yeux la suite de ces expériences, suffit seul pour ébranler la confiance en la méthode par laquelle il a été déduit.

On éprouveroit encore plus de défiance, si cette méthode venoit à présenter quelques cas, où la quantité des rayons transmis parût plus grande que celle des rayons libres. Or, ce cas peut très bien se présenter; puisqu'il suffit pour cela, qu'au 1er instant, la différence de la température du thermométre placé sous le corps interceptant, à celle du milieu où il est plongé, soit moindre que la différence de la température du thermométre exposé aux rayons libres, à celle de ces mêmes rayons. Si ce cas ne s'est pas présenté ici, c'est, sans doute, parceque l'observateur avoit à dessein pris soin de mettre ces deux thermométres au même dégré initial. Cependant, cela n'a pas toujours eu lieu ; et, en conséquence, il est arrivé une fois, que les deux thermométres ont varié également pendant la durée de l'expérience. S'il tentoit de nouvelles expériences, en ayant soin de tenir, au premier instant, la température du thermométre garanti beaucoup plus basse que celle du thermométre exposé aux rayons libres, on peut prévoir qu'il arriveroit souvent, en suivant sa méthode de calcul, que la transmission paroîtroit avoir accru le nombre des rayons.

§ 22. *3me Remarque*. En jettant les yeux sur mes résultats, comparés à ceux de M. HERSCHEL, on verra que ceux-ci donnent tous des interceptions beaucoup plus fortes. Une expérience de M. PICTET* donne une interception encore plus forte, et qui surpasse toutes celles qu'indiquent les tables de M. HERSCHEL, du moins pour les verres polis et sans couleur. Un thermométre, exposé à une source de chaleur, monta de 10°; garanti par un carreau de verre, ce thermométre baissa de 6°. Il paroît donc, que ce verre interceptoit les $\frac{3}{5}$ de la chaleur, ou 600 millièmes.

Ici l'observateur n'a point voulu limiter le temps, et paroît avoir eu dessein de laisser son thermométre atteindre la température de la source, soit libre, soit gênée; ensorte qu'on ne peut se refuser à cette conséquence, que le verre a dérobé au thermométre plus de la moitié de la chaleur, à l'influence de laquelle on l'avoit exposé.

Ce résultat s'éloignera moins de ceux qu'on peut déduire des observations de M. HERSCHEL, si l'on a égard aux considérations suivantes. 1. Quelle que soit la faculté interceptante d'une

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* Essai sur le Feu, § 52.

lame, l'interception doit croître, si l'on augmente son épaisseur. Si donc le carreau de M. Pictet étoit plus épais que les lames employées par M. Herschel, la transmission devoit être moindre. Cette circonstance de l'expérience est inconnue de part et d'autre; je n'en fais mention que comme d'une simple possibilité. La suivante est moins indéterminée. 2. Dans l'expérience de M. Pictet, le verre interposé étoit probablement froid, par comparaison au thermométre; la présence de ce corps froid, (quoiqu'à la distance de 5 pieds 7 pouces,) doit avoir eu quelque influence. 3. De plus, ce carreau interceptoit un courant d'air favorable à l'échauffement du thermométre. 4. Enfin, la source de chaleur, absorbée en partie par le verre, n'auroit pas manqué de l'échauffer à la fin sensiblement, et cet échauffement se seroit fait sentir au thermométre. Mais l'expérience finit probablement à cette époque; car l'observateur dut naturellement étre satisfait, quand il eut obtenu le maximum de refroidissement, qui étoit l'objet unique de son attention. D'ailleurs, l'appareil de M. Pictet est tel, que l'action directe du verre échauffé ne peut se faire sentir, que lorsqu'elle est déjà assez grande.

Au contraire, dans les expériences de M. Herschel, on voit des thermométres placés à environ 2 pouces de la lame interceptante, et participant au moindre échauffement de cette lame. Il n'y a d'ailleurs aucune cause de réfroidissement; et les lames sont probablement très-minces.

Telles sont les causes auxquelles j'attribue les différences observées dans les résultats déduits des expériences de ces deux habiles physiciens; et ces considérations nous ramènent a dire, que ces résultats, de quelque facon qu'on les calcule, varieront tant qu'on ne prendra pas des lames de même épaisseur. Ils varieroient encore probablement, si l'on faisoit varier la distance de la lame au thermométre, puisqu'on feroit varier par cela même, l'influence de la chaleur qui s'accumule dans la lame.

Du reste, la petitesse de mes résultats (fondés sur la loi du § 3) n'a rien qui puisse surprendre, puisque nous avons reconnu dès l'entrée, que nos calculs ne pouvoient nous donner qu'une limite de petitesse. (§ 14.). Il est donc très vraisemblable, que lors qu'on sera parvenu à mesurer à-part la chaleur transmise, on trouvera qu'elle est bien moindre, et l'interception bien plus grande, que nos résultats ne la présentent.

§ 23. *4me Remarque.* Et par quel moyen pourra-t-on parvenir à faire cette appréciation, à décomposer l'effet en ses deux élémens? Il me semble que ce doit être, en observant l'effet instantanée de la chaleur à travers un obstacle, et non son effet au bout d'un temps fini. Il faudra donc recourir à des thermométres très sensibles, tels que ceux d'air, employés et décrits par M. PICTET.* En voyant comment ils se comportent sous le verre, à l'instant même où celui-ci reçoit l'impression calorifique, on jugera d'abord de l'influence de la chaleur transmise, car on sait bien qu'il faut un certain temps pour que l'accumulée ait son effet; mais ce temps n'est pas suffisamment déterminé, et le phénomène varie probablement à différentes époques.

§ 24. *5me Remarque.* Tout ce que je viens de dire s'accorde fort bien avec un phénomène que M. PICTET a observé, et avec l'explication qu'il en donne. Un grand miroir concave de verre étamé, ne renvoyoit presque aucune chaleur à son foyer, sous l'influence des mêmes rayons qui, dans la même situation, élevoient le thermométre de plus de 10°, au foyer d'un miroir métallique. "Dans les miroirs de verre," dit M. PICTET,† "ce n'est point la surface antérieure qui réfléchit la plus grande

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* Essai sur le Feu, § 56.

† Ibid. § 67.

" partie des rayons, c'est surtout la surface métallique appliquée " derrière le verre. La chaleur, pour arriver à cette surface, a " toute l'épaisseur du verre à traverser; elle ne peut se réfléchir " sans la traverser de nouveau, et, étant ainsi doublement *tamisée* " par une substance qui ne lui laisse qu'un passage bien difficile, " il n'en échappe que peu pour agir sur le thermométre...... " mais, que devient cette chaleur ainsi interceptée par le verre? "...... Elle reste...... dans le verre, et s'emploie à le ré- " chauffer; elle se répand dans sa substance, à raison de la " chaleur spécifique du verre, et on s'appercevroit sans doute de " son effet, si le miroir restoit longtemps exposé a l'action du " foyer calorifique." Remarquons seulement, que cette action, n'étant point concentrée au foyer, seroit peu sensible.

§ 25. *6me Remarque.* En conséquence de toutes nos distinctions et explications précédentes, je me demande, quels sont les phénomènes successifs que doit offrir un thermométre placé derrière une lame interceptante? 1. Au premier instant, la chaleur transmise doit agir; mais probablement elle n'est qu'une foible aliquote de la source de chaleur qui atteint la lame. 2. Bientôt la chaleur absorbée par la lame s'y accumule assez pour rayonner, et envoyer au thermométre des emanations calorifiques. Cette influence suit le progrès de l'échauffement de la lame. 3. Enfin la lame s'échauffe au maximum qu'elle peut atteindre; alors le thermométre se trouve dans un courant de chaleur constante, et se fixe.

§ 26. *7me Remarque.* De quelle quantité, la chaleur sous cette lame différera-t-elle finalement de la chaleur libre?

Si la lame étoit plongée toute entière dans la source de chaleur, de sorte que celle-ci l'enveloppât de toutes parts, comme un bain, on sait que la lame acquerroit enfin la température de la source; mais, n'étant en contact avec elle que par une de ses faces, elle doit s'échauffer moins que si toutes deux lui fournissoient du feu; ensorte que, par cette raison, elle ne peut atteindre le dégré de chaleur de la source. Il y a un moment où la lame a acquis son maximum de chaleur; c'est celui où elle perd autant par ses deux surfaces, qu'elle acquiert par une seule; et ce maximum est nécessairement moindre que si elle acqueroit par toutes deux, par conséquent, moindre que la température de la source.

De plus, la chaleur réfléchie n'échauffe pas le corps qui la réfléchit; il faut donc déduire de la source de chaleur, tous les rayons réfléchis, lorsqu'il s'agit d'estimer l'échauffement de la lame interceptante.

Le thermométre placé sous le verre reçoit donc, 1. les rayons transmis instantanément; 2. les émanations de la chaleur accumulée dans le verre; mais il ne reçoit pas les rayons réfléchis; et la chaleur du verre a un maximum peu élevé.

§ 27. 8*me Remarque.* Ceci étant suffisamment éclairci, on concevra en quels cas le calcul des expériences de M. HERSCHEL, par les deux extrêmes, donnera, ou ne donnera pas, des resultats qui s'écartent de ceux qu'on auroit déduit de toutes les observations successives. Dans presque tous les cas de ce genre, il doit y avoir, en vertu de la chaleur accumulée dans la lame, un échauffement final plus grand que ne le comporte la loi. En conséquence, si l'on ne prend que les extrêmes de chaleur, (le dégré initial et le dégré final,) et qu'on suppose l'accroissement de chaleur régulier, (c'est-à-dire conforme à la loi,) on sera conduit nécessairement à trouver l'interception moindre que si on l'eût calculée par les dégrés observés aux premières minutes. C'est ce que nous avons vérifié sur les expériences 24me et

25me. Nous avons reconnu que, dans ces expériences, cette différence alloit à-peu-près à la moitié de l'interception estimée par les deux extrêmes ; ensorte que ces deux résultats étoient entr'eux comme les nombres 2 et 3. (§ 20.)

Comparons maintenant, sous ce point de vue, deux sources de chaleur inégales. Nous supposerons deux expériences, où chacune de ces sources agit, d'un côté librement, de l'autre à travers la même lame interceptante. Si l'accroissement de chaleur sous le verre étoit proportionnel à celui qui a lieu sous l'influence de la source libre, il est facile de voir que le calcul de l'interception la feroit paroître plus grande à la source la plus chaude. En voici un exemple, fictif, mais propre à rendre la chose sensible.

	No. I.		No. II.	
	Soleil.	Verre.	Soleil.	Verre.
0'	600	600	600	600
5	640	620	680	640
Chaleur au soleil libre	655		710	
Sous le verre - -	628		655	
Interception	0,042		0,077	

Il est vrai que les deux accroissemens, que j'ai supposés proportionnels, ne le sont pas; mais, comme ils augmentent et diminuent ensemble, et par la même cause, on peut bien affirmer, que la même lame fera paroître, au calcul, l'interception plus grande sous l'influence d'une source plus chaude, et réciproquement.

C'est aussi ce qu'on peut remarquer dans les expériences de M. Herschel, où, à travers les mêmes lames, on voit une chaleur de feu de charbon, d'environ 730, produire une interception d'environ 200; tandis que, dans les expériences au soleil, une chaleur d'environ 600, n'a produit qu'une interception d'environ 30.

§ 28. *9me Remarque.* C'est par la même cause, qu'à travers 4 verres, au feu de charbon, l'interception a paru moindre qu'à travers un seul; car, dans l'expérience des quatre verres, la chaleur du feu n'étoit que 655, au lieu que dans celles où il n'y avoit qu'un verre, elle étoit 757, 731, 782, 741. Dans celle où il y avoit deux verres, la chaleur étoit 700, moyenne entre celles que je viens de comparer, et l'interception a aussi été moyenne. C'est ce qui résulte du calcul suivant, où j'expose les expériences et leur résultats, en huitièmes de dégré, déduits selon la méthode expliquée ci-dessus, (§ 17,) en supposant, de minute en minute, la progression des différences dans le rapport de 7 à 5, parceque ce rapport est celui que nous a indiqué l'expérience 122, dont nous avons les détails, (§ 11,) et qui a été faite dans les mêmes circonstances.

Expériences faites au feu de charbon.

117*me.*

	Feu.	Verre blanc bleuâtre.
o'	528	528
5	688	568

Chaleur du feu libre 724
Sous le verre 577
Interception 0,203.

118*me.*

	Feu.	*Flint glass.*
o'	536	536
5	696	576

Chaleur du feu libre 732
Sous le verre - 585
Interception 0,201.

119*me*.

	Feu.	*Crown glass.*
0′	536	536
5	694	580

Chaleur du feu libre 729
Sous le verre - 590
Interception 0,191.

136*me*.

	Feu.	*Crown glass* usé à l'émeri du côté exposé seulement.
0′	541	541
5	718	590

Chaleur du feu libre 757
Sous le verre - 601
Interception 0,206.

137*me*.

	Feu.	*Coach glass* (glace de carrosse) usé à l'émeri du côté exposé seulement.
0′	544	540
5	697	577

Chaleur du feu libre 731
Sous le verre - 585
Interception 0,200

138*me.*

	Feu.	*Crown glass* usé à l'émeri des deux côtés.
0'	548	544
5	739	584

Chaleur du feu libre 782

Sous le verre - 593

Interception 0,244.

139*me.*

	Feu.	*Coach glass* usé à l'émeri des deux côtés.
0'	536	536
5	704	564

Chaleur du feu libre 741

Sous le verre - 570

Interception 0,231.

140*me.*

	Feu.	Les deux verres de *crown* et *coach glass* usés à l'émeri d'un côté seulement.
0'	528	528
5	688	559

Chaleur du feu libre 724

Sous les deux verres 566

Interception 0,232.

141*me.*

	Feu.	Les deux mêmes verres, usés à l'émeri des deux côtés.
0'	534	534
5	670	548

Chaleur du feu libre 700

Sous les deux verres 551

Interception 0,213.

142*me.*

	Feu.	Les quatre verres des deux expériences précédentes.
0′	528	528
5	640	539

Chaleur du feu libre - 665
Sous les quatre verres - 541
Interception 0,186.

Je viens à l'exposition de cette partie de la théorie de la chaleur, dont j'ai dit que dépendoit la loi de l'échauffement, que l'observation directe a fait reconnoitre. (§ 3.)

PARTIE II.

§ 29. Plusieurs raisons m'engagent à suivre, dans l'exposé de la théorie que j'ai en vue, un ordre relatif à l'histoire de sa découverte. Il résultera de là, que je paroîtrai d'abord m'écarter un peu de mon sujet; mais j'y rentrerai très vîte, ou plutôt je n'en sortirai point.

BACON proposoit cette expérience: « Les chaleurs brillantes « et radieuses sont exaltées par les verres: les chaleurs obscures « et opaques, (comme celles des pierres et des métaux, avant « d'être rougis par la force du feu,) sont elles sujettes à la même « impression?"[*]

Plusieurs physiciens postérieurs avoient observé qu'un charbon ardent, placé entre deux miroirs concaves, allumoit un corps combustible à plus de 20 pieds de distance. LAMBERT attribuoit cet effet à la chaleur obscure, et non a la chaleur lumineuse. Il étoit conduit à penser ainsi, parce qu'un feu très ardent ne lui paroissoit donner aucune chaleur au foyer d'une lentille convexe.[†]

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[*] *Instaurat.* l. 5, c. 2.

[†] Pyrométrie, § 378 et suiv. cité par M. DE SAUSSURE, Voyage aux Alpes, § 926.

M. DE SAUSSURE résolut de vérifier cette idée de LAMBERT, en substituant au charbon un boulet chaud, sans être rouge. Il s'adressa à M. PICTET, pour faire cette expérience, qui réussit parfaitement. Le boulet, occupant le foyer d'un des miroirs, fit monter de 10½ dégrés le thermométre placé à l'autre foyer. Un matras d'eau bouillante, substitué par M. PICTET au boulet chaud, produisit le même effet, quoiqu'avec moins d'intensité.*

Ces expériences prouvérent incontestablement, que la chaleur étoit susceptible d'être réfléchie, sous la même loi que la lumière. M. PICTET a prouvé de plus, que la vîtesse de la chaleur est si grande en ce cas, qu'elle parcourt 69 pieds, dans un instant sensiblement indivisible.†

§ 30. Ces faits, quelque curieux et importans qu'ils soient, ne forcent peut-être pas le physicien à se décider sur la nature de l'agent qui produit la chaleur, et en particulier sur le moyen par lequel s'établit et se maintient l'équilibre de température entre deux corps, ou entre deux espaces voisins. On se contentoit donc d'exprimer par le mot de *tension*, ou par quelque autre équivalent, l'espèce d'effort par lequel il s'opéroit. Ainsi, lorsque deux espaces sont inégalement chauds, la tension supérieure du plus chaud, l'emportant sur celle de l'autre, amène enfin un état dans lequel les deux tensions sont égales, et se balancent. Et, quoique ce langage n'offrît à l'esprit qu'une conception indéterminée, on se crut obligé de s'en contenter, et de la recevoir comme une loi de la nature. Cette loi étoit d'ailleurs semblable à celle qu'on observe dans les fluides élastiques plus grossiers, par une suite de la compression qu'ils éprouvent. On ne savoit pas si le feu étoit précisément de même nature; mais cette com-

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* Voyage aux Alpes, § 926.

† Essai sur le Feu, § 64.

paraison servoit à satisfaire l'esprit, et paroissoit en quelque sorte éclairer le phénomène.

§ 31. Une expérience nouvelle vint tirer les physiciens de leur sécurité, et dut leur faire sentir l'insuffisance du langage convenu, dont ils s'étoient fait une habitude. BACON l'avoit indiquée. " La chaleur, par les verres," dit-il, " acquiert de l'intensité ; en " est-il de même du froid ?"[*] C'est de lui, probablement, que quelques auteurs subséquens avoient emprunté la même idée.[†] Mais cette idée étoit restée sans exécution, jusqu'à l'époque, encore récente, où M. PICTET l'a réalisé: M. BERTRAND, Professeur de Mathématiques, lui en suggéra l'idée; et voici comment M. PICTET rend compte de l'expérience. " Je disposai l'ap- " pareil précisément comme pour la réflexion de la chaleur; " j'employai les deux miroirs d'étain, à la distance de 10½ pieds " l'un de l'autre. Au foyer de l'un étoit un thermométre d'air, " qu'on observoit avec les précautions requises; et au foyer de " l'autre, un matras plein de neige.—A l'instant où le matras " fut en expérience, le thermométre placé à l'autre foyer des- " cendit de plusieurs dégrés; il remonta dès qu'on enleva le " matras.—Après avoir remis le matras au foyer, et fait ainsi " descendre le thermométre jusqu'à un certain dégré, où il de- " meura stationnaire, je versai de l'acide nitreux sur la neige; et " le froid ainsi produit, fit à l'instant descendre le thermométre " de 5 à 6 dégrés plus bas."[*]

§ 32. A la vue de ce résultat, M. PICTET éprouva d'abord

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[*] *Instaurat.* l. 5. c. 2.

[†] " Les miroirs ardens concentrent la chaleur; peuvent-ils concentrer le froid?" *Logique de* FELICE, T. II. p. 62. J'ai ouï dire (mais je n'en ai point la preuve) que l'auteur de cette logique avoit fait usage des cahiers du célebre Professeur CRAMER.

[*] Essai sur le Feu, § 69.

quelque surprise; mais il n'hésita point à prononcer, que cette réflexion du froid n'étoit qu'apparente, et qu'elle ne pouvoit être que la réflexion de la chaleur, en sens inverse.

§ 33. Cependant, aucune explication fondée sur les idées de tension, de pression, d'équilibre sans mouvement, ne pouvoit faire comprendre comment cette marche inverse de la chaleur étoit déterminée. En effet, les miroirs, l'air, et tous les corps voisins du matras froid, étant tous entr'eux à même température, doivent, selon ces systêmes, lâcher leur chaleur vers ce gouffre, et non dans aucune autre direction. On n'y voit point de raison pour qu'un rayon parte du thermométre, et se porte vers le miroir dont il occupe le foyer.

§ 34. Accoutumé dès long-temps à envisager le feu sous un autre aspect, j'exposai ces difficultés, et je tâchai d'attirer l'attention des physiciens sur cet objet, dans un mémoire sur *l'Equilibre du Feu,** et dans mes *Recherches sur la Chaleur.*† Ces écrits sont, si je ne me trompe, les premiers où l'on ait proposé de substituer un équilibre mobile, à l'équilibre immobile que les physiciens ont coutume d'admettre en cette matière; et la conséquence de cette substitution fut, que le phénomène de la réflexion du froid s'expliqua aussi aisément, et aussi pleinement, que celui de la réflexion de la chaleur. C'est, je pense, un caractère de vérité; car on sent bien, que ces deux faits sont homogènes, et qu'une bonne théorie doit les expliquer à la fois, et les comprendre, pour ainsi dire, sous une même formule. Qu'il me soit permis de rappeler ici cette théorie, que j'ai eu la satisfaction de voir adopter par M. PICTET,‡ et par d'autres bons juges. Peu

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* Journal de Physique, Avril, 1791.

† Publiées à Genève, en 1792.

‡ Bibl. Brit. Sc. et Arts, T. IV. p. 30, et ailleurs.

de mots suffisent pour en faire saisir le principe ; c'est le seul but que je me propose ici.

§ 35. Le feu est un fluide discret, agité : chaque molécule de feu libre est mue avec une grande vîtesse ; l'une se meut dans un sens, l'autre dans l'autre, de sorte qu'en tout sens, un corps chaud émet des rayons calorifiques ; et ces molécules sont assez écartées les unes des autres, pour que deux ou plusieurs courans puissent s'entrecroiser, comme la lumière, sans se troubler mutuellement dans leur cours. Cette constitution du feu étant bien conçue, si l'on feint deux espaces voisins où il abonde, on verra, qu'entre ces espaces il y a de continuels échanges. Si, dans les deux espaces, le feu est également abondant, les échanges seront égaux, il y aura équilibre. Si l'un des espaces contient plus de feu que l'autre, les échanges seront inégaux ; le moins chaud recevra plus de molécules ignées qu'il n'en donnera ; et, après un temps suffisant, la répétition continuelle de ces échanges retablira l'équilibre.

§ 36. De ces principes découlent toutes les lois de la chaleur croissante et décroissante ; en particulier celle qui a servi de base à nos calculs comparatifs de la marche de deux thermométres exposés à une même source de chaleur, l'un sous une lame interceptante, et l'autre sans aucun obstacle. (§ 3)

En effet, supposons un corps placé dans un milieu plus chaud que lui, et que ce milieu jouisse toujours d'une température constante ; on doit considerer la chaleur du milieu comme composée de deux parties, l'une égale à celle du corps, l'autre égale à la différence des deux chaleurs. Quant à la première, les échanges sont égaux entre le corps et le milieu, il y a équilibre. L'excès de chaleur du milieu peut donc être considéré seul ; et, relativement à cet excès, le corps est absolument froid. Supposons, qu'en une seconde le corps reçoive la $\frac{1}{10}$me partie de ce feu; à la fin de de cette seconde, l'excès ne sera plus que de $\frac{9}{10}$. La $\frac{1}{10}$me de ce nouvel excès passera dans le corps pendant le cours de la 2me seconde, et l'excès sera réduit aux $\frac{9}{10}$mes des $\frac{9}{10}$mes. On voit, en suivant ce raisonnement, qu'à la fin de la 3me seconde, l'excès sera la 3me puissance de $\frac{9}{10}$; et ainsi de suite; de manière que, (conformément à la loi observée) les temps croissant selon une progression arithmétique 0, 1, 2, 3, &c. les différences décroissent selon une progression géometrique 1, $\frac{9}{10}$, $(\frac{9}{10})^2$, $(\frac{9}{10})^3$, &c.

On déduit, avec la même facilité, la même loi de réfroidissement, pour le corps plongé dans un milieu plus froid que lui.*

C'est ainsi que la vraie théorie de la chaleur, fondée sur des faits totalement différens de ceux par lesquels RICHMANN a prouvé cette loi, nous y ramène nécessairement.

§ 37. Nous avons vu, dans la 1re Partie, l'application de ce principe. Sous ce point de vue, les expériences de M. HERSCHEL acquièrent un grand intérêt, non seulement en confirmant la loi, mais en déterminant le quotient de la progression des différences dans l'échauffement de ses thermométres; ce qui ne peut manquer d'exciter sur cet objet l'attention des observateurs, et de donner des idées très précises sur le dégré de sensibilité de l'instrument qu'on emploie.

Cette remarque m'engage à ajouter encore ici le calcul d'une expérience de même genre, faite à la lumière réfléchie d'une chandelle. Cette expérience est rapportée dans un mémoire précédent du même auteur, lié étroitement avec celui que j'ai discuté.†

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* Recherches sur la Chaleur, § 19.

† Trans. Phil. pour 1800, p. 297. Exp. 2.

	Dégrés observés.	Dégrés calculés par le rapport de 65 à 51.
0′	432	432
1	440	440
2	448	446
3	456	451
4	458	455
5	458	458

La chaleur du rayon étoit ici de 469 huitièmes de dégré.

Les autres expériences de ce mémoire ne peuvent pas être aisément soumises au calcul, parce que, dans plusieurs, les temps ne sont pas en progression arithmétique; et que, dans d'autres, la chaleur du lieu varioit pendant le cours de l'expérience, indépendamment de celle qui étoit communiquée immédiatement par la source; ce qui trouble tous les résultats.

§ 38. La théorie exposée ci-dessus, (§ 35,) explique la réflexion du froid précisément comme la réflexion du chaud, sans plus ni moins de difficulté. Concevez, dans l'appareil du double miroir, deux thermométres, placés, l'un à un foyer, l'autre à l'autre; et d'abord, que ces deux thermométres soient au même dégré. Il y a équilibre; le feu émis par chacun, et renvoyé à l'autre, en vertu d'une double reflexion, se trouve exactement compensé par le feu que l'autre lui renvoie par la même voie, mais en sens contraire. Maintenant, concevons que l'un des thermométres hausse ou baisse; aussitot (les échanges étant inégaux) l'autre haussera ou baissera conformément.

§ 39. Cette théorie présente, en tout échauffement, trois espèces de chaleur. La 1re est celle qui est immédiatement reçue, dans un instant donné, par le corps qui s'échauffe. La 2de est la chaleur accumulée, et emmagazinée, dans ce même corps, en vertu de l'échauffement qui a eu lieu dans les instans précédens. La 3me est la chaleur rayonnante, qui est l'effet des deux précédentes, et qui sort incessamment du corps, à mesure que les autres y entrent. La considération de ces trois chaleurs distinctes, a de l'influence dans plusieurs phénomènes, surtout dans la météorologie. J'ai eu occasion de faire remarquer, que l'estimation de la température des saisons en dépend.*

En un mot, le nombre des faits auxquels cette théorie s'applique, est assez considérable pour inspirer quelque confiance; et je ne sais pas voir quelle difficulté réelle elle présente. †

§ 40. A la vérité, quelques physiciens semblent disposés à substituer dans la nature, les fluides continus aux fluides discrets, et le mouvement ondulatoire à celui de translation. Je pourrois dire, comme il est assez commode de faire, que je ne détermine rien à cet égard, et qu'on n'a qu'à mettre partout, dans ce qui precéde, des ondes qui se croisent, au lieu de courants et de particules distinctes. Mais je ne crois pas cette substitution légitime; et, sans parler de plusieurs raisons qui la combattent, il en est une générale, qui seule devroit, à ce qu'il me semble, la faire rejetter: les agens continus obstrueroient l'univers, et s'opposeroient aux mouvemens libres et rapides qu'on y observe.

§ 41. Pour me résumer, je dis, 1. Que l'effet d'une source de chaleur constante sur le thermométre, en un temps limité, n'est pas proportionnel à la chaleur de la source. 2. Qu'on a

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* Réflexions sur la Chaleur solaire, &c. Journ. de Phys. Fevrier, 1793.

† Je n'ai point parlé de la communication de la chaleur par les corps qui la conduisent; et je ne me suis point occupé, dans ce mémoire, du feu latent et combiné; ce n'étoit pas mon sujet. Il est du reste facile a voir, que ces effets ne contrarient en rien la théorie que j'ai exposé; mais s'allient, au contraire, très bien avec les phénomènes de la chaleur rayonnante et libre.

néanmoins un moyen de conclure la chaleur de la source, de son effet sur le thermométre; parcequ'on connoît la loi que suit cet effet, dans ses accroissemens successifs. 3. Que cette méthode est la seule qu'on doive employer, lorsqu'il s'agit de comparer deux sources de chaleur, d'après leur effet en un temps limité, moindre que celui qui est requis pour le maximum de l'effet. 4°. Que, lorsqu'il s'agit de chaleur transmise, il faut distinguer celle qui est transmise immédiatement, de celle que le corps transmettant y ajoute dès qu'il s'échauffe. 5. Que, lorsqu'on néglige cette distinction, l'interception de chaleur attribuée à la lame n'est qu'une limite de petitesse; ensorte qu'il reste indécis, si l'interception n'a pas été beaucoup plus grande, ou même totale. 6. Qu'en appliquant ces principes aux expériences de M. Herschel, l'appréciation devient plus exacte, mais dépend néanmoins de quelques circonstances accessoires, et jusqu'ici indéterminées. 7°. Que, dans ces mêmes expériences, la différence apparente entre l'interception de la chaleur et celle de la lumière, par les mêmes matières, n'établit aucune conclusion légitime sur la différence ou l'identité de la lumière et de la chaleur. 8. Que la loi mentionnée ci-dessus (et que j'ai énoncé au § 3) n'est pas seulement prouvée par l'expérience directe, mais par son accord avec la vraie théorie de la chaleur. 9. Que cette théorie est établie sur des faits variés, tout-à-fait différens de cette loi, en particulier sur la réflexion du froid; et qu'elle est la seule qui s'accorde avec les phénomènes généraux de la nature.

XVII. *Of the Rectification of the Conic Sections. By the Rev.* John Hellins, *B. D. F. R. S. and Vicar of Potter's-Pury, in Northamptonshire.*

Read July 8, 1802.

PART I.

Of the Rectification of the Hyperbola: containing several new Series for that Purpose; together with the Methods of computing the constant Quantities by which the ascending Series differ from the descending ones.

INTRODUCTION.

The conic sections are a part of geometry so requisite in mensuration, in optics, astronomy, and other branches of natural philosophy, that the properties of these curves have been much studied in the course of the last hundred and fifty years; and there is hardly a writer on fluxions, of any note, who has not treated of their rectification. It may therefore seem, that little is now left to the industry of the present and future generations, in this part of the mathematics, but the proper application of theorems already investigated. Yet, while we admire the skill, and praise the industry, of those who have discovered new truths, or thrown new light on old ones, within that period, we shall do well to recollect, that it is now no more than one hundred and thirty-seven years, since the two great discoveries of *fluxions* and *infinite series* were made by Sir Isaac Newton; and that the observation of the late Mr. Emerson, respecting the state of fluxions in his time, is in a great measure applicable to it in ours; viz. "*If arts and sciences of many hundred* "*years standing receive daily improvements and additions, it can-* "*not be supposed that this most sublime art of all, found out but* "*yesterday, can be arrived at perfection all on a sudden. If this* "*art be so exceedingly useful and valuable, it certainly deserves* "*the pains and attention of the learned mathematicians.*"*—And indeed, whoever considers the great number of mathematical and physico-mathematical problems which are solved by means of fluxions and series only, the several different ways in which series may be applied to the solution of the same problem, the fewness of those who employ themselves at all about these abstract sciences, and the still smaller number of those who have skill, leisure, and resolution enough to attempt any improvement in them; I say, whoever duly considers these things, (even without making allowance for the want of patronage which the liberal arts have of late years experienced,) will see reason to think, that many ages must yet elapse, before this most sublime and extensively useful method of computation will receive all the improvements of which it is capable. He will perceive, that, of the large field opened by Sir ISAAC NEWTON, a considerable part is still covered with briars and thorns. He will have no doubt, that the mine is not yet exhausted, but that, although the first workers of it have carried away the largest and most brilliant diamonds, enough still remain to reward the labour of those who shall have the resolution to dig deeper, and the patience of those who shall yet carefully sift the rubbish which has been thrown up by former adventurers.

The subject of the following sheets was first offered to my

* Preface to his Fluxions.

consideration in the year 1770, when a problem requiring the rectification of a large portion of an equilateral hyperbola was proposed in a periodical work; which problem I then solved by means of descending series; but, for want of an easy method of correcting the fluent so found, I laid it aside for the exercise of maturer judgment. Afterwards, the subject was resumed at different times, as leisure permitted, and put into nearly the same form in which it now appears, in the year 1795; since which time, till now, the duties of my station, and unexpected occurrences, have left me no opportunity to revise my papers.

As the investigation of the following theorems is very obvious and easy, I thought it probable that they might have been discovered by some other person before me; yet, upon perusal of Dr. HUTTON's *mathematical and philosophical Dictionary*, lately published, I find but one of them. And since, in the compilation of that work, as the learned and industrious author professes, "*Not only most of the encyclopedias already extant, and the*
"*various transactions of the learned societies throughout Europe,*
"*have been carefully consulted, but also all the original works, of*
"*any reputation, which have hitherto appeared upon these subjects,*"[*] I therefore conclude that all the rest are new.

The subject of this Paper is naturally divided into three sections: the first containing the investigations of the several series; the second, the methods of computing the constant quantities by which the ascending series differ from the descending ones; and the third, examples of their use, by way of illustration. But, for more convenient reference, I have further divided it into articles, or minor sections.

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[*] See Dr. HUTTON's Address to the public, on the publication of the first part of the *Dictionary* above mentioned, in 1795; and preface to the *Dictionary*.

SECT. I. *The Investigations of the several Series.*

1. That the following processes may not be incumbered with symbols, and that the rate of convergency of the series obtained therefrom may be the more obvious, let the transverse axis of any hyperbola be called $2a$, and the conjugate axis 2; (by which notation, any ratio that these two lines can possibly have to each other may be expressed;)* let the abscissa be called x, the corresponding ordinate to the axis, y, and the length of the curve from the vertex to the ordinate, z. Then, by the well-known property of the curve, we have $2ax + xx = aayy$; from which x is found $= a\sqrt{(1 + yy)} - a$, and $\dot{x} = \frac{ay\dot{y}}{\sqrt{(1 + yy)}}$, and $\dot{z} = \sqrt{(\dot{y}\dot{y} + \dot{x}\dot{x})} = \dot{y}\sqrt{\left(1 + \frac{aayy}{1 + yy}\right)} = \frac{\dot{y}\sqrt{(1 + yy + aayy)}}{\sqrt{(1 + yy)}}$; which equation, by writing ee for $1 + aa$, will become $\dot{z} = \frac{\dot{y}\sqrt{(1 + eeyy)}}{\sqrt{(1 + yy)}}$.

2. Now, the fluent of the expression on the right-hand side of the last equation may be taken in different series, according as the numerator or denominator of it is converted into series, and according as 1, $eeyy$, or yy, is made the leading term. By converting the numerator, $\sqrt{(1 + eeyy)}$, into series, making 1 the leading term, we get $\dot{z} = \frac{\dot{y}}{\sqrt{(1 + yy)}} \times : 1 + \frac{eeyy}{2} - \frac{e^4y^4}{2.4} + \frac{3e^6y^6}{2.4.6} - \frac{3.5e^8y^8}{2.4.6.8}$, &c. and then, by taking the fluents of $\frac{\dot{y}}{\sqrt{(1 + yy)}}$, $\frac{\dot{y}yy}{\sqrt{(1 + yy)}}$, $\frac{\dot{y}y^4}{\sqrt{(1 + yy)}}$, &c. and denoting them by A, B, C, &c. respectively, we shall have

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* With respect to homogeneity, about which some have shown more scrupulosity than discernment, I shall add a few words in a subsequent part.

$$A = \text{H. L. of } \overline{y + \surd(1+yy)},$$

$$B = \frac{y\surd(1+yy) - A}{2},$$

$$C = \frac{y^3\surd(1+yy) - 3B}{4},$$

$$D = \frac{y^5\surd(1+yy) - 5C}{6},$$

$$E = \frac{y^7\surd(1+yy) - 7D}{8},$$

&c. &c.

And, lastly, by multiplying these quantities by their proper coefficients, we obtain (THEOREM I,)

$z = A + \frac{ee}{2}B - \frac{e^4}{2.4}C + \frac{3e^6}{2.4.6}D - \frac{3.5e^8}{2.4.6.8}E$, &c. where it is manifest that, unless the quantities B, C, D, &c. decrease in the ratio of ee to 1, the series will at last cease to converge; or, in other words, if yy be greater than $\frac{1}{ee}$, the terms of the series, at a great distance from the first, will diverge. And, of the nine theorems now produced, this is the only one that I have found in any other book.

3. But, by converting $\surd(1+yy)$, the denominator of the fraction in the fluxionary equation in Art. I. into series, making 1 the leading term, we have $\dot{z} = \dot{y}\surd(1+eeyy) \times : 1 - \frac{yy}{2} + \frac{3y^4}{2.4} - \frac{3.5y^6}{2.4.6} + \frac{3.5.7y^8}{2.4.6.8}$, &c. and, by taking the fluents of $\dot{y}\surd(1+eeyy)$, $\dot{y}yy\surd(1+eeyy)$, $\dot{y}y^4\surd(1+eeyy)$, &c. and calling them A, B, C, &c. we shall have

$$A = \frac{y}{2}\surd(1+eeyy) + \frac{1}{2e} \times \text{H.L.} \overline{ey + \surd(1+eeyy)},$$

$$B = \frac{y(1+eeyy)^{\frac{3}{2}} - A}{4ee},$$

$$C = \frac{y^3(1+eeyy)^{\frac{3}{2}} - 3B}{6ee},$$

$$D = \frac{y^5(1+eeyy)^{\frac{3}{2}} - 5C}{8ee},$$

$$E = \frac{y^7(1+eeyy)^{\frac{3}{2}} - 7D}{10ee},$$

&c. &c.

And then, by multiplying these quantities by their respective coefficients, we obtain (Theorem II,)

$$z = A - \tfrac{1}{2}B + \tfrac{3}{2.4}C - \tfrac{3.5}{2.4.6}D + \tfrac{3.5.7}{2.4.6.8}E, \&c.$$

which series will converge till y becomes greater than 1; and consequently is a better series than that above found, which ceases to converge when y becomes greater than $\frac{1}{e}$. But, when y is much greater than 1, each of these series will diverge very swiftly; and, notwithstanding they are of that form which admits of a transformation to others which will converge, still, even by that means, their values will not be obtained without great labour. But here we shall have the pleasure of finding series which will quickly answer the purpose. For,

4. By converting the denominator, $\sqrt{(yy+1)}$, into series, making yy the leading term, we get $\dot{z} = \dot{y}\sqrt{(ee\,yy+1)}$ $\times : \frac{1}{y} - \frac{1}{2y^3} + \frac{3}{2.4y^5} - \frac{3.5}{2.4.6y^7} + \frac{3.5.7}{2.4.6.8y^9}$, &c.

And here, again, by denoting the fluents of $\frac{\dot{y}\sqrt{(ee\,yy+1)}}{y}$, $\frac{\dot{y}\sqrt{(ee\,yy+1)}}{y^3}$, $\frac{\dot{y}\sqrt{(ee\,yy+1)}}{y^5}$, &c. by A, B, C, &c. respectively, we shall have $A = \sqrt{(ee\,yy+1)} + \text{H. L.}\ \frac{\sqrt{(ee\,yy+1)}-1}{ey}$,

$$\begin{aligned}
B &= \frac{-(ee\,yy+1)^{\frac{3}{2}}}{2yy} + \frac{eeA}{2},\\
C &= \frac{-(ee\,yy+1)^{\frac{3}{2}}}{4y^4} - \frac{eeB}{4},\\
D &= \frac{-(ee\,yy+-)^{\frac{3}{2}}}{6y^6} - \frac{3eeC}{6},\\
E &= \frac{-(ee\,yy+1)^{\frac{3}{2}}}{8y^8} - \frac{5eeD}{8},\\
&\&c. \qquad \&c.
\end{aligned}$$

and then, (Theorem III,)

$$z = \begin{cases} A - \tfrac{1}{2}B + \tfrac{3}{2.4}C - \tfrac{3.5}{2.4.6}D + \tfrac{3.5.7}{2.4.6.8}E, \&c. \\ -d: \end{cases}$$

which series will converge the swifter the greater y is in comparison of 1, but will diverge when y is less than 1. It also wants a correction, (here denoted by the letter d,) which shall be given in its due place. This series then, when y becomes great in comparison of 1, will converge very swiftly, and becomes useful in those cases where the ascending series above investigated fail.

But, since the value of z may be expressed in another descending series, it will be proper to consider that also.

5. The expression $\frac{\dot{y}\sqrt{(1+eeyy)}}{\sqrt{(1+yy)}}$ is evidently $=\frac{e\dot{y}y}{\sqrt{(1+yy)}}\sqrt{(1+\frac{1}{eeyy})}$, which, by converting $\sqrt{(1+\frac{1}{eeyy})}$ into series, making 1 the leading term, becomes $\frac{e\dot{y}y}{\sqrt{(1+yy)}} \times : 1 + \frac{1}{2eeyy} - \frac{1}{2.4e^4y^4} + \frac{3}{2.4.6e^6y^6} - \frac{3.5}{2.4.6.8e^8y^8}$, &c. Here, the fluent of $\frac{e\dot{y}y}{\sqrt{(1+yy)}}$, the first term of the series, is $e\sqrt{(1+yy)}$; and, calling the fluents of $\frac{\dot{y}}{y\sqrt{(1+yy)}}$, $\frac{\dot{y}}{y^3\sqrt{(1+yy)}}$, $\frac{\dot{y}}{y^5\sqrt{(1+yy)}}$, &c. A, B, C, &c. respectively, we have

$$A = \text{H. L.}\ \frac{\sqrt{(yy+1)}-1}{y},$$
$$B = \frac{-\sqrt{(yy+1)}}{2yy} - \frac{A}{2},$$
$$C = \frac{-\sqrt{(yy+1)}}{4y^4} - \frac{3B}{4},$$
$$D = \frac{-\sqrt{(yy+1)}}{6y^6} - \frac{5C}{6},$$

&c. &c.

and thence, (THEOREM IV,)

$$z = \begin{cases} e\sqrt{(yy+1)} \\ +\frac{1}{2e}A - \frac{1}{2.4e^3}B + \frac{3}{2.4.6e^5}C - \frac{3.5}{2.4.6.8e^7}D, \text{ \&c.} \\ -d: \end{cases}$$

which series will converge the swifter, the greater y is in comparison of 1, and has an evident advantage over the last, in that it converges by the powers of ee, as well as by those of yy; so that its convergency will not cease, till the quantities B, C, D, &c. increase in the ratio of 1 to ee, that is, when y becomes equal to, or less than, $\frac{1}{e}$. This series, therefore, will be very useful for the greatest part of the hyperbola, when it is corrected by the constant quantity here denoted by d, the value of which is attainable several ways, as will appear in the next section.

6. These four theorems, or indeed two of them only, are sufficient for the rectification of any portion whatever of any conical hyperbola. Yet, as I have discovered several other series for that purpose, which are more convenient in particular cases, and of which some are useful in computing the constant quantity above denoted by d, (by which the ascending series differ from the descending ones,) it may be proper now to give the investigations of them also.

7. Put $1 + ee\,yy = uu$; then will yy be $= \frac{uu-1}{ee}$, and $1 + yy$ $= \frac{uu+ee-1}{ee} =$ (by the notation in Art. 1, where ee was put $= aa + 1$,) $\frac{uu+aa}{ee}$, and therefore $\sqrt{(1+yy)} = \frac{\sqrt{(uu+aa)}}{e}$, and thence $\sqrt{\left(\frac{1+ee\,yy}{1+yy}\right)} = \frac{eu}{\sqrt{(uu+aa)}}$. Moreover, $\dot{y}$ will be $=$ $\frac{\dot{u}u}{e\sqrt{(uu-1)}}$, and we shall have $\dot{y}\sqrt{\left(\frac{1+ee\,yy}{1+yy}\right)} = \dot{z} =$ $\frac{\dot{u}\,uu}{\sqrt{(uu-1)}\times\sqrt{(uu+aa)}} = \frac{\dot{u}\,u}{\sqrt{(uu-1)}\times\sqrt{\left(1+\frac{aa}{uu}\right)}} = \frac{\dot{u}\,u}{\sqrt{(uu-1)}} \times : 1 -$ $\frac{aa}{2uu} + \frac{3a^4}{2.4u^4} - \frac{3.5a^6}{2.4.6u^6} + \frac{3.5.7a^8}{2.4.6.8u^8}$, &c. Now the fluent of $\frac{\dot{u}\,u}{\sqrt{(uu-1}}$ is $\sqrt{(uu-1)}$; and, if the fluents of $\frac{\dot{u}}{u\sqrt{(uu-1)}}$, $\frac{\dot{u}}{u^3\sqrt{(uu-1)}}$, $\frac{\dot{u}}{u^5\sqrt{(uu-1)}}$, &c. are denoted by A, B, C, &c. respectively, we shall have

A = circ. arch, rad. being 1, and sec. u,

$B = \frac{\sqrt{(uu-1)}}{2uu} + \frac{A}{2}$,

$C = \frac{\sqrt{(uu-1)}}{4u^4} + \frac{3B}{4}$,

$D = \frac{\sqrt{(uu-1)}}{6u^6} + \frac{5C}{6}$,

&c. &c.

And, lastly, by multiplying these quantities by their proper coefficients, and collecting the several terms in due order, we shall have (THEOREM V,)

$$z = \sqrt{(uu-1)} - \frac{aa}{2}A + \frac{3a^4}{2.4}B - \frac{3.5a^6}{2.4.6}C + \frac{3.5.7a^8}{2.4.6.8}D, \text{ \&c.}$$

Here it is remarkable, that the terms $\frac{\sqrt{(uu-1)}}{2uu}$, $\frac{\sqrt{(uu-1)}}{4u^4}$, $\frac{\sqrt{(uu-1)}}{6u^6}$, &c. which enter into the values of B, C, D, &c. always decrease while y increases from 0 *ad infinitum;* and indeed decrease more swiftly than the terms of either of the descending series in the preceding articles; and therefore this series may be used for computing the length of any portion of the hyperbola. For although the terms of it, taken at a great distance from the first, will diverge by the powers of aa, when a is greater than 1, yet, as the signs of these terms are alternately + and −, it admits of an easy transformation into another series, which will always converge by the powers of $\frac{aa}{1+aa}$. It also wants no correction; in consequence of which it has a peculiar use, which will appear in the next section.

8. But the fluxionary expression $\frac{\dot{u}\,uu}{\sqrt{(uu-1)}\times\sqrt{(uu+aa)}}$, obtained in the preceding Art. is $= \frac{\dot{u}\,u}{\sqrt{(uu+aa)}\times\sqrt{(1-\frac{1}{uu})}} = \frac{\dot{u}\,u}{\sqrt{(uu+aa)}} \times :$ $1 + \frac{1}{2uu} + \frac{3}{2.4u^4} + \frac{3.5}{2.4.6u^6} + \frac{3.5.7}{2.4.6.8u^8}$, &c. Here the fluent

of $\frac{\dot{u}u}{\sqrt{(uu+aa)}}$ is $\sqrt{(uu+aa)}$; and, if the fluents of $\frac{\dot{u}}{u\sqrt{(uu+aa)}}$, $\frac{\dot{u}}{u^3\sqrt{(uu+aa)}}$, $\frac{\dot{u}}{u^5\sqrt{(uu+aa)}}$, &c. are denoted by A, B, C, &c. we shall have

$$\begin{aligned}
A &= \frac{1}{a}\ \text{H. L.}\ \frac{\sqrt{(uu+aa)}-a}{u}, \\
B &= \frac{-\sqrt{(uu+aa)}}{2aa\,uu} - \frac{A}{2aa}, \\
C &= \frac{-\sqrt{(uu+aa)}}{4aa\,u^4} - \frac{3B}{4aa}, \\
D &= \frac{-\sqrt{(uu+aa)}}{6aa\,u^6} - \frac{5C}{6aa}, \\
&\text{&c.} \qquad \text{&c.}
\end{aligned}$$

And, by multiplying these quantities by their proper coefficients, and collecting the products together, we shall have (THEOREM VI,)

$$z = \begin{cases} \sqrt{(uu+aa)} + \frac{1}{2}A + \frac{3}{2.4}B + \frac{3.5}{2.4.6}C + \frac{3.5.7}{2.4.6.8}D, \text{ &c.} \\ -\,d. \end{cases}$$

Here also, the terms $\frac{\sqrt{(uu+aa)}}{2aa\,uu}$, $\frac{\sqrt{(uu+aa)}}{4aa\,u^4}$, $\frac{\sqrt{(uu+aa)}}{6aa\,u^6}$, &c. which are component parts of this series, always decrease while y increases from 0 *ad infinitum;* and therefore the length of any portion whatever of the hyperbola may be computed by this series also, when the value of the constant quantity d, to be taken from it, is known. But the case to which this theorem ought to be applied is, when y is equal to, or greater than 1. And it has an advantage over some of the descending series, in that the terms $\frac{A}{2}$, $\frac{3}{4}B$, $\frac{5}{6}C$, &c. are divided by aa, as will appear in the use of it.

9. When $a = 1$, that is, when the hyperbola is equilateral, the fluxionary equation in Article 7 becomes $\dot{z} = \frac{\dot{u}uu}{\sqrt{(uu-1)} \times \sqrt{(uu+1)}}$

$$=\frac{\dot u\, uu}{\sqrt{(u^4-1)}}=\frac{\dot u}{\sqrt{\left(1-\frac{1}{u^4}\right)}}=\dot u+\frac{\dot u}{2u^4}+\frac{3\dot u}{2.4u^8}+\frac{3.5\dot u}{2.4.6^{12}}+\frac{3.5.7\dot u}{2.4.6.8u^{16}},$$

&c.; the correct fluents of which are (THEOREM VII,)

$$z=\begin{cases} u-\frac{1}{2.3u^3}-\frac{3}{2.4.7u^7}-\frac{3.5}{2.4.6.11u^{11}}-\frac{3.5.7}{2.4.6.8.15u^{15}},\ \&c. \\ -d. \end{cases}$$

Which series is better adapted to this case than either of the preceding ones, in that it is much simpler, and converges twice as fast. And the correction of it is easily attainable by various methods.

10. But the original fluxionary equation in Art. 1, admits of a conversion into series, two different ways from any of those which have yet been taken. For, by the Binomial theorem, $\dot y\sqrt{\left(1+\frac{aa\,yy}{1+yy}\right)}$ is $=\dot y+\frac{aa}{2}\cdot\frac{\dot y\,yy}{1+yy}-\frac{a^4}{2.4}\cdot\frac{\dot y\,y^4}{(1+yy)^2}+\frac{3a^6}{2.4.6}\cdot\frac{\dot y\,y^6}{(1+yy)^3}-\frac{3.5a^8}{2.4.6.8}\cdot\frac{\dot y\,y^8}{(1+yy)^4}$, &c. where, putting A, B, C, &c. for the fluents of $\frac{\dot y\,yy}{1+yy}$, $\frac{\dot y\,y^4}{(1+yy)^2}$, $\frac{\dot y\,y^6}{(1+yy)^3}$, &c. respectively, we have

$A=y$ — circ. arch, rad. 1, and tang. y,

$B=-\frac{y^3}{2\,(1+yy)}+\frac{3A}{2}$,

$C=-\frac{y^5}{4\,(1+yy)^2}+\frac{5B}{4}$,

$D=-\frac{y^7}{6\,(1+yy)^3}+\frac{7C}{6}$,

&c. &c.

and thence (THEOREM VIII,)

$$z=y+\frac{aa}{2}A-\frac{a^4}{2.4}B+\frac{3a^6}{2.4.6}C-\frac{3.5a^8}{2.4.6.8}D,\ \&c.$$

In which series, it is pretty evident, the quantities A, B, C, D, &c. will have a convergency while y increases from 0 *ad infinitum*, although the convergency will be but slow after y becomes greater than 1. It is obvious too, that this series vanishes together with y, and therefore needs no correction. And for this reason chiefly I have introduced it, as it affords us another mean of obtaining the value of the constant quantity d, by which the descending series are to be corrected.

11. But the fluxionary expression $\dot{y}\surd\left(\frac{1+ee\,yy}{1+yy}\right)$, obtained in Art. 1, is evidently $=\dot{y}\surd\left(ee+\frac{1-ee}{1-yy}\right)=$ also to $\dot{y}\surd\left(ee-\frac{aa}{1+yy}\right)$; and this expression converted into series, by the Binomial theorem, becomes $e\dot{y}-\frac{aa\dot{y}}{2e(1+yy)}-\frac{a^4\dot{y}}{2\;4e^3(1+yy)^2}-\frac{3a^6\dot{y}}{2.4.6e^5(1+yy)^3}-\frac{3.5a^8\dot{y}}{2.4.6.8e^7(1+yy)^4}$, &c. Here again, denoting the fluents of $\frac{\dot{y}}{1+yy}$, $\frac{\dot{y}}{(1+yy)^2}$, $\frac{\dot{y}}{(1+yy)^3}$, &c. by A, B, C, &c. we shall have

$$A = \text{circ. arch, rad. } 1\text{, and tang. } y,$$

$$B=\frac{y}{2(1+yy)}+\frac{A}{2},$$

$$C=\frac{y}{4(1+yy)^2}+\frac{3B}{4},$$

$$D=\frac{y}{6(1+yy)^3}+\frac{5C}{6},$$

&c. &c.

And, by proceeding as before directed, we get (THEOREM IX,)

$$z=ey-\frac{aa}{2e}A-\frac{a^4}{2.4e^3}B-\frac{3a^6}{2.4.6e^5}C-\frac{3.5a^8}{2.4.6.8e^7}D\text{, \&c.}$$

And this series, it is obvious, will converge the swifter the greater y is, so that it will begin to converge swiftly when the preceding series begins to converge slowly. It is evident too, that this series vanishes together with y, and therefore wants no correction. Moreover, it has an advantage over the preceding series, in that the coefficients of it decrease by the powers of $\frac{aa}{ee}$, that is, by $\frac{aa}{1+aa}$. And it supplies us with a different expression of the value of d, as will appear in the next section, to which I now proceed.

SECT. II. *The Methods of computing the Values of the constant Quantities by which the ascending Series differ from the descending ones.*

12. Now the methods of obtaining these constant quantities are such as are shewn in my *Mathematical Essays*, (published in 1788,) pages 100, 101, 102, &c. to 112; viz. either by computing the value of both an ascending and a descending series, taking for y some small definite quantity, or by comparing the values of those series together when y is taken immensely great: the former of which methods is more general, but the latter, when it can be applied, commonly affords the easiest computation. In this section, I shall make use of both these methods, as the one or the other is best suited to the case in hand. I begin with the use of the latter method, in comparing together all the different expressions of the value of z, which are reduced to few terms in the case when y becomes immensely great.

Now, when y is taken immensely great, the value of z in THEOREM III. Art. 4, becomes barely $= ey - d$. For, in this case, the H. L. $\frac{\sqrt{(eeyy+1)}-1}{ey}$ becomes the logarithm of the ratio of equality, which is $= 0$. And then A is barely $= \sqrt{(eeyy+1)} + 0 = ey + \frac{1}{2ey} - \frac{1}{2.4e^3y^3}$, &c. all which terms, after the first, vanish in this case; and therefore eeA, which occurs in the value of B, becomes barely e^3y. Moreover, the radical expression $\frac{-(eeyy+1)^{\frac{3}{2}}}{2yy}$, which enters into the value of B, becomes barely $= \frac{-e^3y}{2}$; and thence we have $\text{B} = \frac{-e^3y+e^3y}{2} = 0$. And, since each of the expressions $\frac{(eeyy+1)^{\frac{3}{2}}}{4y^4}$, $\frac{(eeyy+1)^{\frac{3}{2}}}{6y^6}$, &c. evidently becomes $= 0$, in this case, and since B has been shown to be $= 0$, it will thence follow, that all the terms denoted by C, D, E, &c. will vanish, and there will be left $z = ey - d$.

13. And in like manner it will appear, that the value of z in THEOREM IV, Art. 5, when y becomes immensely great, is also $= ey - d$. For, in this case, H. L. $\frac{\sqrt{(yy+1)} - 1}{y}$ becomes $= 0$; and each of the expressions $\frac{\sqrt{(yy+1)}}{2yy}$, $\frac{\sqrt{(yy+1)}}{4y^4}$, &c. also becomes $= 0$; and, consequently, $z = \begin{cases} e\sqrt{(yy+1)} \\ -d. \end{cases}$ But, since e is a finite quantity, the expression $e\sqrt{(yy+1)} = ey + \frac{e}{2y} - \frac{e}{2.4y^3}$, &c. when y is immensely great, becomes barely $= ey$. Therefore, in this case, we have $z = ey - d$.

14. COROLLARY. And hence it appears, that the series in these two theorems are equal to each other, and, consequently, that the constant quantity to be subtracted from each of them, by way of correction, is the same.

15. The first term of the series which expresses the value of z in Theorem V, Art. 7, is $\sqrt{(uu-1)}$, which, by the notation there used, is always $= ey$. And, when y becomes immensely great, the terms $\frac{\sqrt{(uu-1)}}{2uu}$, $\frac{\sqrt{(uu-1)}}{4u^4}$, $\frac{\sqrt{(uu-1)}}{6u^6}$, &c. which enter into the values of B, C, D, &c. vanish; but A becomes = *the quadrantal arch of the circle, of which the radius is* 1; and thence we have $B = \frac{A}{2}$, $C = \frac{3}{4} B, = \frac{3}{2.4} A$, $D = \frac{5}{6} C = \frac{3.5}{2.4.6} A$, &c. and these values being written for B, C, D, &c. in the series, we have, in this case, $z = ey - \frac{aa}{2} A + \frac{3a^4}{2.2.4} A - \frac{3.3.5a^6}{2.2.4.4.6} A + \frac{3.3.5.5.7a^8}{2.2.4.4.6.6.8} A$, &c. And, since this series always gives the correct value of z, we have now discovered the value of d, the constant quantity to be subtracted from the

descending series given in Theorems III and IV. The series to which d is $=$, viz. $A \times : \frac{aa}{2} - \frac{3a^4}{2.2.4} + \frac{3.3.5a^6}{2.2.4.4.6} - \frac{3.3.5.5.7a^8}{2.2.4.4.6.6.8}$, &c. will indeed diverge when a is greater than 1; yet, as was observed in Art. 7, it is of that form which admits of transformation into another which will always converge.

16. For the reasons above given in Articles 12 and 13, each of the terms A, B, C, &c. in Theorem VI, Art. 8, vanishes when y becomes immensely great, and z is then barely $= \begin{cases} \sqrt{(uu + aa)} \\ - d. \end{cases}$ And, since $\sqrt{(uu + aa)}$ is, by the notation in Art. 1 and 7, $= \sqrt{(ee\,yy + ee)}$, which, in this case, becomes barely $= ey$, we have $z = ey - d$. Here we see that the series in this Theorem, and in Theorems III and IV, are always $=$ to each other, and consequently differ from each of the ascending series by the same constant quantity d, the value of which was discovered in the preceding Article.

17. When y becomes immensely great, the value of z in Theorem VII, Art. 9, becomes barely $= u - d$. And, since u is universally $= \sqrt{(ee\,yy + 1)}$, it will, in this case, be $= ey$; and we shall have $z = ey - d$, which is the very expression given by all the other descending series in the like case. But, when the hyperbola is equilateral, as was supposed in Art. 9, a is $= 1$, and we have $d = 1{\cdot}57079632 \times : \frac{1}{2} - \frac{3}{2.2.4} + \frac{3.3.5}{2.2.4.4.6} - \frac{3.3.5.5.7}{2.2.4.4.6.6.8}$, &c.

Moreover, when y is $= 0$, z is also $= 0$, and u is $= 1$; and therefore, by this theorem, we have $0 =$

$$\begin{cases} 1 - \frac{1}{2.3} - \frac{3}{2.4.7} - \frac{3.5}{2.4.6.11} - \frac{3.5.7}{2.4.6.8.15}, \text{ \&c. or} \\ - d \end{cases}$$

$$d = 1 - \frac{1}{2.3} - \frac{3}{2.4.7} - \frac{3.5}{2.4.6.11} - \frac{3.5.7}{2.4.6.8.15}, \text{ \&c.}$$

And hence it follows, that this very slowly converging series is $= 1{\cdot}57079632 \times : \frac{1}{2} - \frac{3}{2.2.4} + \frac{3.3.5}{2.2.4.4.6} - \frac{3.3.5.5.7}{2.2.4.4.6.6.8}$, &c. by which expression its value is easily attainable, and will be found to be $= 0{\cdot}59907012$.

I observe, *in transitu*, that the ratio of this slowly converging series, $1 - \frac{1}{2.3} - \frac{3}{2.4.7} - \frac{3.5}{2.4.6.11} - \frac{3.5.7}{2.4.6.8.15}$, &c. to a series of good convergency, is easily attainable; by which mean we may likewise compute its value to any degree of exactness.

18. A general expression of the value of d being found in Art. 15, by which it may be computed, whatever be the ratio of the two axes of the hyperbola, I might now proceed to show the use of the theorems by a few examples; but, as the same series is attainable another way, and the same value of d is attainable also by different series, it will be no less curious than useful to show in what manner.

19. The nth term of the series of quantities $\frac{-y^3}{2(1+yy)}$, $\frac{-y^5}{4(1+yy)^2}$, $\frac{-y^7}{6(1+yy)^3}$, &c. which enter into the values of B, C, D, &c. in Theorem VIII, Art. 10, is evidently $\frac{-y^{2n+1}}{2n(1+yy)^n}$, which, by the Binomial theorem, is $= \frac{-y^{2n+1}}{2n} \times : y^{-2n} - ny^{-2n-2} + n . \frac{n+1}{2} . y^{-2n-4} - n . \frac{n+1}{2} . \frac{n+2}{2} y^{-2n-6}$, &c. $= -\frac{y}{2n} + \frac{1}{2y} - \frac{n+1}{4y^3}$, &c. which, when y becomes immensely great, is barely $= \frac{y}{2n}$. And the value of A, in this case, is y — *the quadrantal arch of a circle, of which the radius is* 1. Let this quadrantal arch be denoted by α; then, by substituting for A, B, C, &c. their proper values as they thus arise, we have

$$\begin{aligned}
A &= \ldots\ldots\ldots\ldots\ldots\ldots \; y - \alpha,\\
B &= -\frac{y}{2} + \frac{3y}{2} - \frac{3}{2}\alpha = y - \frac{3}{2}\alpha,\\
C &= -\frac{y}{4} + \frac{5y}{4} - \frac{3\cdot5}{2.4}\alpha = y - \frac{3\cdot5}{2.4}\alpha,\\
D &= -\frac{y}{6} + \frac{7y}{6} - \frac{3\cdot5\cdot7}{2.4.6}\alpha = y - \frac{3\cdot5\cdot7}{2.4.6}\alpha,\\
&\text{\&c.} \qquad\qquad \text{\&c.}
\end{aligned}$$

And, lastly, by writing these values of A, B, C, &c. in the Theorem, we have, in this case,

$$z = \begin{cases} y + \frac{aa}{2}y - \frac{a^4}{2.4}y + \frac{3a^6}{2.4.6}y - \frac{3\cdot5a^8}{2.4.6.8}y, \ \text{\&c.}\\ -\frac{aa}{2}\alpha + \frac{3a^4}{2.2.4}\alpha - \frac{3\cdot3\cdot5a^6}{2.2.4.4.6}\alpha + \frac{3\cdot3\cdot5\;5\;7a^8}{2.2.4.4.6.6.8}\alpha, \ \text{\&c.} \end{cases}$$

But the series $y + \frac{aa}{2}y - \frac{a^4}{2.4}y + \frac{3a^6}{2.4.6}y - \frac{3\cdot5a^8}{2.4.6.8}y$, &c. is evidently $= y\sqrt{(1 + aa)}$, which, by the notation in Art. 1, is $= ey$. We therefore have, in this case,

$$z = ey - \alpha \times : \frac{aa}{2} - \frac{3a^4}{2.2.4} + \frac{3\cdot3\cdot5a^6}{2.2.4.4.6} - \frac{3\cdot3\cdot5\cdot5\cdot7a^8}{2.2.4.4.6.6.8}, \ \text{\&c.}$$

And, since this theorem always gives the correct value of z, we have now the satisfaction of seeing a confirmation of the truth of our conclusion in Art. 15, by obtaining the very same expression by a very different process.

20. From what has been shewn in Articles 12, 13, &c. it will be very evident to any one who runs his eye over the component parts of the series given in Theorem IX, Art. 11, that, when y becomes immensely great, A becomes = *the quadrantal arch of a circle, of which the radius is* 1, which arch was denoted in the preceding Art. by α; and that

$$\begin{aligned}
B &= 0 + \frac{A}{2} = \frac{1}{2}\alpha,\\
C &= 0 + \frac{3B}{4} = \frac{3}{2.4}\alpha,\\
D &= 0 + \frac{5C}{6} = \frac{3\cdot5}{2.4.6}\alpha,\\
&\text{\&c.} \qquad \text{\&c.}
\end{aligned}$$

And, these values being written for A, B, C, &c. in the Theorem, it gives, in this case,

$$z = ey - \frac{aa}{2e}\alpha - \frac{a^4}{2.2.4e^3}\alpha - \frac{3.3a^6}{2.2.4.4 6e^5}\alpha - \frac{3.3.5.5a^8}{2.2.4.4.6.6.8e^7}\alpha, \text{ \&c.}$$

And, since this Theorem also always gives the correct value of z, we shall, by comparing the expression now obtained with those which were found for z, in the like case, in Articles 12, 13, 15, 16, and 19, see that we have now got another general expression of the value of d, viz. $\alpha \times : \frac{aa}{2e} + \frac{a^4}{2.2.4e^3} + \frac{3.3a^6}{2.2.4.4 6e^5} + \frac{3.3 5.5a^8}{2.2.4.4 6.6.8e^7}$, &c. in which series ee is $= aa + 1$, and therefore it must always converge. Yet it should not be hastily concluded, that this expression of the value of d is always preferable to that which was obtained in Articles 15 and 19; for, when a is a large number, the powers of $\frac{aa}{ee} = \frac{aa}{1 + aa}$, by which the series converges, will decrease very slowly.

21. However, when it happens that a is a large number, the value of d may be obtained by means of two series, which, in that case, will converge pretty swiftly; or indeed by means of three series, each of which will converge about twice as fast as either of the two series. But, for the sake of brevity, I shall at present describe the method of computing the value of d by two series only, and so conclude this section.

The series proper to be used on this occasion, it is obvious, are those which are given in Theorems II and IV, Articles 3 and 5; and the value of y to be assumed, is $\frac{1}{\sqrt{e}}$, with which value each of the series will have nearly the same rate of convergency. As this will best appear by an example, I will give one, taking $a = 7$. Now, with this value of a, we have $ee = aa + 1 = 50$, and $y = \frac{1}{\sqrt{e}} = \frac{1}{\sqrt{50}} = 0.141421356$; and, by writing these values for e and y in Theorem II, Art. 3, we have

$$A = \frac{1}{2\surd e}\surd(1+e) + \frac{1}{2e}\,\text{H.L.}\left(\surd e + \surd(1+e)\right) = 0{\cdot}6547{,}320,$$

$$B = \frac{e^{-\frac{1}{2}}(1+e)^{\frac{3}{2}} - A}{4ee} = 0{\cdot}0398{,}409,$$

$$C = \frac{e^{-\frac{3}{2}}(1+e)^{\frac{3}{2}} - 3B}{6ee} = 0{\cdot}0036{,}665,$$

$$D = \frac{e^{-\frac{5}{2}}(1+e)^{\frac{3}{2}} - 5C}{8ee} = 0{\cdot}0003{,}853.$$

$$E = \frac{e^{-\frac{7}{2}}(1+e)^{\frac{3}{2}} - 7D}{10ee} = 0{\cdot}0000{,}434,$$

$$F = \frac{e^{-\frac{9}{2}}(1+e)^{\frac{3}{2}} - 9E}{12ee} = 0{\cdot}0000{,}051,$$

$$G = \frac{e^{-\frac{11}{2}}(1+e)^{\frac{3}{2}} - 11F}{14ee} = 0{\cdot}0000{,}006,$$

&c. &c.

and thence

$$\begin{array}{r|r}
+ & - \\
A = 0{\cdot}6547{,}320 & \frac{1}{2}B = 0{\cdot}0199{,}205 \\
\frac{3}{2.4}C = 0{\cdot}0013{,}750 & \frac{3.5}{2.4.6}D = 0{\cdot}0001{,}204 \\
\frac{3.5.7}{2.4.6.8}E = 0{\cdot}0000{,}119 & \frac{3.5.7.9}{2.4.6.8.10}F = 0{\cdot}0000{,}013 \\
\frac{3.5.7.9.11}{2.4.6.8.10.12}G = 0{\cdot}0000{,}001 & \\
\hline
+\,0{\cdot}6561{,}190 & -\,0{\cdot}0200{,}422 \\
-\,0{\cdot}0200{,}422 & \\
\hline
\end{array}$$

and $z = 0{\cdot}6360{,}768$; which value of z needs no correction.

We must now, in order to find the value of d, write the same values of e and y in Theorem IV, Art. 5, where we shall then have

$$A = \text{H. L.}\ (\surd(1 + e) - \surd e),$$

$$B = \frac{-e\surd\left(\frac{1}{e} + 1\right) - A}{2},$$

$$C = \frac{-ee\surd\left(\frac{1}{e} + 1\right) - 3B}{4},$$

$$D = \frac{-e^3\surd\left(\frac{1}{e} + 1\right) - 5C}{6},$$

$$E = \frac{-e^4\surd\left(\frac{1}{e} + 1\right) - 7D}{8},$$

$$F = \frac{-e^5\surd\left(\frac{1}{e} + 1\right) - 9E}{10},$$

&c. &c.

But, since the terms A, B, C, &c. are to be divided by e, e^3, e^5, &c. respectively, it will be best to divide them by these quantities, before we begin the arithmetical calculations; otherwise much unnecessary labour must be taken. The terms, then, being so divided, and the proper value of e being written for it, viz. $\surd 50$, we shall have as below:

$$\frac{A}{e} = \frac{\text{H. L.}}{e}\left(\surd(1 + e) - \surd e = -\ 0{\cdot}2410{,}905,\right.$$

$$\frac{B}{e^3} = \frac{-\surd\left(\frac{1}{e} + 1\right)}{2ee} - \frac{A}{2e^3} = -\ 0.0082{,}728,$$

$$\frac{C}{e^5} = \frac{-\surd\left(\frac{1}{e} + 1\right)}{4e^3} - \frac{3B}{4e^5} = -\ 0{\cdot}0006{,}314,$$

$$\frac{D}{e^7} = \frac{-\surd\left(\frac{1}{e} + 1\right)}{6e^4} - \frac{5C}{6e^7} = -\ 0{\cdot}0000{,}607,$$

$$\frac{E}{e^9} = \frac{-\surd\left(\frac{1}{e} + 1\right)}{8e^5} - \frac{7D}{8e^9} = -\ 0{\cdot}0000{,}065,$$

$$\frac{F}{e^{11}} = \frac{-\surd\left(\frac{1}{e} + 1\right)}{10e^6} - \frac{9E}{10e^{11}} = -\ 0{\cdot}0000{,}007,$$

&c. &c.

And thence

$$\begin{array}{rl|rl}
 & \qquad + & & \qquad - \\
e\sqrt{\left(\frac{1}{e}+1\right)} & = 7{\cdot}5545,396, & +\frac{A}{2e} & = 0{\cdot}1205,452, \\
-\frac{B}{2.4e^{3}} & = 0{\cdot}0010,341, & +\frac{3C}{2.4.6e^{5}} & = 0{\cdot}0000,395 \\
-\frac{3.5D}{2.4.6.8e^{7}} & = 0{\cdot}0000,024, & +\frac{3\,5.7E}{2.4.6.8.10e^{9}} & = 0{\cdot}0000,002 \\ \cline{2-2}\cline{4-4}
 & +7{\cdot}5555,761 & & -0{\cdot}1205,849 \\
 & -0{\cdot}1205,849 & & \\ \cline{2-2}
\end{array}$$

and $z = 7{\cdot}4349,912 - d$.

But, by the foregoing part of this article, $z = 0{\cdot}6360,768$; we therefore have $d = 7{\cdot}4349,912 - 0{\cdot}6360,768 = 6{\cdot}7989,144$.

22. With the value of a above given, viz. 7, we see a swift convergency, both in the ascending and in the descending series; but, if a were given $= \sqrt{3}$, (which is as small a value of a as need be used in these theorems, for this purpose, because if it were less than, or even $= \sqrt{3}$, the value of d might be computed by one series only, as was observed in Art. 15,) each of the series would converge but slowly, $\frac{1}{e}$, in this case, being $= \frac{1}{2}$; to remedy which, as the terms of each of the series have the signs $+$ and $-$ alternately, it would be expedient to compute a moderate number (from six to ten, as the case shall require,) of the initial terms of each, and then to transform the remainders into other series, which should converge by the powers of $\frac{1}{1+e}$, instead of the powers of $\frac{1}{e}$. This increase of convergency in the geometrical progression, assisted as it would be by the decrease of the coefficients of the new series, would enable us to get a result accurate enough for all common uses, by computing ten (or fewer) terms of each of the new series.

But, as the transformation now mentioned requires but a moderate skill in series, I shall, for the sake of brevity, omit examples of it, and proceed to

SECT. III. *Examples of the Use of the foregoing Theorems.*

23. My intention in this section is, to illustrate the use of the foregoing theorems by a few examples, selecting at the same time such of the theorems as are best adapted to the case in hand; by which, and attention to what was said in the first section, of the limits of the convergency of the several series, I hope the reader will be directed how to make a proper choice of theorems on all other occasions.

EXAMPLE I.

Let there be an hyperbola of which the semi-axes are 40 and 30 respectively, and the ordinate is 10; it is required to find the length of the arch from the vertex of the ordinate.

Since the conjugate semi-axis of this hyperbola is 30, we must, in order to fit the given numbers to our theorems, divide them all by 30; and then we shall have the corresponding dimensions of a similar hyperbola as follows; viz. the transverse semi-axis $= \frac{4}{3}$, the conjugate semi-axis $= 1$, and the ordinate $= \frac{1}{3}$. And the proper theorem to be used in this case is the second.

Writing, then, $\frac{4}{3}$ for a, and $\frac{1}{3}$ for y, in the IId Theorem, we have $ee = aa + 1 = \frac{25}{9}$, and

$$A=\frac{y}{2}\sqrt{(1+eeyy)}+\frac{1}{2e}\,\text{H.L.}\left(ey+\sqrt{(1+eeyy)}\right)=0{\cdot}3497,6260,$$

$$B=\frac{y(1+eeyy)^{\frac{3}{2}}-A}{4ee}=0{\cdot}0134,3234,$$

$$C=\frac{y^{3}(1+eeyy)^{\frac{3}{2}}-3B}{6ee}=0{\cdot}0009,0892,$$

$$D=\frac{y^{5}(1+eeyy)^{\frac{3}{2}}-5C}{8ee}=0{\cdot}0000,7272,$$

$$E=\frac{y^{7}(1+eeyy)^{\frac{3}{2}}-7D}{10ee}=0{\cdot}0000,0632,$$

$$F=\frac{y^{9}(1+eeyy)^{\frac{3}{2}}-9E}{12ee}=0{\cdot}0000,0058,$$

$$G=\frac{y^{11}(1+eeyy)^{\frac{3}{2}}-11F}{14ee}=0{\cdot}0000,0005;$$

and thence

$$+$$

$$A=0{\cdot}3497,6260,$$

$$\frac{3}{2.4}C=0{\cdot}0003,4084,$$

$$\frac{3.5.7}{2.4.6.8}E=0{\cdot}0000,0173,$$

$$\frac{3.5.7.9.11}{2.4.6.8.10.12}G=0{\cdot}0000,0001,$$

$$-$$

$$\tfrac{1}{2}B=0{\cdot}0067,1617,$$

$$\frac{3.5}{2.4.6}D=0{\cdot}0000,2273,$$

$$\frac{3.5.7.9}{2.4.6.8.10}F=0{\cdot}0000,0014,$$

and the sum $= -\,0{\cdot}0067,3904.$

and the sum $= +\,0{\cdot}3501,0518,$

$$-\,0{\cdot}0067,3904,$$

difference of these sums is $\}$ $0{\cdot}3433,6614 = z$, the length of the arch of an hyperbola, from the vertex to the ordinate, of which the transverse and conjugate semi-axes are $\frac{4}{3}$ and 1, and the ordinate $\frac{1}{3}$. And, since like parts of similar hyperbolas are to each other as their semi-axes, we shall have, by multiplying 0·3433,6614 by 30, the semi-conjugate of the hyperbola proposed, 10·3009,842 for the length required.

Having in this example shown how to adapt these theorems to hyperbolas that have a conjugate semi-axis different from 1, it need not be repeated again. I shall therefore, in the remaining examples, show the convergency of these new series in most of the different cases that can occur.

EXAMPLE II.

24. Given $a = 1$, and $y = 1$, to find z.

This arch, it is obvious, may be computed by Theorem IVth, VIth, VIIth, and some others; but the VIIth is the proper one to be chosen on this occasion, as the series there given has the swifter convergency.

Writing, then, 1 for a, and 1 for y, in Theorem VII, we have (by Article 1,) $ee = aa + 1 = 2$, and, (by Art. 7,) $uu = 1 + eeyy = 3$; and then, (by Art. 9,)

$$+$$

$$u = \sqrt{3} = 1{\cdot}7320{,}5081,$$

$$-$$

$$\begin{aligned}
\frac{1}{2.3} u^{-3} &= 0{\cdot}0320{,}7501, \\
\frac{3}{2.4.7} u^{-7} &= 0{\cdot}0011{,}4554, \\
\frac{3.5}{2.4.6.11} u^{-11} &= 0{\cdot}0000{,}6750, \\
\frac{3.5.7}{2.4.6.8.15} u^{-15} &= 0{\cdot}0000{,}0481, \\
\frac{3.5.7.9}{2.4.6.8.10.19} u^{-19} &= 0{\cdot}0000{,}0038, \\
\frac{3.5.7.9.11}{2.4.6.8.10.12.23} u^{-23} &= 0{\cdot}0000{,}0003;
\end{aligned}$$

sum of the neg. terms $- 0{\cdot}0332{,}9327$;

sum of the series $+ 1{\cdot}6987{,}5754$;

correction of the fluent $- 0{\cdot}5990{,}7012 = -d$, (by Art. 17;)

the difference of which is $+ 1{\cdot}0996{,}8742 = z$.

Example III.

25. Given $a = 1$, and $y = \surd(1000000 - 1) = 999{\cdot}9995$ nearly, to find z.

This arch, it is very obvious, may be computed by Theorem IIId, IVth, VIth, and VIIth, the series in each of them converging, in this case, very swiftly. And it may be computed also by the IXth; but the proper Theorem to be used in this case is the VIIth.

Now, since ee is $= 2$, and $y = \surd(1000000 - 1)$, we have (by Article 7,) $u = \surd(2yy + 1) = \surd(2000000 - 1) = \surd 2 \times \surd(1000000 - \frac{1}{2}) = \surd 2 \times (1000 - \frac{1}{4.1000})$ very nearly, $= 1000\surd 2 - \frac{\surd 2}{4000} = 1414{\cdot}2132088$, which may be taken for the value of the whole series, since $\frac{1}{6}u^{-3}$, the second term of it, does not give a 1 in the tenth place of decimals. If, therefore, from $u = 1414{\cdot}2132088$, we subtract $d = 0{\cdot}5990701$, (by Article 17,) we shall have $z = 1413{\cdot}6141387$,* the length required.

Example IV.

26. Let a be given $= 7$, and $y = 10$, to find z.

This Example may be computed by Theorem IIId, IVth, VIth, and some others; the VIth is to be chosen rather than the IIId, and the IVth rather than the VIth.

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* The computation of the value of z, in this example, is the problem alluded to in the Introduction to this Paper, which first turned my thoughts to the subject of it, in the year 1770. In the next year, two answers were given to it, by two persons of good reputation for their skill in mathematics, one of them making $z = 1414{\cdot}2132088$, the other, $z = 1413{\cdot}8921$. These two are the only solutions of this problem that I know of; and, if my calculation be right, both are erroneous.

Now, if 10 be written for y in Theorem IVth, we shall have

$$A = \text{H. L.}\ \frac{\sqrt{(yy+1)}-1}{y} = -0\ 0998,341,$$

$$B = \frac{-\sqrt{(yy+1)}}{2yy} - \frac{A}{2} = -0\cdot0003,323,$$

$$C = \frac{-\sqrt{(yy+1)}}{4y^4} - \frac{3B}{4} = -0\cdot0000,020;$$

of which terms, two only are wanted to obtain a result true to seven places of figures. And then, ee being $= aa + 1 = 50$, we have

$+$	$-$
$e\sqrt{(yy+1)} = 71\cdot0633,520,$	$+\frac{1}{2e}A = 0\cdot0070,593,$
$-\frac{1}{2.4e^3}B = 0\cdot0000,001,$	$-d = 6\cdot7989,144,$ (Art. 21,)
sum of the posit. terms $71\cdot0633,521;$	the sum $-6\cdot8059,737;$
neg. term, and correct. $-6\cdot8059,737;$	

the difference is $+64\cdot2573,748 = z$.

EXAMPLE V.

27. Let a be given $= \frac{1}{2}$, and $y = 10$, to find z.

This example may be computed by Theorem IIId, IVth, Vth, VIth, VIIIth, and IXth; of which the IVth, Vth, and IXth, are more eligible than the other three. I make choice of the fourth, on account of the facility of the computation by it, with the present value of y.

Now, by writing 10 for y in Theorem IV, we shall have (as in the preceding example,)

$$A = \text{H. L.}\ \frac{\sqrt{(yy+1)}-1}{y} = -0\cdot0998,341,$$

$$B = \frac{-\sqrt{(yy+1)}}{2yy} - \frac{A}{2} = -0\cdot0003,323,$$

$$C = \frac{-\sqrt{(yy+1)}}{4y^4} - \frac{3B}{4} = -0\cdot0000,020;$$

and then, ee being $= aa + 1 = \frac{5}{4}$, we have

$+$	$-$
$e\sqrt{(yy+1)} = 11{\cdot}2361,025,$	$\frac{A}{2e} = 0{\cdot}0446,472,$
$-\frac{1}{2\,4e^3}B = 0{\cdot}0000,297,$	$\frac{3}{2\cdot 46e^5}C = 0{\cdot}0000,001,$
sum of affirm. terms $+\ 11{\cdot}2361,322;$	$-d = 0{\cdot}1803,793,$ (Art. 15,)
neg. terms and corr. $-\ 0{\cdot}2250,266;$	the sum is $-\ 0{\cdot}2250,266.$
the difference is $= 11{\cdot}0111,056,$	

which is $= z$.

28. Having now produced series, of good convergency, for computing the length of the arch from the vertex to the ordinate, (and consequently any portion of such an arch,) of any conical hyperbola, I shall conclude this Paper with a few remarks: reserving some other theorems which I have discovered for the purpose, till I shall have found an opportunity to describe nearly an equal number of theorems, which I have long had by me, for the Rectification of the Ellipsis.

The utility of hyperbolic and elliptic arches, in the solution of various problems, and particularly in the business of computing fluents, has been shown by those eminent mathematicians, McLAURIN, SIMPSON, and LANDEN; the last of whom hath written a very ingenious paper on hyperbolic and elliptic arches, which was published in the Ist volume of his *Mathematical Memoirs,* in the year 1780. I have indeed heard, that some improvement in the rectification of the ellipsis and hyperbola had been produced, and some of the same theorems discovered, by a learned Italian, many years before Mr. LANDEN's *Mathematical Memoirs* were published; but, as Mr. LANDEN has declared that he had never seen nor heard any thing of that work, and as various instances are to be found of different men discovering the same truth, without any knowledge of each other's works, I see no reason for disbelieving him. But I have seen no writings on this subject which contain any thing more than what is very common, besides those of the three gentlemen above mentioned, and Dr. WARING's *Meditationes Analyticæ;* and, while I have no inclination to detract from their merits, I may be allowed to say that I have borrowed nothing from their works.

29. With respect to Dr. WARING, (who was well known to be a profound mathematician, and I can testify that he was a good-natured man,) he has given, in page 470 of his *Meditationes Analyticæ*, (published in 1776,) these two series, as expressions of the length of an arch of an equilateral hyperbola; viz.

" Arcus hyperbolicus exprimi possit per seriem $-\frac{1}{x}+\frac{1}{2\times3}x^{3}$
" $-\frac{1}{2^{2}.2\times7}x^{7}+\frac{1.3}{2^{3}.2.3\times11}x^{11}-\frac{1.3.5}{2^{4}.2.3.4\times15}x^{15}+\frac{1.3\,5.7}{2^{5}.2.3.4.5\times19}x^{19}$,
" &c. ubi x denotat abscissam ad asymptoton."

" Si vero requiratur descendens series, tum erit $x-\frac{1}{2\times3}x^{-3}$
" $+\frac{1}{2^{2}.2\times7}x^{-7}-\frac{3^{*}}{2^{3}.2.3\times11}x^{-11}$, &c. quæ, quoad coefficientes
" attinet, prorsus eandem observat legem ac præcedens."

30. These series, as they now stand, are of little use. But, if proper corrections were applied to them, (which may easily be done from what has been shewn in this Paper, and in my *Mathematical Essays,)* and the first of them were transformed into another series converging by the powers of $\frac{x^{4}}{1+x^{4}}$, they would become very useful for computing any arch of an equilateral

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[*] In the original, this term is erroneously printed, there being a 1 in the numerator, instead of a 3.

hyperbola, when the abscissa is taken on the asymptote. This I thought it might be proper to remark, that the less experienced readers of this Paper might not be misled by so great an authority as that of Dr. WARING. Whether or not he ever corrected these oversights in any of his subsequent publications, I cannot ascertain, for want of books.

XVIII. *Catalogue of 500 new Nebulæ, nebulous Stars, planetary Nebulæ, and Clusters of Stars; with Remarks on the Construction of the Heavens. By* William Herschel, *LL. D. F. R. S.*

Read July 1, 1802.

SINCE the publication of my former two catalogues of nebulæ, I have, in the continuation of my telescopic sweeps, met with a number of objects that will enrich our natural history, as it may be called, of the heavens. A catalogue of them will be found at the end of this paper, containing 500 new nebulæ, nebulous stars, planetary nebulæ, and clusters of stars. These objects have been arranged in eight classes, in conformity with the former catalogues, of which the present one is therefore a regular continuation. This renders it unnecessary to give any further explanation, either of the contents of its columns, or the abbreviations which have been used in the description of the objects.

It has hitherto been the chief employment of the physical astronomer, to search for new celestial objects, whatsoever might be their nature or condition; but our stock of materials is now so increased, that we should begin to arrange them more scientifically. The classification adopted in my catalogues, is little more than an arrangement of the objects for the convenience of the observer, and may be compared to the disposition of the books in a library, where the different sizes of the volumes is often more considered than their contents. But here, in dividing the different parts of which the sidereal heavens are composed into proper classes, I shall have to examine the nature of the various celestial objects that have been hitherto discovered, in order to arrange them in a manner most conformable to their construction. This will bring on some extensive considerations, which would be too long for the compass of a single paper; I shall therefore now only give an enumeration of the species that offer themselves already to our view, and leave a particular examination of the separate divisions, for some early future occasions.

In proceeding from the most simple to the more complex arrangements, several methods, taken from the known laws of gravitation, will be suggested, by which the various systems under consideration may be maintained; but here also we shall confine ourselves to a general review of the subject, as observation must furnish us first with the necessary data, to establish the application of any one of these methods on a proper foundation.

ENUMERATION OF THE PARTS THAT ENTER INTO THE CONSTRUCTION OF THE HEAVENS.

I. *Of insulated Stars.*

In beginning our proposed enumeration, it might be expected that the solar system would stand foremost in the list; whereas, by treating of insulated stars, we seem, as it were, to overlook one of the great component parts of the universe. It will, however, soon appear that this very system, magnificent as it is, can only rank as a single individual belonging to the species which we are going to consider.

By calling a star insulated, I do not mean to denote its being totally unconnected with all other stars or systems; for no one, by the laws of gravitation, can be intirely free from the influence of other celestial bodies. But, when stars are situated at such immense distances from each other as our sun, Arcturus, Capella, Lyra, Sirius, Canobus, Markab, Bellatrix, Menkar, Shedir, Algorah, Propus, and numberless others probably are, we may then look upon them as sufficiently out of the reach of mutual attractions, to deserve the name of insulated stars.

In order not to take this assertion for granted, without some examination, let us admit, as is highly probable, that the whole orbit of the earth's annual motion does not subtend more than an angle of one second of a degree, when seen from Sirius. In consequence of this, it appears by computation, that our sun and Sirius, if we suppose their masses to be equal, would not fall together in less than 33 millions of years, even though they were not impeded by many contrary attractions of other neighbouring insulated stars; and that, consequently, with the assistance of the opposite energies exerted by such surrounding stars, these two bodies may remain for millions of ages, in a state almost equal to undisturbed rest. A star thus situated may certainly deserve to be called insulated, since it does not immediately enter into connection with any neighbouring star; and it is therefore highly probable, that our sun is one of a great number that are in similar circumstances. To this may be added, that the stars we consider as insulated are also surrounded by a magnificent collection of innumerable stars, called the milky-way, which must occasion a very powerful balance of opposite attractions, to hold the intermediate stars in a state of rest. For, though our sun, and all the stars we see, may truly be said to be in the plane of the milky-way, yet I am now convinced, by a long inspection and continued examination of it, that the milky-way itself consists of stars very differently scattered from those which are immediately about us. But of this, more will be said on another occasion.

From the detached situation of insulated stars, it appears that they are capable of being the centres of extensive planetary systems. Of this we have a convincing proof in our sun, which, according to our classification, is one of these stars. Now, as we enjoy the advantage of being able to view the solar system in all its parts, by means of our telescopes, and are therefore sufficiently acquainted with it, there will be no occasion to enter into a detail of its construction.

The question will now arise, whether every insulated star be a sun like ours, attended with planets, satellites, and numerous comets? And here, as nothing appears against the supposition, we may from analogy admit the probability of it. But, were we to extend this argument to other sidereal constructions, or, still farther, to every star of the heavens, as has been done frequently, I should not only hesitate, but even think that, from what will be said of stars which enter into complicated sidereal systems, the contrary is far more likely to be the case; and that, probably, we can only look for solar systems among insulated stars.

II. *Of Binary sidereal Systems, or double Stars.*

The next part in the construction of the heavens, that offers itself to our consideration, is the union of two stars, that are formed together into one system, by the laws of attraction.

If a certain star should be situated at any, perhaps immense, distance behind another, and but very little deviating from the line in which we see the first, we should then have the appearance of a double star. But these stars, being totally unconnected, would not form a binary system. If, on the contrary, two stars should really be situated very near each other, and at the same time so far insulated as not to be materially affected by the attractions of neighbouring stars, they will then compose a separate system, and remain united by the bond of their own mutual gravitation towards each other. This should be called a real double star; and any two stars that are thus mutually connected, form the binary sidereal system which we are now to consider.

It is easy to prove, from the doctrine of gravitation, that two stars may be so connected together as to perform circles, or similar ellipses, round their common centre of gravity. In this case, they will always move in directions opposite and parallel to each other; and their system, if not destroyed by some foreign cause, will remain permanent.

Figure 1 (Plate XVI.) represents two equal stars *a* and *b*, moving in one common circular orbit round the centre *o*, but in the opposite directions of *at* and *bt*. In Fig. 2 we have a similar connection of the two stars *a b;* but, as they are of different magnitudes, or contain unequal quantities of matter, they will move in circular orbits of different dimensions round their common centre of gravity *o*. Fig. 3 represents equal, and Fig. 4 unequal stars, moving in similar elliptical orbits round a common centre; and, in all these cases, the directions of the tangents *t t*, in the places *a b*, where the stars are, will be opposite and parallel, as will be more fully explained hereafter.

These four orbits, simple as they are, open an extensive field for reflection, and, I may add, for calculation. They shew, even before we come to more complicated combinations, where the same will be confirmed, that there is an essential difference between the construction of solar and sidereal systems. In each solar system, we have a very ponderous attractive centre, by which all the planets, satellites, and comets are governed, and kept in their orbits. Sidereal systems take a greater scope: the stars of which they are composed move round an empty centre, to which they are nevertheless as firmly bound as the planets to their massy one. It is however not necessary here to enlarge on distinctions which will hereafter be strongly supported by facts, when clusters of stars come to be considered. I shall only add, that in the subordinate bodies of the solar system itself, we have already instances, in miniature, as it may be called, of the principle whereby the laws of attraction are applicable to the solution of the most complicated phenomena of the heavens, by means of revolutions round empty centres. For, although both the earth and its moon are retained in their orbits by the sun, yet their mutual subordinate system is such, that they perform secondary monthly revolutions round a centre without a body placed in it. The same indeed, though under very narrow limits, may be said of the sun and each planet itself.

That no insulated stars, of nearly an equal size and distance, can appear double to us, may be proved thus. Let Arcturus and Lyra be the stars: these, by the rule of insulation, which we must now suppose can only take place when their distance from each other is not less than that of Sirius from us, if very accurately placed, would be seen under an angle of 60 degrees from each other. They really are at about 59°. Now, in order to make these stars appear to us near enough to come under the denomination of a double star of the first class, we should remove the earth from them at least 41253 times farther than Sirius is from us. But the space-penetrating power of a 7-feet reflector, by which my observations on double stars have been made, cannot intitle us to see stars at such an immense distance; for, even the 40-feet telescope, as has been shewn,* can only reach stars of the 1342d magnitude. It follows, therefore, that these stars could not remain visible in a 7-feet reflector, if they were so far removed as to make their angular distance less than about $24\frac{1}{4}$ minutes; nor could even the 40-feet telescope, under the same circumstances of removal, shew them, unless they were to be seen at least $2\frac{1}{2}$ minutes asunder. Moreover, this calculation is made on a supposition that the stars of which a double star is composed, might be as small as any that can possibly be perceived; but if, on the contrary, they should still appear of a considerable size, it will then be so much the more evident that such stars cannot have any great real distance, and that, consequently, insulated stars cannot appear double, if they are situated at equal distances from us. If, however, their arrangement should be such as has been mentioned before, then, one of them being far behind the other, an apparent double star may certainly be produced; but here the appearance of proximity would be deceptive; and the object so circumstanced could not be classed in the list of binary systems. However, as we must grant, that in particular situations stars apparently double may be composed of such as are insulated, it cannot be improper to consult calculation, in order to see whether it be likely that the 700 double stars I have given in two catalogues, as well as

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* See Phil. Trans. for 1800, Part I. page 83.

many more I have since collected, should be of that kind. Such an inquiry, though not very material to our present purpose, will hereafter be of use to us, when we come to consider more complicated systems. For, if it can be shown that the odds are very much against the casual production of double stars, the same argument will be still more forcible, when applied to treble, quadruple, or multiple compositions.

Let us take ζ Aquarii, for an instance of computation. This star is admitted, by FLAMSTEED, DE LA CAILLE, BRADLEY, and MAYER, to be of the 4th magnitude. The two stars that compose it being equal in brightness, each of them may be supposed to shine with half the light of the whole lustre. This, according to our way of reckoning magnitudes,* would make them $4m \times \surd 2 = 5\tfrac{2}{3}m$; that is, of between the 6th and 5th magnitude each. Now, the light we receive from a star being as the square of its diameter directly, and as the square of its distance inversely, if one of the stars of ζ Aquarii be farther off than the stars of between the 6th and 5th magnitude are from us, it must be so much larger in diameter, in order to give us an equal quantity of light. Let it be at the distance of the stars of the 7th magnitude; then its diameter will be to the diameter of the star which is nearest to us as 7 to $5\tfrac{2}{3}$, and its bulk as 1,885 to 1; which is almost double that of the nearest star. Then, putting the number of stars we call of between the 6th and 5th magnitude at 450, we shall have 686 of the 7th magnitude to combine with them, so that they may make up a double star of the first class, that is to say, that the two stars may not be more than 5″ asunder. The surface of the globe contains

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* The expressions 2m, 3m, 4m, &c. stand for stars at the distance of 2, 3, 4, &c. times that of Sirius, supposed unity.

34036131547 circular spaces, each of 5″ in diameter; so that each of the 686 stars will have 49615357 of these circles in which it might be placed; but, of all that number, a single one would only be the proper situation in which it could make up a double star with one of the 450 given stars. But these odds, which are above 75½ millions to one against the composition of ζ Aquarii, are extremely increased by our foregoing calculation of the required size of the star, which must contain nearly double the mass allotted to other stars of the 7th magnitude; of which, therefore, none but this one can be proper for making up the required double star. If the stars of the 8th and 9th magnitudes, of which there will be 896 and 1134, should be taken in, by way of increasing the chance in favour of the supposed composition of our double star, the advantage intended to be obtained by the addition of numbers, will be completely counteracted by the requisite uncommon bulk of the star which is to serve the purpose; for, one of the 8th magnitude, ought to be more than 2¾ times bigger than the rest; and, if the composition were made by a star of the 9th magnitude, no less than four times the bulk of the other star which is to enter the composition of the double star would answer the purpose of its required brightness. Hence therefore it is evident, that casual situations will not account for the multiplied phenomena of double stars, and that consequently their existence must be owing to the influence of some general law of nature; now, as the mutual gravitation of bodies towards each other is quite sufficient to account for the union of two stars, we are authorised to ascribe such combinations to that principle.

It will not be necessary to insist any further on arguments drawn from calculation, as I shall soon communicate a series of observations made on double stars, whereby it will be seen, that *many of them have actually changed their situation with regard to each other, in a progressive course, denoting a periodical revolution round each other; and that the motion of some of them is direct, while that of others is retrograde.* Should these observations be found sufficiently conclusive, we may already have their periodical times near enough to calculate, within a certain degree of approximation, the parallax and mutual distance of the stars which compose these systems, by measuring their orbits, which subtend a visible angle.

Before we leave the subject of binary systems, I should remark, that it evidently appears, that our sun does not enter into a combination with any other star, so as to form one of these systems with it. This could not take place without our immediately perceiving it; and, though we may have good reason to believe that our system is not perfectly at rest, yet the causes of its proper motion are more probably to be ascribed to some perturbations arising from the proper motion of neighbouring stars or systems, than to be placed to the account of a periodical revolution round some imaginary distant centre.

III. *Of more complicated sidereal Systems, or treble, quadruple, quintuple, and multiple Stars.*

Those who have admitted our arguments for the existence of real double stars, will easily advance a step farther, and allow that three stars may be connected in one mutual system of reciprocal attraction. And, as we have from theory pointed out, in figures 1, 2, 3, and 4, how two stars may be maintained in a binary system, we shall here shew that three stars may likewise be preserved in a permanent connection, by revolving in proper orbits about a common centre of motion.

In all cases where stars are supposed to move round an empty centre, in equal periodical times, it may be proved that an imaginary attractive force may be supposed to be lodged in that centre, which increases in a direct ratio of the distances. For since, in different circles, by the law of centripetal forces, the squares of the periodical times are as the radii divided by the central attractive forces, it follows, that when these periodical times are equal, the forces will be as the radii. Hence we conclude, that in any system of bodies, where the attractive forces of all the rest upon any one of them, when reduced to a direction as coming from the empty centre, can be shewn to be in a direct ratio of the distance of that body from the centre, the system may revolve together without perturbation, and remain permanently connected without a central body.

Hence may be proved, as has been mentioned before, that two stars will move round a hypothetical centre of attraction. For, let it be supposed that the empty centre o, in Fig. 1 and 3, is possessed of an attractive force, increasing in the direct ratio of the distances $oa : ob$. Then, since here ao and bo are equal, the hypothetical attractions will be equal, and the bodies will revolve in equal times. That this agrees with the general law of attraction, is proved thus. The real attraction of b upon a is $\frac{b}{ab^2}$; and that of a upon b is $\frac{a}{ab^2}$; and, since $b = a$, it will be $\frac{b}{ab^2} : \frac{a}{ab^2} :: ao : bo$; which was required.

In Figures 2 and 4, when the stars a and b are unequal, and their distances from o also unequal, let $oa = n$, and $ob = m$; and let the mass of matter in $a = m$, and in $b = n$. Then the attraction of b on $a = \frac{b}{ab^2}$, will be to the attraction of a on $b = \frac{a}{ab^2}$, as $n : m$; which is again directly as $ao : bo$.

I proceed now to explain a combination of three bodies, moving round a centre of hypothetical attraction. Fig. 5 contains a single orbit, wherein three equal bodies $a\ b\ c$, placed at equal distances, may revolve permanently. For, the real attraction of b on a will be expressed by $\frac{a}{ab^2}$; but this, reduced to the direction ao, will be only $\frac{b \,.\, by}{ab^3}$; for, the attraction in the direction ba is to that in the direction by, parallel to ao, as $\frac{b}{ab^2}$ to $\frac{b \,.\, by}{ab^3}$. The attraction also of c on a is equal to that of b on a; therefore the whole attraction on a, in a direction towards o, will be expressed by $\frac{2b \,.\, by}{ab^3}$. In the same manner we prove, that the attraction of a and c on b, in the direction bo, is $\frac{2a \,.\, by}{ab^3}$; and that of a and b on c, in the direction co, is $\frac{2c \,.\, by}{ab^3}$. Hence, $a\ b$ and c being equal, the attractions in the directions ao bo and co will also be equal; and, consequently, in the direct ratio of these distances. Or rather, the hypothetical attractions being equal, it proves that, in order to revolve permanently, $a\ b$ and c must be equal to each other.

Instead of moving in one circular orbit, the three stars may revolve in three equal ellipses, round their common centre of gravity, as in Fig. 6. And here we should remark, that this centre of gravity will be situated in the common focus o, of the three ellipses; and that the absolute attraction towards that focus, will vary in the inverse ratio of the squares of the distances of any one of the stars from that centre, while the relative attractions remain in the direct ratio of their several distances from the same centre. This will be more fully explained, when we come to consider the motion of four stars.

A very singular straight-lined orbit, if so it may be called, may also exist in the following manner. If *a* and *b*, Fig. 7, are two large equal stars, which are connected together by their mutual gravitation towards each other, and have such projectile motions as would cause them to move in a circular orbit about their common centre of gravity, then may a third small star *c*, situated in a line drawn through *o*, and at rectangles to the plane described by the stars *a b*, fall freely from rest, with a gradually acquired motion to *o*; then, passing through the plane of the orbit of the two stars, it will proceed, but with a gradually retarded motion, to a second point of rest *d*; and, in this manner, the star *c* may continue to oscillate between *c* and *d*, in a straight line, passing from *c*, through the centre *o*, to *d*, and back again to *c*.

In order to see the possibility and permanency of this connection the better, let *o* be the centre of gravity of the three bodies, when the oscillating body is at *c*; then, supposing the bodies *a* and *b* to be at that moment in the plane *p l*, and admitting *m* to represent a body equal in mass to the two bodies *a b*, *o* will be the common centre of gravity of *m* and *c*. Then, by the force of attraction, the body *c* and the fictitious body *m* will meet in *o*; that is to say, the plane *p l*, of the bodies *a b*, will now be at *p′ l′*. The fictitious body *m* may then be conceived to move on till it comes to *n*, while the body *c* goes to *d*; or, which is the same, the plane of the bodies *a b* will now be in the position *p″ l″*, as much beyond the centre of gravity *o*, as it was on the opposite side *m*. By this time, both the fictitious body *m*, now at *n*, and the real body *c*, now at *d*, have lost their motion in opposite directions, and begin to approach to their common centre of gravity *o*, in which they will meet a second time. It is evident that the orbit of the two large stars will suffer considerable perturbations, not only in its plane, but also in its curvature, which will not remain strictly circular; the construction of the system, however, is such as to contain a sufficient compensation for every disturbing force, and will consequently be in its nature permanent.

In order to add an oscillating star, it is not necessary that the two large stars should be so situated as to move in a circular orbit, without the oscillating star. In Fig. 8, the stars *a* and *b* may have such projectile forces given them as would cause them to describe equal ellipses, of any degree of excentricity. If now the small star *c* be added, the perturbations will undoubtedly affect not only the plane of the orbits of the stars, but also their figures, which will become irregular moveable ovals. The extent also of the oscillations of the star *c* will be affected; and will sometimes exceed the limits *c d*, and sometimes fall short of them. All these varieties may easily be deduced from what has been already said, when Fig. 7 was considered. It is however very evident, that this system also must be permanent; since not only the centre of gravity *o* will always be at rest, but *a o*, whatever may be the perturbations arising from the situation of *c*, will still remain equal to *b o*.

It should be remarked, that the vibratory motion of the star *c* will differ much from a cometary orbit, even though the latter should be compressed into an evanescent ellipsis. For, while the former extends itself over the diameter of a globe in which it may be supposed to be inscribed, the hypothetical attractive force being supposed to be placed in its centre, the cometary orbit will only describe a radius of the same globe, on account of its requiring a solid attractive centre.

After what has been said, it will hardly be necessary to add, that with the assistance of any proper one of the combinations pointed out in the four last figures, the appearance of every treble star may be completely explained; especially when the different inclinations of the orbits of the stars, to the line of sight, are taken into consideration.

If we admit of treble stars, we can have no reason to oppose more complicated connections; and, in order to form an idea how the laws of gravitation may easily support such systems, I have joined some additional delineations. A very short explanation of them will be sufficient.

Fig. 9 (Plate XVII.) represents four stars, *a b c* and *d*, arranged in a line; *a* being equal to *b*, and *c* equal to *d*. Then, if $ao = bo$, and $co = do$, the centre of gravity will be in *o*; and, with a proper adjustment of projectile forces, the four stars will revolve in two circular orbits round their common centre. By calculating in the manner already pointed out, it will be found, that when, for instance, $ao = 1$, $co = 3$, and $c = d = 1$, then the mass of matter in $a = b$, will be required to be equal to 1,3492.

It is not necessary that the projectile force of the four stars should be such as will occasion them to revolve in circles. The system will be equally permanent when they describe similar ellipses about the common centre of gravity, which will also be the common focus of the four ellipses. In Fig. 10, the stars *a b c d*, revolving in ellipses that are similar, will always describe, at the same time, equal angles in each ellipsis about the centre of hypothetical attraction; and, when they are removed from *a b c d* to *a′ b′ c′ d′*, they will still be situated in a straight line, and at the same proportionate distances from each other as before. By this it appears, as we have already observed, that the absolute hypothetical force in the situation *a′ b′ c′ d′*, compared to what it was when the stars were at *a b c d*, is inversely as the squares of the distances; but that its comparative exertion on the stars, in their present situation, is still in a direct ratio of their distances from the centre *o*, just as it was when they were at *a b c d;* or, to express the same perhaps more clearly, the force exerted on *a′*, is to that which was exerted on *a* as $\frac{1}{\overline{a'o}|^{2}} : \frac{1}{\overline{ao}|^{2}}$. But the force exerted on *a* is to that exerted on *c*, in our present instance, as $ao = 1$ to $co = 3$; and still remains in the same ratio when the stars are at *a′* and *c′;* for the exertion will here be likewise as $a'o = 1$ to $c'o = 3$.

Fig. 11 represents four stars in one circular orbit; and its calculation is so simple, that, after what has been said of Fig. 5, I need only remark that the stars may be of any size, provided their masses of matter are equal to each other.

It is also evident, that the projectile motion of four equal stars is not confined to that particular adjustment which will make them revolve in a circle. It will be sufficient, in order to produce a permanent system, if the stars *a b c d*, in Fig. 12, are impressed with such projectile forces as will make them describe equal ellipses round the common centre *o*. And, as the same method of calculation which has been explained with Figs. 6 and 10 may here be used, it will not be necessary to enter into particulars.

Fig. 13 represents four stars, placed so that, with properly adjusted projectile forces, they may revolve in equal times, and in two different circles, round their common centre of gravity *o*.

If $ao = bo = 4$, $co = do = 5$, and $c = d = 1$, then will the mass of matter in $a = b$, required for the purpose, be 1,5136. This arrangement, remarkable as it may appear, cannot be made in all situations; for instance, if the distance $ao = bo$ were assumed equal to 1, that of $co = do$ being 2, it would be impossible to find such quantities of matter in *a* and *b* as would unite the four stars into one system.

As we have shewn how the arrangement in Fig. 10 may be derived from that of Fig. 9, so it will equally appear, that four stars may revolve in different but similar ellipses round their common centre, as in Fig. 14. For here the four stars, when placed at *a b c d*, are exactly in the situation represented in Fig. 13; but, on account of different projectile forces, they revolve, not as before in concentric circles, but in similar elliptical orbits.

Fig. 15 represents three stars, *a b c*, in the situation of Fig. 5, to which a small oscillating star, *d*, is added. The addition of such a star to Fig. 1, has been sufficiently explained in Fig. 7; and, what has been remarked there, may easily be applied to our present figure. As the fictitious body *m*, in Fig. 7, was made to represent the stars *a* and *b*, it will now stand for the three stars *a b* and *c*. If we suppose these stars to be of an equal magnitude in both figures, the centre of gravity *o*, of the three stars, will not be so far from *m* and *n* as in Fig. 7; and the perturbations will be proportionally lessened.

Fig. 16 gives the situation of three stars, *a b c*, moving in equal elliptical orbits about their common focus *o*, while the star *d* performs oscillations between *d* and *e*. What has been said in explaining Fig. 8, will be sufficient to shew, that the present arrangement is equally to be admitted among the constructions of sidereal systems that may be permanent.

We have before remarked, that any appearance of treble stars might be explained, by admitting the combinations pointed out in Figs. 5, 6, 7, and 8; and it must be equally obvious, that quadruple systems, under what shape soever they may show themselves, whether in straight lines, squares, trapezia, or any other seemingly the most irregular configurations, will readily find a solution from one or other of the arrangements of the eight last figures.

More numerous combinations of stars may still take place, by admitting simple and regular perturbations; for then all sorts of erratic orbits of multiple flexures may have a permanent existence. But, as it would lead me too far, to apply calculation to them, I forbear entering upon the subject at present.

Before I proceed, it will be proper to remark, that it may possibly occur to many, who are not much acquainted with the arrangement of the numberless stars of the heavens, that what has been said may all be mere useless surmise; and that, possibly, there may not be the least occasion for any such speculations upon the subject. To this, however, it may be answered, that such combinations as I have mentioned, are not the inventions of fancy: they have an actual existence; and, were it necessary, I could point them out by thousands. There is not a single night when, in passing over the zones of the heavens by sweeping, I do not meet with numerous collections of double, treble, quadruple, quintuple, and multiple stars, apparently insulated from other groups, and probably joined in some small sidereal system of their own. I do not imagine that I have pointed out the actual manner in which they are held together; but it will always be a desirable step towards information, if the possibility of such unions, in many different ways, can be laid before us; and, very probably, those who have more leisure to consider the different combinations of central forces, than a practical astronomer can have, may easily enlarge on what has been laid down in the foregoing paragraphs.

IV. *Of clustering Stars, and the Milky-way.*

From quadruple, quintuple, and multiple stars, we are naturally led to a consideration of the vast collections of small stars that are profusely scattered over the milky-way. On a very slight examination, it will appear that this immense starry aggregation is by no means uniform. The stars of which it is composed are very unequally scattered, and show evident marks of clustering together into many separate allotments. By referring to some one of these clustering collections in the heavens, what will be said of them will be much better understood, than if we were to treat of them merely in a general way. Let us take the space between β and γ Cygni for an example, in which the stars are clustering with a kind of division between them, so that we may suppose them to be clustering towards two different regions. By a computation, founded on observations which ascertain the number of stars in different fields of view, it appears that our space between β and γ, taking an average breadth of about five degrees of it, contains more than 331 thousand stars; and, admitting them to be clustering two different ways, we have 165 thousand for each clustering collection. Now, as a more particular account of the milky-way will be the subject of a separate paper, I shall only observe, that the above mentioned milky appearances deserve the name of clustering collections, as they are certainly brighter about the middle, and fainter near their undefined borders. For, in my sweeps of the heavens, it has been fully ascertained, that the brightness of the milky-way arises only from stars; and that their compression increases in proportion to the brightness of the milky-way.

We may indeed partly ascribe the increase, both of brightness and of apparent compression, to a greater depth of the space which contains these stars; but this will equally tend to shew their clustering condition: for, since the increase of brightness is gradual, the space containing the clustering stars must tend to a spherical form, if the gradual increase of brightness is to be explained by the situation of the stars.

V. *Of Groups of Stars.*

From clustering stars there is but a short transition to groups of stars; they are, however, sufficiently distinct to deserve a separate notice. A group is a collection of closely, and almost equally compressed stars, of any figure or outline; it contains no particular condensation that might point out the seat of an hypothetical central force; and is sufficiently separated from neighbouring stars to shew that it makes a peculiar system of its own. It must be remembered, that its being a separate system does not exclude it from the action or influence of other systems. We are to understand this with the same reserve that has been pointed out, when we explained what we called insulated stars.

The construction of groups of stars is perhaps, of all the objects in the heavens, the most difficult to explain; much less can we now enter into a detail of the numerous observations I have already made upon this subject. I therefore proceed in my enumeration.

VI. *Of Clusters of Stars.*

These are certainly the most magnificent objects that can be seen in the heavens. They are totally different from mere groups of stars, in their beautiful and artificial arrangement: their form is generally round; and the compression of the stars shews a gradual, and pretty sudden accumulation towards the centre, where, aided by the depth of the cluster, which we can have no doubt is of a globular form, the condensation is such, that the stars are sufficiently compressed to produce a mottled lustre, nearly amounting to the semblance of a nucleus. A centre of attraction is so strongly indicated, by all the circumstances of the appearance of the cluster, that we cannot doubt a single moment of its existence, either in a state of real solidity, or in that of an empty centre, possessed of an hypothetical force, arising from the joint exertion of the numerous stars that enter into the composition of the cluster.

The number of observations I have to give relating to this article, in which my telescopes, especially those of high space-penetrating power, have been of the greatest service, of course can find no room in this enumeration.

VII. *Of Nebulæ.*

These curious objects, which, on account of their great distance, can only be seen by instruments of great space-penetrating power, are perhaps all to be resolved into the three last mentioned species. Clustering collections of stars, for instance, may easily be supposed sufficiently removed to present us with the appearance of a nebula of any shape, which, like the real object of which it is the miniature, will seem to be gradually brighter in the middle. Groups of stars also may, by distance, assume the semblance of nebulous patches; and real clusters of stars, for the same reason, when their composition is beyond the reach of our most powerful instruments to resolve them, will appear like round nebulæ that are gradually much brighter in the middle. On this occasion I must remark, that with instruments of high space-penetrating powers, such as my 40-feet telescope, nebulæ are the objects that may be perceived at the greatest distance. Clustering collections of stars, much less than those we have mentioned before, may easily contain 50000 of them; and, as that number has been chosen for an instance of calculating the distance at which one of the most remote objects might be still visible,* I shall take notice of an evident consequence attending the result of the computation; which is, that a telescope with a power of penetrating into space, like my 40-feet one, has also, as it may be called, a power of penetrating into time past. To explain this, we must consider that, from the known velocity of light, it may be proved, that when we look at Sirius, the rays which enter the eye cannot have been less than 6 years and 4½ months coming from that star to the observer. Hence it follows, that when we see an object of the calculated distance at which one of these very remote nebulæ may still be perceived, the rays of light which convey its image to the eye, must have been more than nineteen hundred and ten thousand, that is, almost two millions of years on their way; and that, consequently, so many years ago,

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* See Phil. Trans. for 1800, page 83. N. B. In the same page, line 22, for 5000 read 50000.

this object must already have had an existence in the sidereal heavens, in order to send out those rays by which we now perceive it.

VIII. *Of Stars with Burs, or Stellar Nebulæ.*

Situated as we are, at an immense distance from the remote parts of the heavens, it is not in the power of telescopes to resolve many phenomena we can but just perceive, which, could we have a nearer view of them, might probably shew themselves as objects that have long been known to us. A stellar nebula, perhaps, may be a real cluster of stars, the whole light of which is gathered so nearly into one point, as to leave but just enough of the light of the cluster visible to produce the appearance of burs. This, however, admits of a doubt.

IX. *Of milky Nebulosity.*

The phenomenon of milky nebulosity is certainly of a most interesting nature: it is probably of two different kinds; one of them being deceptive, namely, such as arises from widely extended regions of closely connected clustering stars, contiguous to each other, like the collections that construct our milky-way. The other, on the contrary, being real, and possibly at no very great distance from us. The changes I have observed in the great milky nebulosity of Orion, 23 years ago, and which have also been noticed by other astronomers, cannot permit us to look upon this phenomenon as arising from immensely distant regions of fixed stars. Even HUYGENS, the discoverer of it, was already of opinion that, in viewing it, we saw, as it were, through an opening into a region of light.* Much more would

* See *Systema Saturnium,* page 8 and 9.

he be convinced now, when changes in its shape and lustre have been seen, that its light is not, like that of the milky-way, composed of stars. To attempt even a guess at what this light may be, would be presumptuous. If it should be surmised, for instance, that this nebulosity is of the nature of the zodiacal light, we should then be obliged to admit the existence of an effect without its cause. An idea of its phosphorical condition, is not more philosophical, unless we could shew from what source of phosphorical matter, such immeasurable tracts of luminous phenomena could draw their existence, and permanency; for, though minute changes have been observed, yet a general resemblance, allowing for the difference of telescopes, is still to be perceived in the great nebulosity of Orion, even since the time of its first discovery.

X. *Of nebulous Stars.*

The nature of these remarkable objects is enveloped in much obscurity. It will probably require ages of observations, before we can be enabled to form a proper estimate of their condition. That stars should have visible atmospheres, of such an extent as those of which I have given the situation in this and my former catalogues, is truly surprising, unless we attribute to such atmospheres, the quality of self-luminous milky nebulosity. We can have no reason to doubt of the starry nature of the central point; for, in no respect whatever does its appearance differ from that of a star of an equal magnitude; but, when the great distance of such stars is taken into consideration, the real extent of the surrounding nebulosity is truly wonderful. A very curious one of this kind will be found in the 4th class, No. 69, of the annexed catalogue.

XI. *Planetary Nebulæ.*

This seems to be a species of bodies that demands a particular attention. To investigate the planetary nature of these nebulæ, is not an easy undertaking. If we admit them to contain a great mass of matter, such as that of which our sun is composed, and that they are, like the sun, surrounded by dense luminous clouds, it appears evidently that the intrinsic brightness of these clouds must be far inferior to those of the sun. A part of the sun's disk, equal to a circle of 15″ in diameter, would far exceed the greatest lustre of the full moon; whereas, the light of a planetary nebula, of an equal size, is hardly equal to that of a star of the 8th or 9th magnitude. If, on the other hand, we should suppose them to be groups, or clusters of stars, at a distance sufficiently great to reduce them to so small an apparent diameter, we shall be at a loss to account for their uniform light, if clusters; or for their circular forms, if mere groups of stars.

Perhaps they may be rather allied to nebulous stars. For, should the planetary nebulæ with lucid centres, of which the next article will give an account, be an intermediate step between planetary nebulæ and nebulous stars, the appearances of these different species, when all the individuals of them are fully examined, might throw a considerable light upon the subject.

XII. *Of planetary Nebulæ with Centres.*

In my second catalogue of nebulæ, a single instance of a planetary nebula with a bright central point was mentioned; and, in the annexed one, No. 73 of the 4th class, is another of very nearly the same diameter, which has also a lucid, though not quite so regular a centre. From several particularities observed in their construction, it would seem as if they were related to nebulous stars. If we might suppose that a gradual condensation of the nebulosity about a nebulous star could take place, this would be one of them, in a very advanced state of compression. A further discussion of this point, however, must be reserved to a future opportunity.

CORRECTION OF A FORMER PAPER.

In my Paper on two lately discovered celestial Bodies,
Page 224, line 18, of this volume, instead of 135, read 31.

CATALOGUE OF 500 ADDITIONAL NEW NEBULÆ, AND CLUSTERS OF STARS.

First Class. Bright Nebulæ.

I.	1788.	Stars.		M.	S.		D.	M.	Ob.	Description.
216	Dec. 3	22 Ursæ	*p*	13	52	*ſ*	3	4	2	*v*B.*p*L.*i*F.*r*.*mb*M. Towards the *ſf*, within the nebulosity, is a *v*S.*ſt*.
217	27	54 Persei	*f*	9	25	*n*	0	46	2	*c*B. *c*L. *mb*M. Stands nearly in the center of a trapezium.
218	31	63 Aurigæ	*f*	26	43	*ſ*	0	20	1	*c*B. R. *vgmb*M. about 3′*d*.
219	1789 Mar. 23	55 Ursæ	*f*	5	33	*n*	0	36	1	*v*B. *c*L. *i*F. *vgmb*M.
220	Apr. 12	64 (γ) Ursæ	*p*	43	59	*ſ*	0	20	2	*c*B. *m*E. 70° *np ſf*. 3 or 4′ *l*. 2′ *b*.
221	—	— —	*p*	21	41	*ſ*	0	37	2	*c*B. R. *vgmb*M, 4 or 5′*d*.
222	—	— —	*p*	20	20	*ſ*	0	35	2	*c*B. *i*E. near mer. *gb*M. 2′*l*.
223	—	— —	*f*	6	4	*ſ*	2	45	2	*v*B. *m*E. *np ſf*. BN. 5′*l*. 1½′*b*.
224	—	1 Canum	*p*	9	19	*ſ*	3	10	2	*c*B. *p*L. *m*E. SN.
225	—	— —	*p*	8	31	*ſ*	0	46	2	*v*B. *p*L. B*r*N. just *f a cſt*.
226	14	64 (γ) Ursæ	*p*	33	32	*ſ*	0	34	1	*c*B.R.SB*r*N and *v*F chev. 4′*d*.
227	—	— —	*p*	15	28	*n*	2	37	2	*c*B. *c*L. *i*F. *r*. *vgb*M. 3′ *l*. 2′*b*.
228	—	— —	*p*	5	20	*n*	2	24	2	*v*B. *v*B*i*N. and F. bran. 1½′*l*. ¾′ *b*.
229	—	— —	*f*	3	46	*n*	1	47	1	The 2d of 2. *v*B. R. *vgb*M. See II. 791.
230	—	83 Ursæ	*f*	20	24	*n*	0	27	2	*c*B. S. E. *ſp nf*. *c*BN. and F bran.
231	—	— —	*f*	24	34	*n*	0	10	2	*c*B. *p*S. *i*R.
232	—	— —	*f*	27	7	*n*	0	16	1	The 2d of 2. *c*B.S.R. *vgmb*M. See III. 791.
233	17	44 Ursæ	*f*	1	14	*ſ*	0	16	2	*c*B. E. 30° *ſp nf*. *r*. *mb*M. 3′*l*. 1½′*b*.

I.	1789.	Stars.		M.	S.		M.	D	ob.	Description.
234	Apr. 17	74 Ursæ -	*f*	1	31	ſ	0	28	2	*c*B. S. *l*E. Just *p a p*L *ſt*.
235	—	12 (ι) Draconis	*p*	66	52	ſ	2	3	2	*c*B. *i*F *vgmb*M. 7′ *l*, 5′ *b*.
236	—	— —	*p*	59	56	ſ	2	13	3	*v*B. S. *i*R. B*ir*N. *vgmb*M.
237	—	— —	*p*	54	10	ſ	0	52	1	B. *i* oval. *vgmb*M.
238	24	69 Ursæ Hev.	*f*	27	55	ſ	0	32	2	*c*B. *p*L. *i*R *vgmb*M.
239	—	— —	*f*	28	10	ſ	0	17	3	*c*B. *p*L. E. *mb*M.
240	—	— —	*f*	28	34	ſ	0	17	2	*c*B. *p*L. E. SBN.
241	1790 Feb. 17	19 (ξ) Hyd. Crat.	*p*	14	43	ſ	0	57	1	*c*B. E. 70° *np ſf*. *vgb*M 7′ *l*, 4′*b*. within a parallelogram.
242	Mar. 17	15 (*f*) Ursæ	*p*	15	40	ſ	0	21	1	*v*B. LB*r*N. with *v*F chev.
243	—	77 (ε) Ursæ	*f*	1	47	*n*	2	25	1	*c*B. S. R. *gb*M.
244	18	39 Ursæ -	*f*	36	44	*n*	0	40	2	*c*B. R. *vgmb*M. $1\frac{1}{2}'$ *d*.
245	—	— —	*f*	39	27	*n*	1	58	3	*v*B. *c*L. R. *vgb*M.
246	—	66 Ursæ -	*p*	29	19	*n*	0	20	2	*c*B. *p*L. E.
247	—	— —	*p*	28	13	*n*	2	0	2	*v*B. *p*L. *l*E. near par. *mb*M.
248	—	— —	*p*	7	5	*n*	2	52	2	*c*B. *p*L. *i*F.
249	19	17 Ursæ -	*p*	9	0	*n*	3	43	2	*c*B. E. near par. *er*. *b*M. 4′ *l*, 2′ *b*. I suppose, with a higher power and longer attention, the stars would become visible.
250	—	— —	*p*	4	47	*n*	3	17	1	*v*B. *c*L. *l*E. LBNM.
251	—	76 Ursæ -	*p*	50	48	ſ	2	3	1	*v*B. perfectly R. BN and F chev. *vgb*M. $1\frac{1}{2}'$ *d*.
252	—	— —	*p*	41	11	ſ	0	34	1	*v*B. *c*L. R.
253	—	— —	*p*	41	46	ſ	0	51	1	*v*B. *v*L. E.
254	—	— —	*p*	1	47	ſ	1	8	1	*e*B. E. par. 5′ *l*. all over equally B. except just on the edges.
255	—	69 Ursæ Hev.	*f*	19	26	*n*	1	1	1	*v*B. BENM. 3′ *l*. $\frac{1}{2}'$ *b*.
256	—	— —	*f*	21	33	*n*	0	13	1	*v*B. *p*L. *i*F. suddenly *mb*M.
257	Oct. 9	12 Eridani	*f*	16	58	ſ	1	58	1	*c*B. *i*R *vgmb*M. $1\frac{1}{2}'$ *d*.
258	Dec. 28	47 (λ) Persei	*p*	3	41	*n*	1	0	1	*v*B. *i*F. *r*. *b*M. 5′ *l*. 4′*b*. A *p*L star in it towards the *f* side, but unconnected.

I.	1791.	Stars.		M.	S.		D.	M.	Ob.	Description.
259	Mar. 7	17 Hydræ Crat.	*f*	18	31	*n*	0	27	1	*c*B. *p*L. *l*E. *gb*M. The brightness takes up a large space of it.
260	Apr. 2	23 (*b*) Ursæ	*p*	1	49	ſ	0	34	1	*v*B. *v*S. *i*R. *mb*M.
261	1793 Feb. 4	38 of the *Connois.*	*f*	3	7	ſ	1	35	1	*v*B. *i*R. *vgb*M. 5′*d.* Seems to have 1 or 2 stars in the middle, or an *i*N; the chev. diminishes *vg*.
262	Apr. 6	1 (λ) Draconis	*p*	2	6	ſ	2	41	1	*c*B. *v*S. *i*F. N. with *v*F. chev.
263	—	4 Draconis -	*p*	22	48	ſ	0	23	1	*c*B. *l*E. *b*M.
264	7	— —	*p*	14	18	*n*	1	36	1	*c*B. S. *b*M.
265	8	37 Ursæ -	*p*	16	16	*n*	1	5	1	*c*B. S. *i*R. *vgmb*M.
266	—	— —	*p*	13	35	ſ	0	11	1	*c*B. *p*L. *i*F. *gb*M.
267	—	39 Ursæ -	*f*	11	21	ſ	0	10	1	*c*B. *p*L. *i*R. $1\frac{1}{4}$′*d.* The greatest part of it almost equally B.
268	—	— —	*f*	12	46	ſ	0	4	1	*v*B. *v*S. R. Stellar.
269	—	— —	*f*	18	1	*n*	0	29	1	*c*B. R. 1′*d.* just *n* of a S*ſt.*
270	—	— —	*f*	35	36	*n*	1	42	2	*v*B. *c*L. E. par. SN. E par.
271	—	— —	*f*	35	54	*n*	0	55	1	*v*B. *c*L. E. *mb*M.
272	1796 Mar. 4	Georgian planet	*p*	0	53	*n*	0	6	2	*c*B. S. *i*R. BN. *mb*M. This nebula was seen at 9^h 27′, sidereal time; the telescope being out of the meridian.
273	Nov. 22	A double star	*f*	5	45	ſ	0	39	3	*v*B. *v*L. E. near par. The determining star follows 5 Draconis Hevelii 13′ 54″ in time, and is 0° 23′ more south.
274	—	— —	*f*	10	13	ſ	0	24	3	*c*B. *v*S. *i*R. *b*M.
275	Dec. 10	5 Dracon. Hev.	*f*	1	32	*n*	0	12	2	*c*B. S. R.
276	—	— —	*f*	2	45	*n*	0	12	2	*c*B. *c*L. *i*F. *l*E. *mb*M.
277	—	— —	*f*	6	20	*n*	0	20	2	*v*B. *c*L. *l*E. *mb*M.
278	12	— —	*p*	11	5	ſ	0	15	1	*c*B. *c*L. *i*R. *mb*M.

I.	1796.	Stars.		M.	S.		D.	M.	Ob.	Description.
279	Dec. 12	— —	*p*	10	28	*n*	1	38	2	*c*B. *c*L. *l*E. *b*M.
280	—	16 (ζ) Ursæ min.	*f*	51	33	*n*	0	3	3	*v*B. *c*L. *l*E. *lb*M. The greatest brightnes confined to a small point.
281	1798 Dec. 9	τ Apps. Sculps. L. C. 95 -	*p*	1	47	*n*	0	27	1	*c*B. E. *np ff.* NM. 6′*l*. 1½′*b*.
282	1801 Apr. 2	208 (N) Camelop. of BODE's Cat.	*p*	153	15	*ſ*	2	43	1	*c*B. *p*L. *i*F.
283	—	— —	*p*	113	40	*ſ*	3	4	1	*c*B. *c*L. *er*.
284	—	— —	*p*	85	18	*ſ*	0	23	1	*c*B. *v*S. *i*F.
285	Nov. 8	24 *(d)* Ursæ	*f*	13	14	*ſ*	1	53	1	*v*B. *v*L. E. *np ff.* 6′*l*. 2′*b*.
286	—	— —	*f*	30	0	*ſ*	1	8	1	*v*B. *c*L. R. *vgmb*M. On the north-following side there is a F ray interrupting the roundness.
287	Dec. 7	1 (λ) Draconis	*p*	4	37	*n*	1	13	1	*c*B. *m*E. *np ff.* *mb*M. 3′*l*, 1′*b*.
288	1802 Sept. 26	184 Camelopar. of BODE's Cat.	*p*	11	58	*ſ*	2	34	1	*v*B. *c*L. *l*E. suddenly *mb*M.

Second Class. Faint Nebulæ.

II.	1789.	Stars.		M.	S.		D.	M.	Ob.	Description.
769	Feb. 22	81 (*g*) Geminor.	*p*	37	58	*n*	0	4	1	*p*B. *p*L. *i*R. *er*. *b*M.
770	—	62 Ursæ -	*p*	13	44	*ſ*	2	15	1	*p*B. *p*L. R. *lb*M.
771	Mar. 20	26 (χ) Virginis	*p*	7	0	*n*	0	26	2	*p*B. *c*L. *i*F. *er*. *mb*M. 4 or 5′*d*.
772	—	— —	*f*	3	9	*n*	0	57	2	F. S. E.
773	—	— —	*f*	3	5	*n*	1	1	2	F. S. E. *b*M.
774	—	— —	*f*	6	27	*n*	0	55	2	*p*B. S. *i*R. *mb*M.
775	23	55 Ursæ -	*f*	3	31	*ſ*	0	25	1	*p*B. *c*L. *l*E. *vgmb*M.
776	—	26 (χ) Virginis	*p*	8	19	*ſ*	0	4	1	F. *v*L. *er*.
777	—	— —	*f*	17	15	*n*	1	9	1	F. S. R. *b*M.
778	—	— —	*f*	21	12	*n*	1	54	1	F. S. *ff*. a double star.
779	—	26 (χ) Virginis	*f*	22	44	*n*	0	14	1	F. S.
780	26	46 (γ) Hydræ	*f*	1	22	*ſ*	1	14	1	F. R. *r*. *vglb*M. 4′*d*.

II.	1789.	Stars.		M.	S.		D.	M.	Ob.	Description.
781	Apr. 12	1 Canum -	*p*	10	55	ſ	0	53	2	A *p*S. *ſt.* involved in nebulosity of no great extent; the *ſt.* does not seem to belong to it.
782	14	64 (γ) Ursæ	*p*	31	7	*n*	0	7	1	*p*B. S. R. *vgmb*M. just *f* a S*ſt*.
783	—	— —	*p*	18	40	*n*	0	50	1	*p*B. *p*L. *b*M.
784	—	— —	*p*	17	41	*n*	0	37	1	*p*B. *c*L. *l*E. 3′*l*.
785	—	— —	*p*	7	3	*n*	2	18	1	*p*B. S. *l*E.
786	—	— —	*p*	3	31	*n*	1	39		F. E.
787	—	— —	*p*	3	2	*n*	1	27	1	Two nebulæ; the 1st *p*B. S.
788				3	7		1	24		The 2d *p*B. S.
789	—	— —	*f*	1	35	*n*	1	38	1	Two nebulæ; the 1st *p*B. E.
790										The 2d F. S.
791	—	— —	*f*	3	24	*n*	1	48	1	The 1st of 2. *p*B. S. E. See I. 229.
792	—	1 Canum -	*p*	3	12	*n*	2	47	1	F. S. R. *b*M.
793	—	— —	*p*	0	57	*n*	2	36	2	F. *p*L. *i*F. *b*M.
794	—	77 (ε) Ursæ	*p*	11	32	ſ	0	49	2	F. S.
795	—	— —	*p*	8	25	ſ	1	13	2	*p*B. *v*S. *mb*M.
796	—	— —	*p*	7	20	ſ	1	25	2	*p*B. *c*S. *l*E. B*r*N.
797	—	81 Ursæ -	*p*	3	33	ſ	2	18	2	*p*F. *p*S. R. *vgb*M.
798	—	83 Ursæ -	*f*	0	49	*n*	1	1	1	*p*B. E. 1½′*l*, ½′*b*.
799	—	— —	*f*	21	27	*n*	1	7	2	*p*B. *c*L. E.
800	—	— —	*f*	25	7	*n*	1	2	1	*p*B. S.
801	—	— —	*f*	27	27	*n*	0	23	2	F. *c*L.
802	17	71 Ursæ -	*p*	15	20	*n*	1	33	1	F. S. E.
803	—	— —	*p*	13	57	*n*	0	59	2	F. S. R.
804	—	— —	*p*	5	43	ſ	0	3	1	*p*B. *p*L. *i*F.
805	—	— —	*p*	4	41	*n*	1	20	1	The 2d of 2. *p*B. *p*L. *mb*M. See III. 798.
806	—	— —	*p*	2	13	*n*	1	42	1	*p*B.
807	—	12 (ι) Draconis	*p*	55	48	*n*	0	42	1	*p*B. E. *mer.* 1½′*l*, ¾*b*.
808	24	Neb. II. 756	*p*	24	16	*n*	0	41	1	*p*B. S. *i*F. *er.* mixed with some *p*L. stars, which may perhaps belong to it.
809	—	— —	*p*	15	5	ſ	0	26	1	F. S. E.

II.	1789.	Stars.		M.	S.		D.	M	Ob.	Description.
810	Apr. 24	21 (μ) Draconis	*p*	46	31	*n*	3	23	1	*p*F. *p*S. *l*E.
811	—	— —	*p*	44	9	*n*	0	50	1	*p*B. *i*R. *vgvlb*M.
812	—	— —	*f*	10	4	*n*	2	55	1	F. S. R. *vglb*M.
813	26	5 Canum	*p*	10	53	*ſ*	0	50	1	*p*B. S. *l*E.
814	—	7 Canum	*f*	20	24	*n*	1	20	1	F. S. *vſmb*M.
815	—	82 Ursæ -	*p*	31	48	*ſ*	0	52	1	F. *v*S. Stellar.
816	—	— —	*p*	26	52	*ſ*	1	36	1	F. S. *i*R. *vgmb*M.
817	—	— —	*p*	3	42	*ſ*	1	40	1	*p*B. S. R. *vgb*M.
818	—	12 Draconis	*p*	40	16	*n*	0	33	1	*p*F. *c*S. R. *vgb*M.
	1790									
819	Mar. 8	13 (λ) Hyd. Crat.	*p*	11	58	*n*	0	31	1	*p*F. *p*L. *i*F. *b*M.
820	10	65 Aurigæ	*f*	7	22	*n*	0	1	1	*p*B. S. Stellar.
821	—	70 Geminorum	*p*	1	43	*n*	0	12	1	*p*B. *c*S. *r*. *p a cſt*.
822	17	27 Lyncis -	*p*	25	42	*n*	0	41	1	*p*F. R. *r*. *vgb*M.
823	—	15 (*f*) Ursæ	*p*	12	10	*ſ*	0	18	1	*p*B. S. R. *mb*M.
824	—	26 Ursæ -	*f*	139	17	*ſ*	0	1	1	*p*B. *m*E. 6′*l*, 2′*b*.
825	—	— —	*f*	139	40	*ſ*	1	44	1	*p*B. S. *i*F. *b*M.
826	—	77 (ε) Ursæ	*f*	28	0	*n*	1	42	1	F. S. E.
827	—	— —	*f*	69	19	*n*	3	27	1	*p*B. S. *i*F. *mb*M.
828	18	17 Ursæ -	*p*	6	25	*ſ*	2	57	1	*p*B. S. *vgmb*M.
829	—	66 Ursæ -	*p*	31	14	*n*	1	9	2	F. E. *np ſf. er.* 1½′*l*.
830	—	— —	*p*	15	23	*ſ*	0	20	1	*p*B. E.
831	—	— —	*p*	11	44	*n*	1	22	1	*p*B. *v*S. *l*E.
832	—	— —	*p*	6	53	*n*	2	52	2	*p*B. *p*L. R. The nebulosity of this runs into that of I. 248.
833	—	— —	*p*	1	1	*n*	1	46	1	F. S.
834	19	17 Ursæ -	*p*	11	34	*n*	3	10	1	*p*F. *p*S. *i*F. *er*.
835	—	29 (υ) Ursæ	*f*	5	11	*n*	0	15	2	F. S. E. near par.
836	—	76 Ursæ -	*p*	70	41	*ſ*	0	53	1	F. S. R. *r*. almost of equal light throughout.
837	—	— —	*p*	66	54	*ſ*	1	0	1	*p*B. *l*E.
838	—	— —	*p*	66	15	*ſ*	3	9	1	*p*B. S.
839	—	— —	*p*	63	0	*ſ*	2	28	1	*p*B. *c*S. R. *mb*M.
840	—	— —	*p*	47	30	*ſ*	2	16	1	F. S. *b*M.
841	—	69 Ursæ Hev.	*f*	4	24	*n*	2	46	2	The 1st of 2. *p*B. S. *i*F.
842	—	— —	*f*	4	35	*n*	2	50	2	The 2d of 2. *p*B. *p*L. *i*F.

II.	1790.	Stars.		M.	S.		D.	M.	Ob.	Description.
843	Mar. 19	— —	*f*	26	40	*n*	0	42	1	F. S.
844	—	— —	*f*	27	43	*f*	0	29	1	*p*B. *c*L.
845	20	50 (α) Ursæ	*f*	22	41	*n*	1	44	3	*p*B. *p*L. *i*R. *b*M.
846	—	76 Ursæ -	*p*	23	9	*n*	3	13	1	*p*B. *m*E. *sp nf*. BN. 5′*l*, ½′*b*.
847	—	— —	*p*	19	1	*n*	3	8	1	*p*B. S. *l*E.
848	—	— —	*p*	14	21	*n*	2	8	1	F. *i*F. *b*M. Stellar.
849	—	— —	*p*	9	7	*n*	1	15	1	*p*B. *v*S. *l*E. SN.
850	—	— —	*p*	7	16	*n*	0	48	1	*p*B. *p*L. *i*R. *r*. *vgb*M.
851	Oct. 9	72 Pegasi -	*f*	18	3	*f*	0	6	2	*p*F. *p*L. *i*R. *lb*M. *ſp*. a *v*S*ſt*.
852	—	σ Fornacis L. C. 285 - -	*p*	4	15	*f*	0	34	1	F. *p*L. *i*R. *gb*M.
853	Nov. 26	29 (π) Androm.	*p*	25	38	*f*	0	24	1	F. S. E. near mer.
854	Dec. 25	44 Piscium -	*f*	3	49	*n*	0	56	1	*p*B. *v*S. R. *vgmb*M. pretty well defined on the margin.
855	—	— —	*f*	4	44	*n*	0	10	2	*p*B. *c*L. *i*R. *r*. *vgb*M. *ſp*. a *v*S*ſt*.
856	—	— —	*f*	13	52	*n*	1	8	1	F. S. *vgb*M.
857	—	— —	*f*	13	52	*n*	0	53	1	F. S. *vgb*M.
858	—	— —	*f*	14	10	*n*	0	58	1	*p*B. S. *vgb*M.
859	—	98 (μ) Piscium	*f*	20	28	*n*	0	1	1	*p*B. S. E. near par. *ſp*. a S*ſt*.
860	28	MAYER's Zod. Cat. No. 18	*p*	5	48	*n*	0	39	1	*p*F. *v*S. *vgb*M.
861	—	57 Aurigæ -	*f*	17	30	*n*	1	54	1	*p*B. *p*L. *i*F. *gb*M.
862	—	— —	*f*	23	5	*n*	1	29	1	F. *p*L.
863	29	63 (δ) Piscium	*p*	0	39	*n*	0	44	1	- - - *p*L. *l*E. *r*. *gb*M.
864	1791 Mar. 7	17 Hyd. Crat.	*f*	16	46	*f*	0	1	1	*p*B. S. R. *vgmb*M. almost resembling a N.
865 866	—	— —	*f*	34	2	*f*	0	31	1	Two nebulæ, both F. S. R. *b*M. and nearly in the same par.
867	April 2	73 Ursæ -	*p*	14	8	*f*	1	12	1	*p*B. *v*S. Stellar.
868 869	3	14 (τ) Ursæ	*f*	10 11	38 8	*n*	0	47	1	Two nebulæ, the 1st F. S. *i*F. The 2d F. *p*L. E.

II.	1791.	Stars.		M.	S.		D.	M.	Ob.	Description.
870	April 3	35 Ursæ -	*f*	2	50	ſ	0	36	1	F. S. *i*R. Almost of equal light throughout.
871	—	— —	*f*	3	37	ſ	0	52	1	F. *v*S. *mb*M.
872	—	— —	*f*	21	30	*n*	0	11	1	F. *c*L. *i*R.
873	May 6	13 (γ) Ursæ min.	*f*	37	53	ſ	1	17	1	F. R. *b*M. 1′*d*.
874	24	37 (ξ) Bootis	*f*	34	48	ſ	1	12	1	*p*B. *p*L. *i*R. *vgmb*M.
875	30	25 Herculis	*f*	3	10	*n*	2	12	1	*p*B. S. *l*E. *vgmb*M.
876	1792 Apr. 20	22 (*f*) Bootis	*p*	15	58	*n*	0	26	1	*p*B. *v*S.
877	—	— —	*p*	13	27	*n*	1	21	1	*p*B. *p*L. *i*F.
878	Sept. 16	3 Cephei Hev.	*p*	29	15	ſ	0	25	1	*p*B. *i*F. *b*M. contains 2 stars.
879	1793 April 6	1 (λ) Draconis	*p*	9	49	ſ	2	5	1	*p*B. S. R. *b*M.
880	—	— —	*p*	7	44	*n*	0	6	2	F. S. *l*E. *ſp nf*. but near mer. *gb*M.
881	7	4 Draconis	*p*	45	43	*n*	0	12	1	F. *m*E. *np ff*. but near par. 1½′*l*.
882	8	37 Ursæ -	*p*	10	40	*n*	1	3	1	*p*B. *p*L. *l*E. *b*M.
883	—	— —	*p*	8	36	*n*	0	8	1	F. S. R. *b*M.
884	—	39 Ursæ -	*f*	22	42	ſ	0	37	1	F. S. R. *b*M.
885	—	— —	*f*	37	41	*n*	0	42	1	F. S. *l*E. *np ff*.
886	—	— —	*f*	44	5	ſ	0	2	1	*p*B. *i*F.
887	9	42 Ursæ -	*f*	2	41	*n*	1	56	1	F. *p*L. *i*F. *b*M.
888	—	— —	*f*	7	21	*n*	0	11	1	F. S. R. *b*M.
889	May 12	19 Bootis Hev.	*p*	26	45	*n*	0	20	1	*p*B. *p*L. R. just foll. a S*ſt*.
890	—	— —	*p*	13	20	*n*	0	33	1	*p*B. *p*L. *i*R.
891	—	— —	*f*	6	44	*n*	0	8	1	*p*B. *p*L. *l*E. BM.
892	—	— —	*f*	7	44	*n*	0	24	1	F. S. E. near mer.
893	—	— —	*f*	9	37	ſ	0	22	1	*p*B. S. *i*F.
894	—	— —	*f*	10	46	ſ	0	31	1	F. S.
895	13	93 (τ) Virginis	*p*	21	54	ſ	0	40	1	F. S. *i*R.
896	—	— —	*p*	21	49	ſ	0	40	1	F. S. *i*R.
897	Sept. 6	53 Aquarii	*p*	16	29	*n*	0	7	1	*p*B. *l*E. *r*. 1½′*l*. 1¼′*b*.
898	1794 Mar. 22	Georgian planet	*f*	3	0	*n*	0	33	1	F. 3′ north of a *p*L. red *ſt*. This nebula was seen at 8h 49′, sidereal time, the

II.	1797.	Stars.		M.	S.		D.	M.	Ob.	Description.
										telescope being out of the meridian.
899	Dec. 20	4 (*b*) Ursæ min.	*p*	26	13	*f*	0	40	1	F. S. E. near mer. 1′*l*.
	1798									
900	Dec. 10	18 (ε) Eridani	*p*	20	53	*f*	1	5	1	F. E. *sp nf.* near par. 3′*l*, 1′*b*.
	1799									
901	June 29	93 Herculis -	*p*	27	30	*f*	0	11	1	F. S. *i*F. *er.* 2′*l*.
902	—	— —	*f*	7	47	*n*	0	49	1	F. *p*L. R. *vgb*M. 3½′*d*.
	1801									
903	April 2	208(N)Camelop. of Bode's Cat.	*p*	139	19	*f*	1	39	1	F. *p*L. *r*.
904	—	— —	*p*	68	9	*f*	1	58	1	F. *p*L. *lb*M.
905	—	— —	*p*	36	53	*f*	2	22	1	*p*B. *p*L.
906	Nov. 28	11 (α) Draconis	*f*	86	13	*n*	0	8	1	F. S. *l*E. *sp nf. vglb*M.
	1802									
907	June 26	2 (μ) Lyræ	*f*	5	21	*n*	0	18	1	F. S. *i*F.

Third Class. Very faint Nebulæ.

III.	1788.	Stars.		M.	S.		D.	M.	Ob.	Description.
748	Dec. 3	43 Camelop.	*f*	35	5	*n*	0	29	1	*v*F. *v*S. has a *v*F. bran *nf.*
749	—	22 Ursæ -	*p*	12	45	*f*	0	24	1	*c*F. *v*S.
750	31	63 Aurigæ -	*f*	48	58	*n*	0	43	1	*v*F. S. R. *lb*M.
751	—	39 Lyncis -	*f*	25	15	*f*	0	30	2	*e*F. S.
	1789									
752	Feb. 22	16 (ζ) Cancri	*p*	4	19	*n*	0	8	1	*e*F. *l*E. *f* of a *v*S*st*.
753	—	33 (η) Cancri	*p*	8	11	*f*	0	4	1	*v*F. S. R. *vlb*M.
754	24	6 Corvi -	*p*	17	33	*f*	1	43	1	*e*F. *v*S. R.
755 756	Mar. 20	26 (χ) Virginis	*p*	13	3	*n*	0	20	1	Two nebulæ, both *v*F. *v*S. E. within 1½′ of each other.
757	—	— —	*p*	5	25	*n*	0	38	2	2 *v*S. stars involved in *v*F. nebulosity of no great extent.

III.	1789.	Stars.			M.	S.		D.	M.	ob.	Description.
758 759	Mar. 23	—	—	*f*	20	55	*n*	1	53	1	Two nebulæ, both *v*F. *v*S.
760	—	—	—	*f*	23	47	ſ	0	9	1	*c*F. *v*S. R.
761	—	—	—	*f*	24	55	*n*	0	18	1	*v*F. S.
762	—	102 (υ′)	Virginis	*p*	11	30	*n*	0	36	1	*v*F. *v*S.
763	—	105 (φ)	Virginis	*p*	1	1	ſ	0	1	1	*e*F. S.
764	26	9 (β)	Corvi	*p*	4	55	*n*	0	15	1	*c*F. *p*S. R. Stellar.
765	—	45 (ψ)	Hydræ	*p*	1	35	ſ	0	53	1	*v*F. *p*L. *i*F.
766	—	—	—	*f*	0	39	ſ	0	16	1	*v*F. *v*S.
767	Apr. 12	64 (γ)	Ursæ	*p*	78	24	ſ	3	45	1	*v*F. *p*S. *i*E.
768	—	—	—	*p*	30	48	ſ	0	49	2	*v*F. *v*S. Stellar.
769	—	—	—	*p*	1	40	ſ	1	44	1	*c*F. S.
770	14	—	—	*p*	39	32	*n*	2	2	1	*v*F. *v*S. Stellar.
771	—	—	—	*p*	19	37	*n*	1	8	1	*e*F. S. *i*E. On account of the brightness of 179 Ursæ maj. of BODE's Cat. which was in the field of view with it, I had nearly overlooked it.
772	—	—	—	*p*	19	2	*n*	1	16	1	*v*F. Stellar.
773	—	—	—	*p*	14	0	*n*	2	32	1	*c*F. *p*S. *l*E. just *f* a *v*Sſ*t*.
774	—	—	—	*p*	10	37	ſ	0	58	2	*v*F. S.
775	—	—	—	*p*	10	17	ſ	1	1	1	*v*F. *v*S.
776	—	—	—	*p*	9	33	*n*	2	12	1	*e*F. *p*L. *l*E.
777	—	1 Canum	-	*p*	1	54	ſ	0	33	1	*e*F. S. Stellar.
778	—	77 (ε)	Ursæ	*p*	9	10	ſ	1	4	2	*c*F. S. *l*E. *i*F.
779	—	—	—	*f*	11	36	*n*	0	20	2	*v*F. S.
780	—	—	—	*f*	12	37	ſ	0	19	1	*c*F. S.
781	—	—	—	*f*	12	30	ſ	2	22	1	Two nebulæ. Both *v*F. S.
782					12	44		2	20		
783	—	—	—	*f*	12	33	ſ	2	28	1	*v*F. S. E.
784	—	81 Ursæ	-	*p*	7	6	*n*	0	9	1	*c*F. S. *i*R.
785	—	83 Ursæ	-	*f*	4	34	*n*	0	37	1	2 *e*F. ſ*t*. with nebulosity.
786	—	—	—	*f*	14	3	ſ	0	22	1	*v*F. *v*S. Stellar.
787	—	—	—	*f*	22	27	ſ	0	28	1	*v*F. *v*S.
788	—	—	—	*f*	23	47	ſ	0	24	1	*v*F. *v*S.
789	—	—	—	*f*	23	54	ſ	0	22	1	*v*F. *v*S.

III.	1789.	Stars.		M.	S.		D.	M.	Ob.	Description.
790	Apr. 14	83 Ursæ	*f*	25	23	*ſ*	0	17	1	*v*F. *p*L.
791	—		*f*	27	7	*n*	0	16	1	The 1st of 2. *v*F. S. 4′ dist. from I. 232.
792	17	44 Ursæ	*p*	2	11	*n*	0	50	1	*v*F. S. E. 20° *ſp nf*. *er*.
793	—	48 (β) Ursæ	*f*	1	25	*ſ*	0	10	1	*v*F. *v*S. Stellar. The brightness of β Ursæ is so considerable, that it requires much attention to perceive this nebula.
794	—	71 Ursæ	*p*	22	30	*n*	1	8	1	*c*F. S. ver 300.
795	—	— —	*p*	16	8	*n*	2	5	2	*v*F. S. *i*F. *r*.
796	—	— —	*p*	11	23	*n*	2	52	1	*e*F.
797	—	— —	*p*	10	56	*n*	3	11	2	*v*F. S.
798	—	— —	*p*	5	4	*n*	1	20	1	The 1st of 2. *c*F. *l*E. *iF*. II. 805.
799	—	— —	*p*	1	12	*n*	1	36	1	*v*F. *v*S.
800 }	—	— —	*p*	1	9	*n*	1	37	1	Two nebulæ, both *e*F. *c*S. R.
801										
802	—	74 Ursæ	*f*	4	54	*n*	0	30	2	The 1st of 2. *v*F. S. *l*E. See III. 807.
803	—	69 Ursæ Hev.	*f*	9	33	*ſ*	2	53	2	*e*F. *v*S.
804	—	— —	*f*	46	59	*ſ*	2	18	2	*e*F. S. E. *r*.
805	—	— —	*f*	48	9	*ſ*	0	1	3	*e*F. *v*S. R. Stellar.
806	—	12 (ι) Draconis	*p*	34	20	*n*	0	8	1	*v*F. *v*S. *l*E.
807	24	74 Ursæ	*f*	5	26	*n*	0	34	1	The 2d of 2. *e*F. S. E. differently from III. 802.
808	—	69 Ursæ Hev.	*p*	7	35	*ſ*	2	19	1	*c*F. S. E.
809	—	— —	*f*	27	7	*ſ*	1	25	1	*v*F. *v*S.
810	—	— —	*f*	30	44	*ſ*	0	13	1	*c*F. *v*S. R.
811	—	Neb. II. 756	*f*	0	32	*n*	0	2	1	*v*F. S. E.
812	—	21 (μ) Draconis	*p*	55	20	*n*	3	18	1	*v*F. *v*S. *l*E.
813	—	— —	*p*	36	1	*n*	1	14	1	*v*F. *v*S. *i*R.
814	26	5 Canum	*p*	15	0	*n*	0	32	1	*v*F. S. *er*.
815	—	7 Canum	*f*	18	48	*ſ*	0	22	1	S. Stellar.
816	—	— —	*f*	25	11	*n*	1	33	1	*e*F. S. *l*E.
817	—	— —	*f*	26	43	*n*	0	45	1	*c*F. S. *i*F.
818	—	— —	*f*	33	4	*ſ*	1	7	1	*c*F. S. R. *vglb*M.

III.	1789.	Stars.		M.	S.		D.	M.	Ob.	Description.
819	Apr. 26	82 Ursæ -	*p*	32	15	ſ	2	12	1	*v*F.
820	—	— —	*p*	29	17	ſ	2	48	1	2*v*S stars at less than 1′ *d.* with *v*F. nebulosity between them.
821	—	— —	*p*	12	59	ſ	0	7	1	*c*F. Stellar.
822	—	— —	*p*	6	23	ſ	1	25	1	*c*F. *p*S. *i*R. *lb*M.
823	—	— —	*p*	5	5	ſ	1	18	1	*c*F. *p*L. R. *vlb*M.
824	1790 Mar. 8	7(α)Hyd.Crat.	*f*	7	26	ſ	1	9	1	*v*F. *v*S. *i*R. *glb*M.
825	10	39 Lyncis	*p*	12	53	ſ	1	31	1	*v*F. S. R. *b*M. *f* of a S*ſt.*
826	—	— —	*p*	5	55	ſ	1	56	1	*v*F. S *r.*
827	—	— —	*f*	2	11	ſ	1	29	1	*e*F. *v*S. *ff* a *v*S*ſt.*
828	—	Hyd. L. C. 1039	*p*	2	1	ſ	1	11	2	*e*F. *p*S. R. *vgb*M. Stellar. just *p* a *v*S*ſt.*
829	17	27 Lyncis	*p*	23	49	*n*	1	30	1	*e*F. *v*S. R. *b*M.
830	—	— —	*p*	10	40	*n*	1	19	1	*c*F. *p*S. *b*M.
831	—	15 (*f*) Ursæ	*p*	12	8	*n*	0	23	1	*v*F. *v*S.
832	—	— —	*f*	9	39	*n*	0	57	1	*v*F. S. *l*E.
833	—	26 Ursæ	*f*	134	3	ſ	1	43	1	*v*F. *v*S.
834	—	74 Ursæ -	*f*	2	4	ſ	1	56	1	*e*F. S. *i*F.
835	—	77 Ursæ	*f*	82	37	*n*	1	52	1	*e*F. S. E. but nearly R.
836	18	17 Ursæ -	*p*	79	17	ſ	0	33	1	*v*F. *v*S. may be a patch of stars.
837	—	— —	*p*	75	32	ſ	0	40	1	*e*F. *v*S.
838	—	— —	*p*	75	10	ſ	0	15	1	*e*F. *v*S.
839	—	— —	*p*	72	22	ſ	3	40	1	*e*F. *v*S.
840	—	— —	*p*	63	56	ſ	1	28	1	*c*F. *c*S.
841	—	— —	*p*	16	9	ſ	1	9	1	*v*F. S.
842	—	43 Ursæ	*p*	5	8	ſ	0	39	1	*v*F. *v*S. R.
843	—	66 Ursæ -	*p*	19	23	*n*	1	52	1	*v*F. Stellar. *np* a S*ſt.*
844	—	— —	*p*	16	1	*n*	2	2	1	*v*F. S. *m*E.
845	—	69 (δ) Ursæ	*p*	4	55	*n*	1	17	1	*v*F. S. E. par.
846	19	20 Ursæ -	*f*	7	53	ſ	2	23	1	*c*F. S. *m*E. very narrow.
847	—	76 Ursæ	*p*	67	53	ſ	2	50	1	*e*F. *v*S. *i*F.
848	—	69 Ursæ Hev.	*p*	19	5	*n*	2	13	1	*v*F. *v*S.
849	—	— —	*f*	23	53	ſ	0	8	1	*v*F. *v*S.
850	20	76 Ursæ -	*p*	26	56	*n*	3	17	1	*v*F. *p*S.

III.	1790.	Stars.		M.	S.		M.	D.	Ob.	Description.
851	Mar. 20	76 Ursæ -	*p*	25	25	*n*	0	43	1	*e*F. S. *i*F.
852	—	— —	*p*	16	38	*n*	2	12	1	*v*F. Stellar, *nf* a S triangle of B*ſt*.
853	Apr. 1	30 (φ) Ursæ	*f*	8	55	*n*	1	35	1	*v*F. S. *vglb*M.
854	Oct. 9	72 Pegasi	*f*	15	8	*ſ*	0	23	2	2*v*S close *ſt*. with nebulosity between.
855 856	—	— —	*f*	27	15	*n*	0	3::	1	Two nebulæ, both *e*F. Stellar. dist. 1′ from 30° *ſp* to *nf*.
857	—	σ Fornacis L. C. 285 -	*p*	12	30	*ſ*	1	54	1	*v*F. S. *i*F. *lb*M.
858	10	6 Pegasi -	*p*	24	40	*n*	0	43	1	*e*F. *p*L. *i*R. *vlb*M. requires great attention to be seen.
859	—	— —	*p*	7	56	*n*	0	17	1	*c*F. *v*S. *i*R. *mb*M. near a *v*S*ſt*.
860	Nov. 2	72 Pegasi	*p*	5	19	*n*	1	7	1	*v*F. S. *lb*M.
861	—	— —	*f*	37	50	*ſ*	0	17	1	*e*F. S.
862	8	1 Lacertæ Hev.	*p*	3	17	*n*	1	19	1	*e*F. *p*L. *i*R. *r*.
863	—	— —	*f*	3	9	*n*	0	48	1	*v*F. *v*S. *mb*M.
864	—	— —	*f*	4	37	*n*	0	50	1	*v*F. S. *m*E. 75° *np ſf*. *b*M.
865	13	26 Aurigæ	*p*	1	9	*n*	1	31	1	*v*F. *v*S. R. *b*M.
866	26	29 (π) Androm.	*p*	27	37	*ſ*	0	20	1	*v*F. *v*S. The *np* corner of a square.
867	Dec. 6	Mayer's Zod. Cat. 20 -	*p*	49	19	*ſ*	1	39	1	*e*F. *p*S. *i*R. *lb*M.
868	—	— —	*p*	39	35	*ſ*	0	42	1	*e*F. *p*S. *i*F.
869	25	44 Piscium	*f*	3	25	*n*	0	55	1	*v*F. *v*S. *b*M. *p*. and in the field with II. 854. *nf*. 2. S*ſt*.
870	—	— —	*f*	12	48	*n*	0	49	1	*v*F. S. *i*R. *vgb*M.
871	28	Mayer's Zod. Cat. 18 -	*p*	8	1	*n*	1	44	1	*v*F. S. R. *vgb*M.
872	—	— —	*p*	5	52	*n*	0	41	1	*v*F. *v*S. *b*M.
873	—	— —	*p*	5	32	*n*	0	39	1	*e*F. *c*L. In the field with the foregoing, and with II. 860.
874	—	57 Aurigæ	*f*	17	56	*n*	1	50	1	*v*F. *v*S. *l*E.
875	—	— —	*f*	21	42	*ſ*	0	7	1	*v*F. *v*S.

III.	1790.	Stars.		M.	S.		D.	M.	Ob.	Description.
876	Dec. 29	51 Piscium	*f*	5	44	*n*	1	43	1	*v*F. *p*L. *i*R. *ff* a S*ſt* which is partly involved in the nebulosity.
877	1791 Feb. 23	26 Hydræ	*p*	73	56	*n*	0	22	1	*v*F. *i*R. *r*. 2′*d*. almost of equal light throughout.
878	Apr. 2	14 (τ) Ursæ	*f*	9	14	*n*	0	38	2	*v*F. *c*L. R. *mb*M. near 5′*d*.
879	—	73 Ursæ -	*p*	2	39	*ſ*	1	12	1	*c*F. S. *i*F.
880	—	— —	*f*	8	13	*ſ*	1	26	1	*e*F. S.
881	3	35 Ursæ -	*f*	21	51	*u*	0	13	1	*v*F. S.
882	May 6	9 Ursæ min.	*p*	34	52	*ſ*	2	0	1	*v*F. *p*L. R. *b*M.
883	—	13 (γ) Ursæ min.	*f*	42	41	*ſ*	1	36	1	*e*F. *v*S. ver. 300.
884	—	— —	*f*	44	51	*ſ*	2	22	1	*v*F. *v*S. with 300 *c*L.
885	24	37 (ξ) Bootis	*p*	3	44	*ſ*	0	35	1	*e*F. *v*S. E. near par.
886 887	26	7 Serpentis	*p*	15	32	*n*	0	20	1	Two nebulæ, both *e*F. *v*S. the *p* is the most *n*. dist. 1½′.
888	27	19 (ξ) Coronæ	*p*	6	41	*n*	1	7	1	*e*F. *v*S. R. with 300 *p*L.
889	28	17 (σ) Coronæ	*p*	2	1	*ſ*	0	52	1	*v*F. S. R. *vglb*M.
890	—	20 (ν′) Coronæ	*f*	8	9	*n*	1	20	1	*v*F. *p*L. *l*E. *lb*M.
891	30	25 Herculis	*p*	3	41	*n*	0	37	1	*e*F. *v*S. R. *lb*M.
892	—	— —	*p*	2	5	*ſ*	0	9	1	*e*F. S. *b*M.
893	—	44 (η) Herculis	*p*	6	26	*n*	0	8	1	*e*F. vS. *i*F. ver. 300.
894	1792 Apr. 20	22 (*f*) Bootis	*f*	12	29	*n*	1	15	1	*v*F. *v*S.
895	—	— —	*f*	12	55	*n*	0	47	1	*v*F. *v*S.
896	—	— —	*f*	16	45	*ſ*	0	25	1	*e*F. S. *vlb*M.
897 898	1793 Feb. 4	34 (θ) Gemin.	*p*	1	33	*ſ*	0	31	1	Two nebulæ. The most *n*. and *p*. *e*F. S. The other *e*F. *v*S. dist. 4′.
899	—	— —	*f*	15	18	*n*	1	17	1	*v*F. S. nearly R. *b*M.
900 901	—	— —	*f*	36	21	*n*	0	9	1	Two nebulæ just preceding III. 703. Both *e*F.
902	Mar. 8	18 Navis	*f*	10	36	*n*	0	32	1	*v*F. *l*E. *r*. *b*M.
903	Apr. 6	4 Draconis	*p*	30	43	*n*	0	10	1	*e*F. S. *i*F. *vlb*M.
904	—	— —	*p*	23	25	*n*	0	24	1	*e*F. *v*S. E. mer.

III.	1793.	Stars.		M.	S.		D.	M.	Ob.	Description.
905	Apr. 7	4 Draconis	*p*	37	3	*n*	0	8	1	*e*F. *v*S. ver. 300.
906	—	6 Draconis	*f*	12	31	*n*	1	8	1	*v*F. E. 2′*l*, $\frac{1}{2}$′*b*.
907	—	— —	*f*	16	26	*n*	1	35	1	*v*F. E. *np ſf*. 1$\frac{1}{2}$′*l*, $\frac{1}{2}$′*b*.
908	—	— —	*f*	23	36	*n*	0	10	1	*e*F. *v*S. *i*R. *vlb*M.
909	—	— —	*f*	39	10	*n*	0	35	1	*v*F. *v*S. R.
910	8	37 Ursæ -	*p*	15	47	*n*	0	19	1	*v*F. *p*L. *i*F. *r*. some of the stars visible.
911	—	— —	*p*	11	47	*ſ*	0	5	1	*v*F. *c*L. *i*F.
912	—	— —	*f*	0	59	*n*	1	27	1	*e*F. *v*S. ver 300.
913	—	39 Ursæ -	*f*	8	14	*n*	1	14	1	*v*F. *v*S.
914	—	— —	*f*	10	29	*ſ*	0	2	1	*v*F. S. *l*E.
915	—	— —	*f*	25	35	*n*	0	3	1	*v*F. S.
916	9	42 Ursæ -	*p*	48	48	*n*	0	39	1	*e*F. *v*S. Stellar near a S*ſt*.
917	—	— —	*p*	15	19	*ſ*	0	44	1	Two nebulæ.
918				15	10		0	47		Both *v*F. *p* S. R. *lb*M.
919	—	— —	*p*	0	1	*n*	2	2	1	*v*F. *v*S. near a *v*S*ſt*.
920	—	— —	*f*	19	23	*n*	2	1	1	*e*F. *v*S. E. near mer.
921	—	— —	*f*	24	11	*n*	1	22	1	*e*F. *p*L. E.
922	—	— —	*f*	35	14	*n*	1	11	1	*v*F. *v*S. 2*v*S. stars in it.
923	May 5	Hydr. L. C. 1179	*p*	1	25	*n*	0	5	1	*v*F. *v*S. R. *lb*M.
924	—	6 Hydræ conti	*f*	11	2	*ſ*	1	27	1	*e*F. S. *r*. ver. 300.
925	12	64 Virginis	*f*	1	18	*n*	1	10	1	*c*F. S.
926	—	— —	*f*	13	5	*n*	1	17	1	*v*F. S. *ſp*. a *c*B*ſt*.
927	—	19 Bootis Hev.	*p*	0	20	*n*	0	44	1	*v*F. S.
928	13	93 (τ) Virgin.	*p*	26	17	*ſ*	0	5	1	*v*F. S.
929	—	— —	*p*	9	25	*n*	0	35	1	*v*F. S. E. mer.
930	Sept. 6	53 Aquarii	*p*	27	19	*n*	0	18	1	*e*F. ver. 300.
931	—	— —	*p*	12	23	*ſ*	0	19	1	*e*F. S. *i*R.
932	—	— —	*p*	8	50	*n*	1	11	1	*e*F. S. *l* E. *ſ* of a S*ſt*. to which it seems almost to be attached, but is free from it. The star is the 1st of 3, making a S triangle.
933	—	— —	*p*	6	7	*n*	0	58	1	*v*F. S. R. *b*M.
	1794									
934	Apr. 1	Georgian planet	*p*	0	16	*ſ*	0	2	1	*v*F. This nebula was seen at 9^h 45′, sidereal time, the

III.	1794.	Stars.		M.	S.		D.	M.	Ob.	Description.
										telescope being out of the meridian.
935	Apr. 19	12 (δ) Hydræ crateris -	*f*	15	11	*n*	0	40	1	*e*F. S. *b*M.
936	Oct. 15	5 (α) Cephei	*f*	7	54	*n*	0	16	1	*v*F. *er*,
	1797									
937	Nov. 22	Neb. I. 274	*f*	25	3	*n*	0	53	1	*v*F. S. *i*R. *b*M.
938	Dec. 10	A double st*	*p*	9	5	*n*	0	10	1	*e*F. *p*L. *i*F.* See I. 273.
939	—	— —	*f*	4	0	*ſ*	0	35	1	*e*F. S.
940	12	5 Dracon. Hev.	*p*	32	24	*ſ*	0	49	1	*v*F. S. R. *b*M.
941	—	— —	*p*	8	21	*n*	0	57	1	*v*F. *p*S. 2 S *nf* stars make a a triangle with it.
942	—	— —	*f*	4	16	*n*	0	59	1	*e*F. E. near mer. ver. 300.
943 944	—	5 (*a*) Ursæ mi.	*f*	46	2	*ſ*	0	28	1	Two nebulæ. Both *v*F. *v*S. *r*. dist. 1½′ par.
945	—	35 Draconis	*p*	47	10	*ſ*	1	17	1	*v*F. S. E. *n* of a S*ſt*.
946	20	4 (*b*) Ursæ mi.	*p*	29	31	*n*	1	57	1	*v*F. *v*S. R.
947	—	— —	*p*	14	39	*n*	0	42	1	*v*F. *c*L. *i*F. *vlb*M. *ſ* of a *p*B. *ſt*.
948	—	— —	*f*	2	20	*n*	1	3	1	*e*F. *v*S. E. near mer.
949	—	— —	*f*	14	44	*n*	2	29	1	*e*F. S. *l*E. near par.
950	—	— —	*f*	24	18	*n*	1	13	1	*v*F. S. *r*. It is preceded by a S. patch of *ſt*. which appears almost like this nebula, but more resolved.
951	—	4 Cephei of Bode's Cat.	*p*	21	18	*ſ*	1	25	1	*e*F. S. better with 320.
	1798									
952 953	Dec. 9	2 (π′) Orionis	*p*	10	20	*ſ*	1	34	1	Two nebulæ within 1′ of each other; mer. Both *v*F. *v*S.
954	10	8 Ceti -	*f*	17	5	*ſ*	1	15	1	*e*F. S.
955	—	21 Ceti -	*p*	3	46	*n*	0	4	1	*c*F. *v*S. *i*R.
956	—	18 (ε) Eridani	*p*	15	21	*ſ*	0	53	1	*v*F. *v*S. 2 or 3′ *n* of 2S*ſt*.
	1799									
957 958	June 29	93 Herculis	*p*	4 3	3 59	*n*	1 1	33 37	1	Two nebulæ. Both *v*F. *v*S.

III.	1799.	Stars.		M.	S.		D.	M.	Ob.	Description.
959	Dec. 19	16 Eridani -	*f*	6	37	*n*	0	26	1	The 2d of 2 *v*F. *v*S. $1\frac{1}{2}'$ *ſf*l. 60.
960	—	19 Eridani	*f*	1	19	*n*	1	13	1	*v*F. *v*S. ver. 300.
961	—	— —	*f*	2	43	*n*	0	46	1	*v*F. *v*S.
962	—	— —	*f*	20	51	*n*	1	15	1	*v*F. *v*S. *ſp*. 2 *p*B*ſt*.
	1801									
963	Apr. 2	208(N)Camelop. of Bode's Cat.	*p*	157	36	*f*	1	16	1	*e*F. S. *i*F.
964	—	— —	*p*	119	54	*f*	3	5	1	*c*F. S. Stellar. ver. 300. just *p*. *a* S*ſt*.
965	—	— —	*p*	117	22	*f*	2	56	1	*v*F. *v*S.
966	—	— —	*p*	118	0	*n*	0	29	1	*v*F. *v*S.
967 968	—	— —	*p*	72	10	*f*	1	52	1	Two nebulæ. The 1st *v*F. S. The 2d *nf*. the 1st *e*F. *v*S.
969	—	— —	*p*	37	31	*f*	2	39	1	*e*F. S.
970	—	— —	*p*	24	19	*n*	0	28	1	*v*F. *p*L. *r*.
971	—	— —	*p*	20	34	*f*	2	31	1	*e*F. *v*S. R.
972	Nov. 28	50 (α) Ursæ	*p*	5	7	*f*	0	10	1	*v*F. *v*S. R. *b*M.
973	Dec. 6	16 (ζ) Ursæ mi.	*f*	14	15	*n*	1	8	1	*v*F. S. *l*E. mer. *r*.
	1802									
974 975	Jan. 1	22 (ε) Ursæ mi.	*p*	10	49	*n*	0	37	1	Two nebulæ; the preceding *c*F. S. *b*M. the foll. *v*F. *v*S. it follows the 1st a few seconds, and is 3′ more north.
976	May 21	2 (η) Coronæ	*p*	26	50	*n*	0	2	1	*e*F. S. *i*F.
977	Sep. 26	186 P. Camelop. of Bode's Cat.	*f*	9	49	*f*	1	33	1	*e*F. *v*S. 300 confir.
978	—	— —	*f*	33	19	*f*	0	58	1	*e*F. *p*L. *vlb*M. just *n* of 2*ſt*.

Fourth Class. Planetary Nebulæ.

Stars with Burs, with milky Chevelures, with short Rays, remarkable Shapes, &c.

IV.	1789.	Stars.		M.	S.		D.	M.	Ob.	Description.
59	Mar. 23	55 Ursæ -	*f*	4	51	*n*	0	23	1	*c*B. S. R. BN. The N is considerably well defined, and the chevelure *v*F.
60	Apr. 12	36 Ursæ -	*f*	8	37	ſ	2	28	2	*vB*. R. Planetary, but very ill defined. The indistinctness on the edges is sufficiently extensive to make this a step between planetary neb. and those which are described *vfmb*M.
61	—	64 (γ) Ursæ	*f*	3	56	ſ	0	19	2	*c*B. B*r*N with *v*FE branches about 30° *np ſf*. 7 or 8′*l*, 4 or 5′*b*.
62	14	— —	*f*	2	27	*n*	1	25	1	*c*B. quite R. A large place in the middle is nearly of an equal brightness. Towards the margin it is less bright.
63	24	69 Ursæ Hev.	*f*	1	24	ſ	1	33	1	*c*B. *c*L. *i*R. *er*. *vgmb*M. 4′ diam. I suppose, with a higher power, I might have seen the stars.
64	1790 Mar. 4	6 Navis -	*p*	7	41	ſ	1	2	2	A beautiful planetary nebula, of a considerable degree of brightness; not very well defined, about 12 or 15″ diam.
65	5	28 Monocerotis	*p*	51	49	*n*	0	26	1	A pretty considerable star, 9 or 10m. visibly affected with *v*F. nebulosity, of very little extent all

IV.	1790.	Stars.		M.	S.		D.	M.	Ob.	Description.
										around. A power of 300 shewed the same, but gave a little more extent to the nebulosity. The 22d Monocerotis was quite free from nebulosity.
66	Mar. 18	17 Ursæ -	*p*	16	29	*f*	3	6	1	A small star with a *p*B. fan-shaped nebula. The star is on the *p* side of the diverging chevelure, and seems to be connected with it.
67	—	66 Ursæ -	*p*	0	39	*n*	1	55	1	*p*B. *p*L. R. The greatest part of it equally B, then fading away *p* suddenly; between 2 and 3′ diam.
68	19	45 Lyncis -	*p*	4	15	*n*	1	44	1	*v*B. S. exactly R. BNM. and *v*F. chev. *vg*. joining to the N. In a lower situation the chev. might not be visible, and this neb. would then appear like an ill defined planetary one.
69	Nov. 30	26 Aurigæ or 31 Hevelii	*p* *f*	88 24	24 59	*f* *f*	0 1	11 26	1	A most singular phenomenon; A *st* 8m. with a faint luminous atmosphere of a circular form, about 3′ in diam. The star is perfectly in the centre, and the atmosphere is so diluted, faint, and equal throughout, that there can be no surmise of its consisting of stars, nor can there be a doubt of the evident connection between the atmosphere and the star.

IV.	1790.	Stars.		M.	S.		D.	M.	Ob.	Description.
										Another star, not much less in brightness, and in the same field with the above, was perfectly free from any such appearance.
70	1791 Mar. 6	6 Draconis	*f*	50	27	*n*	0	27	2	*c*B. R. almost equally B throughout, resembling a very ill defined planetary neb. about ½′ diam.
71	May 24	37 (ξ) Bootis	*f*	16	5	*f*	0	44	1	A star 7.6m. enveloped in extensive milky nebulosity. Another star 7m. is perfectly free from such appearance.
72	1792 Sep. 15	34 Cygni -	*p*	5	10	*n*	0	23	1	A double star of the 8th magnitude, with a faint south-preceding milky ray joining to it, 8′*l*, and 1½′ broad.
73	1793 Sep. 6	16 (*c*′) Cygni	*f*	2	51	*f*	0	1	1	A bright point, a little extended, like two points close to one another; as bright as a star of the 8.9 magnitude, surrounded by a very bright milky nebulosity suddenly terminated, having the appearance of a planetary nebula with a lucid centre; the border however is not very well defined. It is perfectly round, and I suppose about half a minute in diam. It

IV.	1793.	Stars.		M.	S.		M.	D.	Ob.	Description.
										is of a middle species, between the planetary nebulæ and nebulous stars, and is a beautiful phenomenon.
74	1794 Oct. 18	7 Cephei -	*p*	24	57	*n*	1	22	1	A star 7m. very much affected with nebulosity, which more than fills the field. It seems to extend to at least a degree all around; smaller stars, such as 9 or 10m. of which there are many, are perfectly free from this appearance. A star 7.8m. is perfectly free from this appearance.
75	—	7 Cephei -	*f*	14	40	*f*	0	46	2	Three stars about 9m. involved in nebulosity. The whole takes up a space of about 1½′ diam. other stars of the same size are free from nebulosity.
76	1798 Sept. 9	3 (η) Cephei	*p*	10	31	*f*	1	36	1	*c*F. *v*L. *i*F. a sort of BNM. The nebulosity 6 or 7′. The N seems to consist of stars, the nebulosity is of the milky kind. It is a pretty object.
77	Dec. 19	16 Eridani -	*f*	4	56	*n*	0	14	1	A star about 9 or 10m. with a nebulous ray to the south-preceding side. The ray is about 1½′ long. The star may not be connected with it.

IV.	1801.	Stars.		M.	S.		D.	M.	Ob.	Description.
78	Nov. 8	8 Ursæ min. of BODE's Cat.	*p*	25	0	*n*	0	12	1	*c*B. R. about 1½′ diam. Somewhat approaching to a planetary nebula, with a strong hazy border.

Fifth Class. Very large Nebulæ.

V.	1789.	Stars.		M.	S.		M.	D.	Ob.	Description.
45	Apr. 12	64 (γ) Ursæ -	*f*	0	9	*ſ*	1	23	2	*c*B. *i*F. E. mer. LBN. with F. branches 7 or 8′*l*, 5 or 6′*b*.
46	17	48 (β) Ursæ	*f*	10	4	*ſ*	0	41	2	*v*B. *m*E. *r*. 10′*l*, 2′*b*. There is an unconnected pretty bright star in the middle.
47	1790 April 1	30 (φ) Ursæ	*f*	10	9	*n*	1	39	1	*v*B. *m*E. *np ſf*. *vgmb*M. 8′*l*, 2′*b*.
48	Oct. 9	ι Fornacis L. C. 182 - -	*f*	8	7	*ſ*	0	2	1	*v*B. E. 75° *np ſf*. 8′ long. A very bright nucleus, confined to a small part, or about 1′ diam.
49	Dec. 28	41 Persei Hev.	*f*	22	0	*n*	0	15	1	6 or 7 small stars, with faint nebulosity between them, of considerable extent, and of an irregular figure.
50	1793 Mar. 4	ε Pixidis Na. L. C. 831 -	*f*	35	26	*ſ*	0	43	1	*v*F. *v*S. *l*E. 15° *ſp nf*. *lb*M. 8′*l*, 5 or 6′*b*.
51	April 6	4 Draconis -	*p*	14	48	*n*	0	20	2	*v*F. *m*E. 70° *npſf*. About 25′*l*, and losing itself imperceptibly, about 6 or 7′ broad.

V.	1801.	Stars.		M.	S.		D.	M.	Ob.	Description.
52	Nov. 28	50 (α) Ursæ	*p*	17	49	*n*	1	30	1	*c*B. E. mer. *vgb*M. About 5′*l.* and 3′ broad; the nebulosity seems to be of the milky kind; it loses itself imperceptibly all around. The whole breadth of the sweep seems to be affected with very faint nebulosity.

Sixth Class. Very compressed and rich Clusters of Stars.

Additional Abbreviations. cl. *Cluster*, com. *compressed*, sc. *scattered*, co. *coarsely*.

VI.	1790.	Stars		M.	S.		D.	M.	Ob.	Description.
36	Mar. 4	6 Navis -	*p*	8	45	*ſ*	1	55	2	A *v.* com. cl. of S, and some L*ſt.* E near mer. The most compressed part is about 8′*l*, and 2′*b.* with many scattered to a considerable distance.
37	1791 Feb. 23	26 Hydræ -	*p*	79	30	*n*	1	0	1	A v. com. and very rich cl. of stars. The stars are of 2 sizes, some considerably L. and the rest next to invisible. The com. part 5 or 6′ in diam.
38	Aug. 25	50 (γ) Aquilæ	*p*	14	50	*ſ*	1	18	1	*c*B. S. *i*F. *er.* Some of the *ſt.* are visible.
39	1793 Mar. 3	ζ Pixidis Naut. L. C. 777 -	*p*	20	39	*ſ*	0	19	2	A cl. of L*ſt.* considerably rich *i*R. above 15′ diam.

VI.	1793.	Stars.		M.	S.		D.	M.	Ob.	Description.
40	May 12	53 (ν) Serpentis	*p*	48	17	*n*	0	2	1	A very beautiful *e* com. cl. of *ſt.* extremely rich, 5 or 6′ in diam. gradually more compressed towards the centre.
41	1797 Dec. 12	35 Draconis -	*p*	22	6	*ſ*	1	7	1	R. *r.* about 3′ diam. *vgb*M. I suppose it to be a cluster of stars extremely compressed. 300 confirms the supposition, and shews a few of the stars; it must be immensely rich.
42	1798 Sep. 9	3 (η) Cephei	*p*	13	26	*ſ*	1	6	1	A beautiful compressed cl. of S*ſt.* extr. rich, of an *i*F. The preceding part of it is round, and branching out on the following side, both towards the *n.* and towards the *ſ.* 8 or 9′ in diam.

Seventh Class. Pretty much compressed Clusters of large or small Stars.

VII.	1788.	Stars.		M.	S.		D.	M.	Ob.	Description.
56	Dec. 16	11 (β) Cassiop	*p*	9	57	*n*	2	6	1	A *p.* com. cl. of S*ſt.* of several sizes, cons. rich. E. near par. 5 or 6′*l.*
57	31	40 Aurigæ -	*f*	8	28	*n*	1	25	1	A compressed cl. of *v*S stars *i*F. 6′ diam. consid. rich.
58	1790 Mar. 4	6 Navis -	*f*	5	18	*ſ*	0	29	1	A *p.* com. and rich cl. of S stars *i*R. 7 or 8′ diam.
59	Sept. 11	18 (δ) Cygni	*f*	18	38	*ſ*	1	4	1	A *v.* rich cl. of L*ſt.* considerably compressed, above

VII.	1790.	Stars.		M.	S.		D.	M.	Ob.	Description.
										15′ diam. by the size of the *ſt.* it is situated in the milky-way, towards us.
60	Dec. 28	47 (λ) Persei	*f*	3	30	*ſ*	0	50	1	A L. cl. of *c*L *ſt.* *p.* com. and very rich. *i*R. 7′ diam.
61	—	41 Persei Hev.	*p*	3	8	*n*	0	56	1	A beautiful cl. of L*ſt.* *v* rich, and considerably com. about 15′ diam.
62	1791 Aug. 21	19 Aquilæ -	*p*	0	26	*ſ*	1	24	1	A S. *p.* com. cl. of stars not very rich.
63	1793 Mar. 3	ζ Pixidis Naut. L. C. 777 -	*p*	2	25	*ſ*	0	24	2	A L. cl. of scattered S*ſt.* *i*F. considerably rich.
64	4	— —	*p*	20	55	*ſ*	1	9	1	A L. cl. of *ſt.* of a middling size. *i* E. considerably rich. The stars are chiefly in rows.
65	8	2 Navis -	*p*	16	10	*n*	0	38	1	A S. cl. of *v*S *ſt.* considerably rich and compressed.
66	1794 Oct. 18	7 Cephei -	*f*	16	45	*ſ*	1	7	2	A cl. of cons. com. *v*S and L. stars about 12′ diam. considerably rich.
67	1799 Jan. 30	15 (π′) Canis	*f*	42	33	*ſ*	0	14	2	A cl. of com. stars, considerably rich.

Eighth Class. Coarsely scattered Clusters of Stars.

VIII	1788.	Stars.		M.	S.		D.	M.	Ob.	Description.
79	Dec. 16	11 (β) Cassiop	*f*	20	35	*n*	1	5	1	A coarsely sc. cl. of L*ſt.* mixed with smaller ones, not very rich.
80	18	1 Camelopar.	*p*	41	36	*ſ*	1	29	1	A cl. of S. stars, containing one large one, 10; 9m. 2 or 3′ diam. not rich.
81	1789 July 18	5 Vulpeculæ	*p*	2	46	*n*	2	4	1	A sc. cl. of *c*L *ſt.* *i*F. pretty rich, above 15′ in extent.
82	1790 Sept. 11	57 Cygni -	*f*	1	0	*n*	0	52	1	A L. cl. of *p*S. stars of several sizes.
83	30	51 Cygni -	*p*	25	24	*ſ*	0	1	1	A cl. of sc. stars, above 15′ diam. pretty rich, joining to the milky-way, or a projecting part of it.
84	Dec. 28	33 (α) Persei	*f*	9	14	*n*	1	36	1	A cl. of S*ſt.* not very rich.
85	—	41 Persei Hev.	*f*	2	42	*ſ*	0	2	1	A coarsely sc. cl. of L*ſt.* pretty rich.
86	1792 Sept. 15	34 Cygni -	*p*	9	43	*n*	0	15	1	A coarsely sc. cl. of L stars, of a right-angled triangular shape.
87	1793 Mar. 8	2 Navis -	*p*	7	10	*ſ*	0	15	1	A small cl. of S. stars, not very rich.
88	1799 Dec. 28	46 (ξ) Persei	*p*	27	13	*n*	1	29	2	A cl. of coarsely sc. L*ſt.* about 15′ diam.

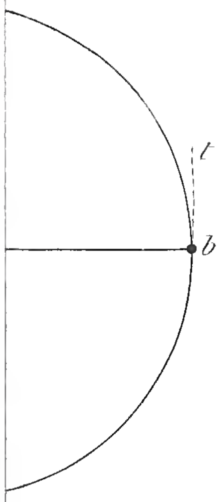
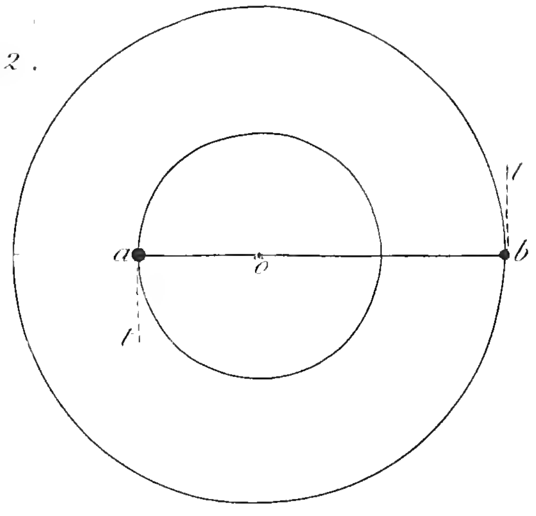
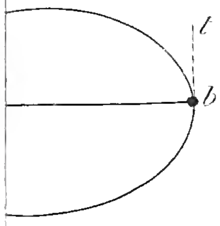


Fig. 2.





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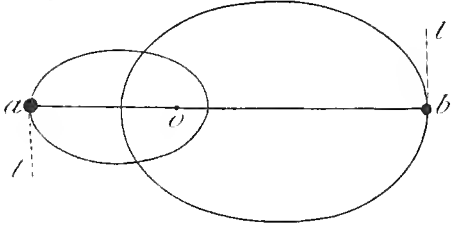

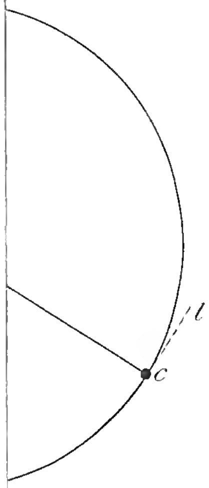

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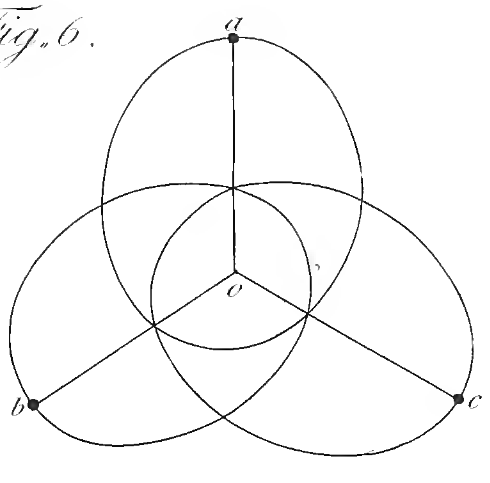

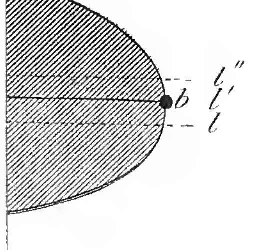

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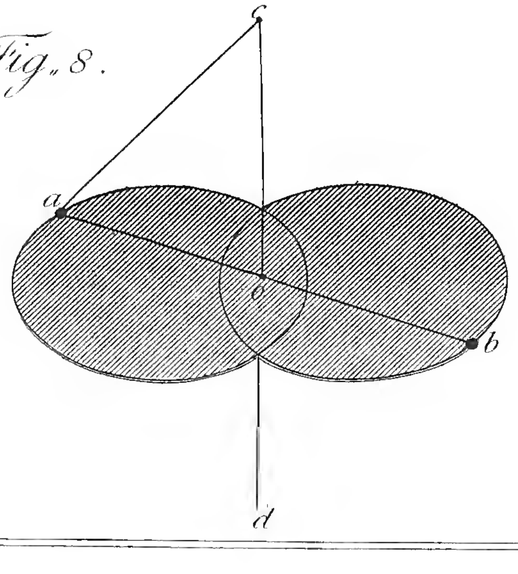

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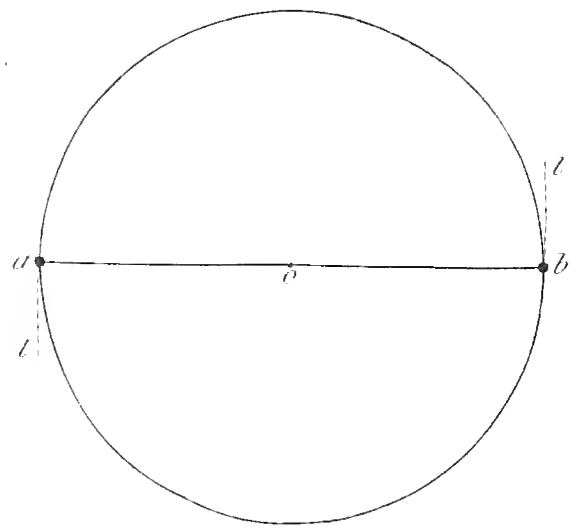
Philos. Trans. MDCCCII. Plate XVI. p. 528.

Fig. 1.



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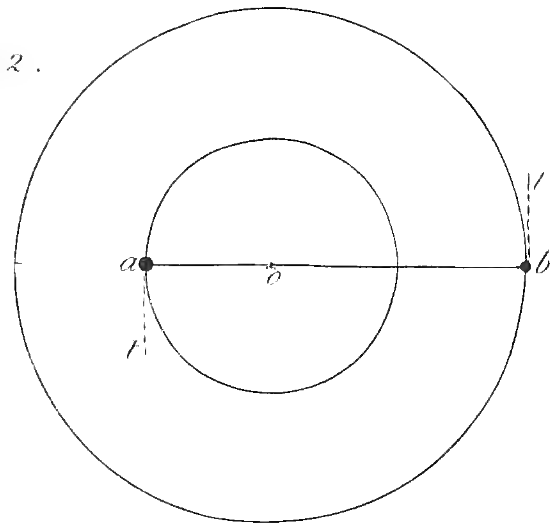
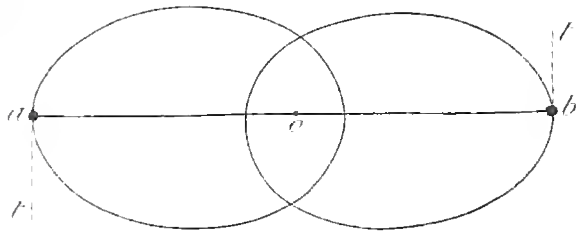


Fig. 3.



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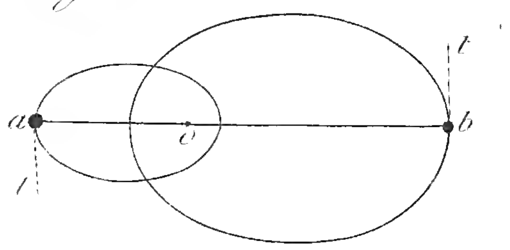

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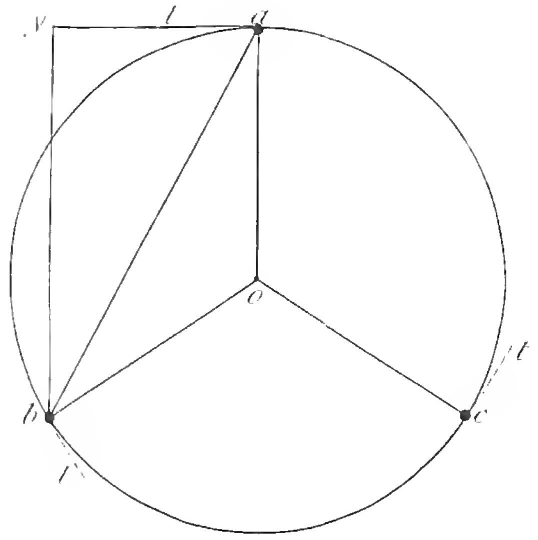

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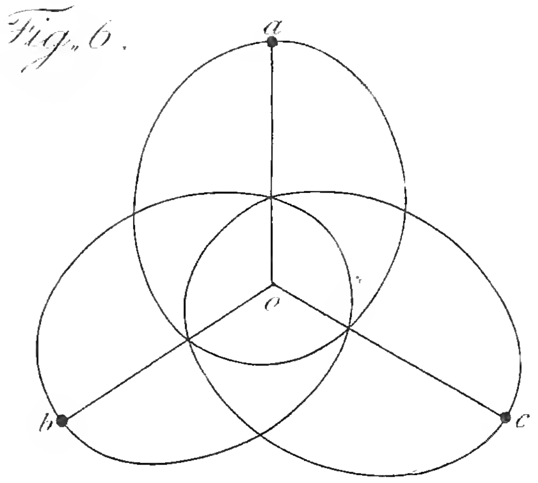


Fig. 7.

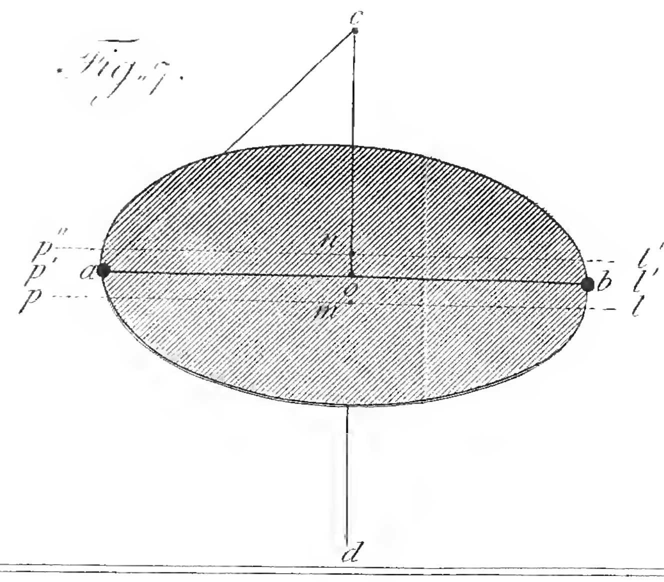
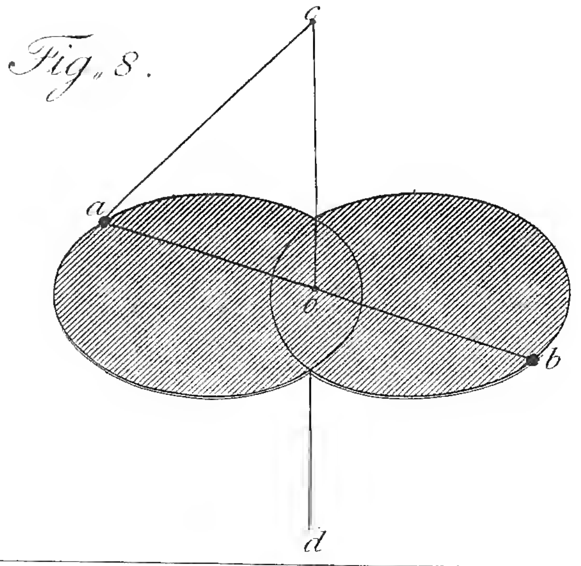


Fig. 8.



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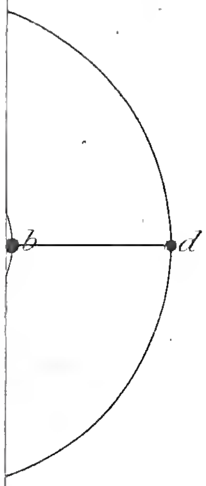

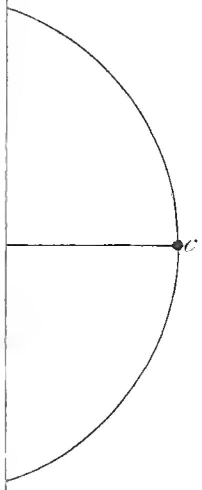

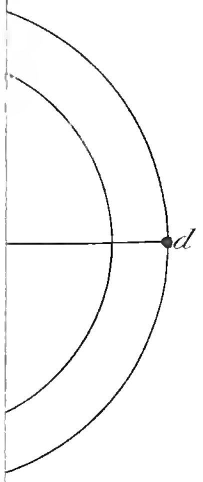

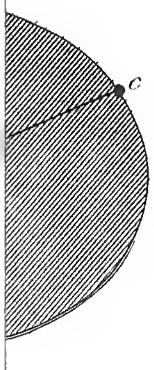

Fig. 10.

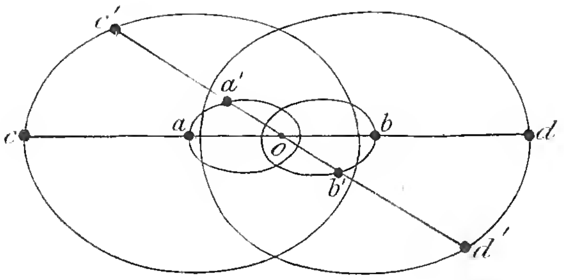

Fig. 12.

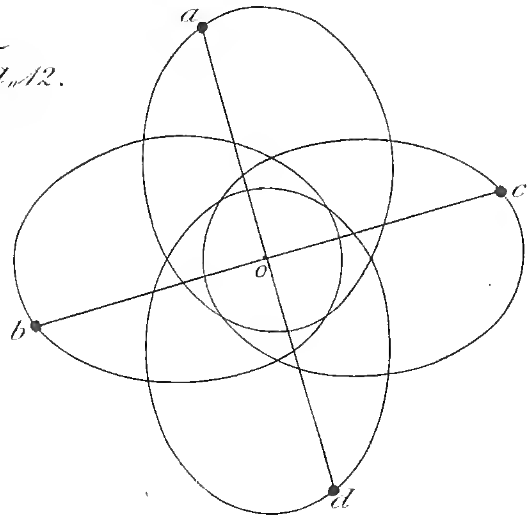

Fig. 14.

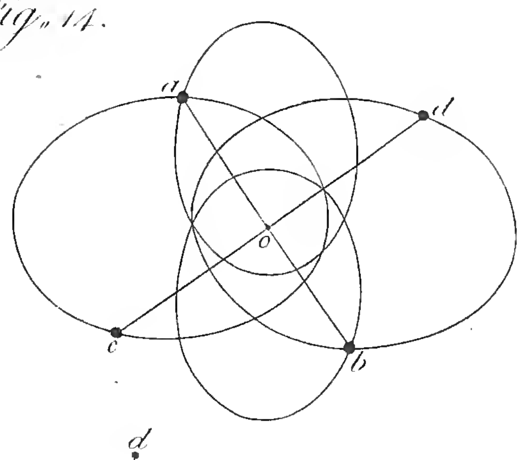

Fig. 16.

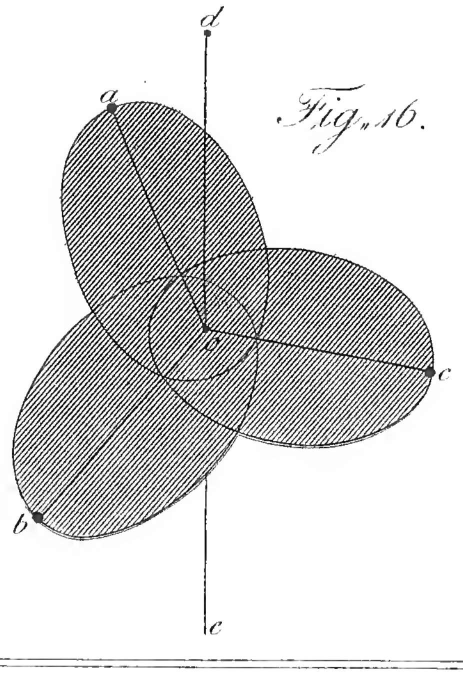

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Fig. 9.

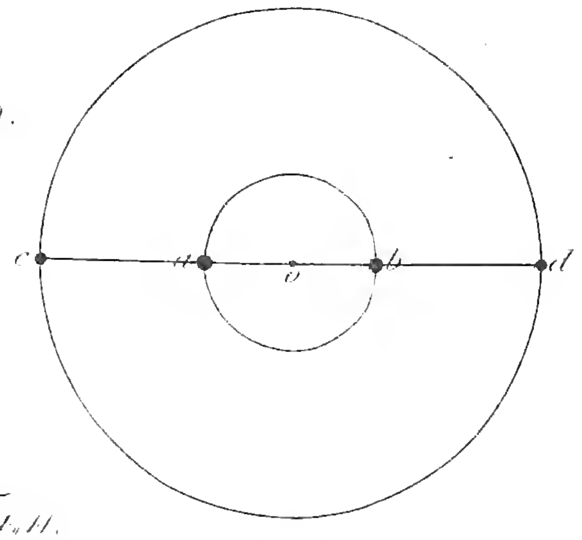


Fig. 10.

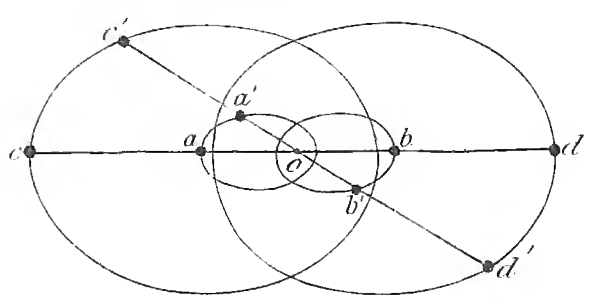


Fig. 11.

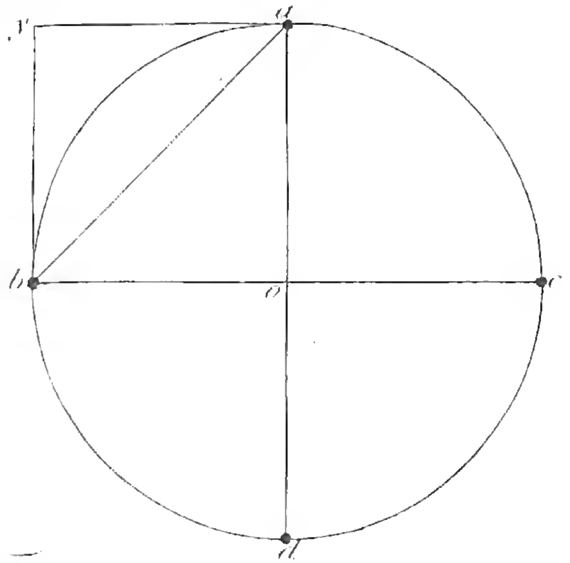


Fig. 12.

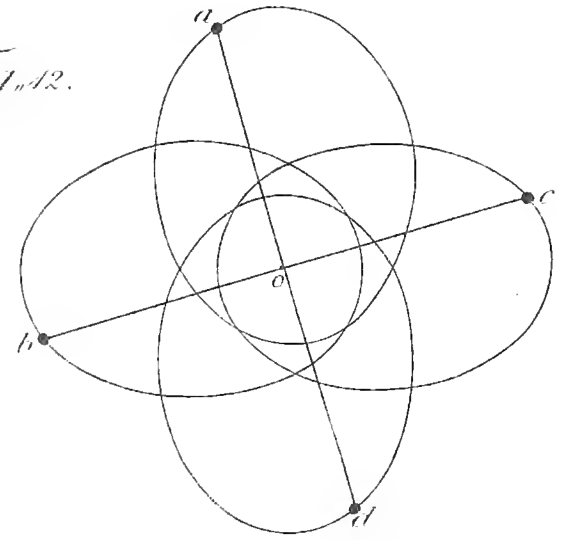


Fig. 13.

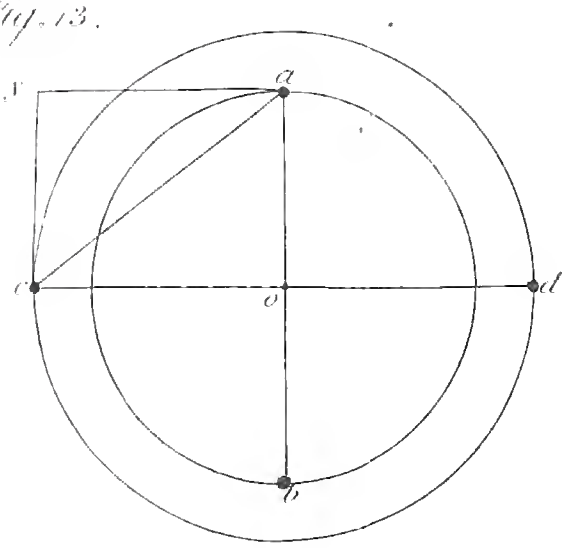


Fig. 14.

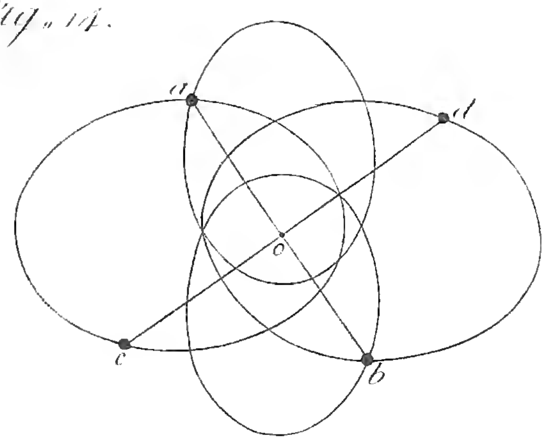


Fig. 15.

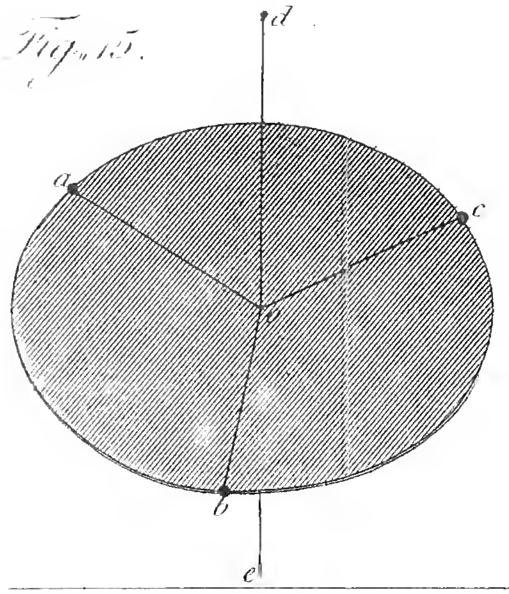
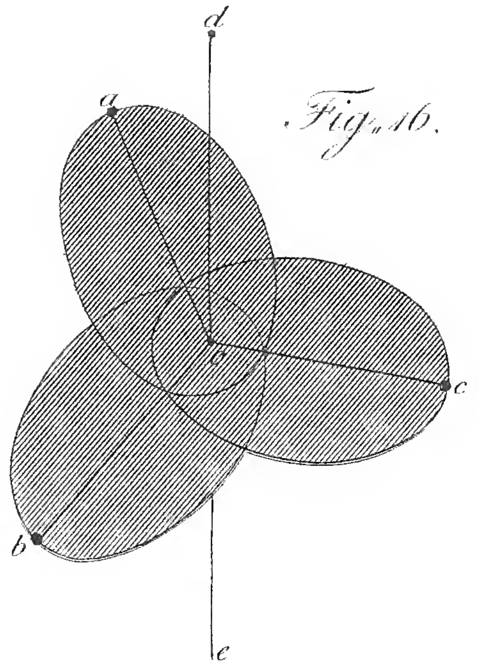


Fig. 16.



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	De' Benificamenti delle Terre Pontine, da N. Nicolaj. Roma, 1800.	fol.	Abate Nicolò Nicolaj.
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INDEX

TO THE

PHILOSOPHICAL TRANSACTIONS

FOR THE YEAR 1802.

INDEX.

INDEX.

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