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DAVID BREWSTER, LL.D.

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VOL. IV.

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CONDUCTED BY

DAVID BREWSTER, LL.D.

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CORRESPONDING MEMBER OF THE INSTITUTE OF FRANCE; CORRESPONDING MEMBER OF THE ROYAL PRUSSIAN ACADEMY OF SCIENCES; MEMBER OF THE ROYAL SWEDISH ACADEMY OF SCIENCES; OF THE ROYAL SOCIETY OF SCIENCES OF DENMARK; OF THE ROYAL SOCIETY OF GOTTINGEN, &c. &c.

VOL. IV.

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No. VI.

NEW SERIES.

ERRATA, Vol. III.

Page 153, line 6, for *errors* read *terms*.
Id. *note*, line 4, for *a* read *c*.
Page 204, line 17, for *eightieth* read *sixtieth*.
Page 290, line 16, for *I made* read *made*.
Page 293, line 2 from the bottom, for *poqssium* read *potassium*.

*** Communications, Books, and Memoirs for this *Journal* to be addressed to Dr BREWSTER, to be forwarded through Mr T. CADELL, Strand, *London*, or Mr THOMAS CLARK, 38, George Street, *Edinburgh*.

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No. VIII.

NEW SERIES.

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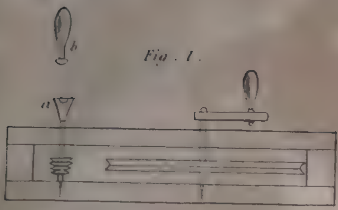

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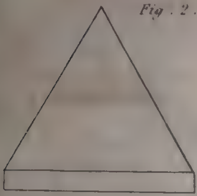

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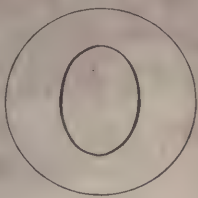

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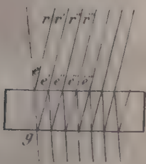

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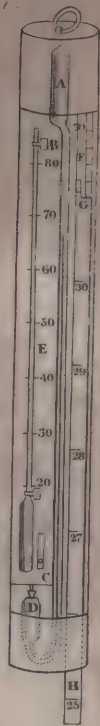

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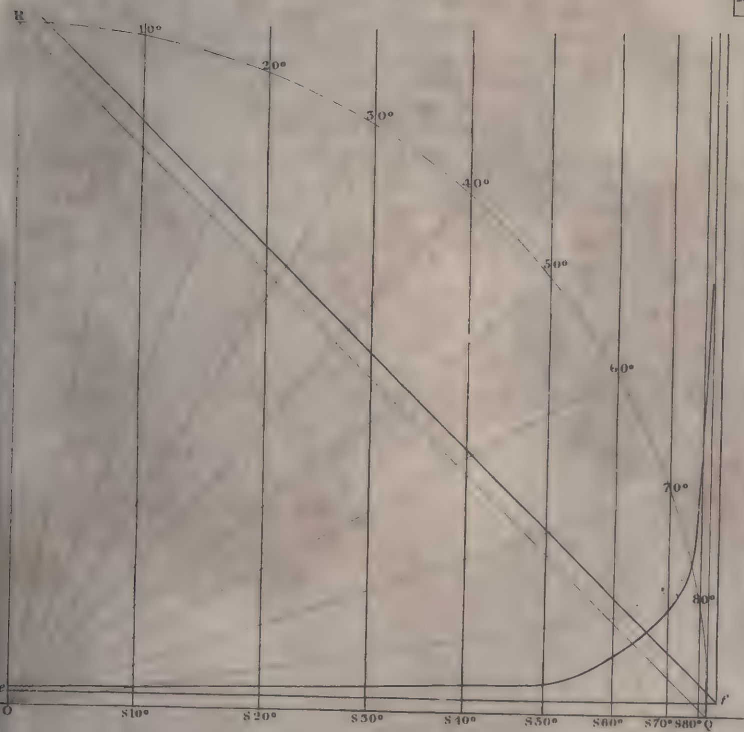

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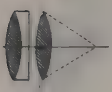

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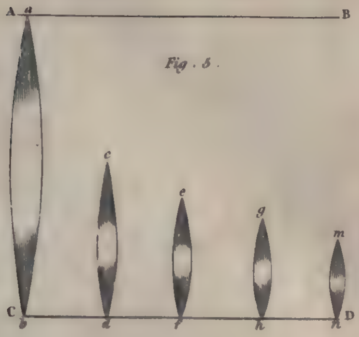

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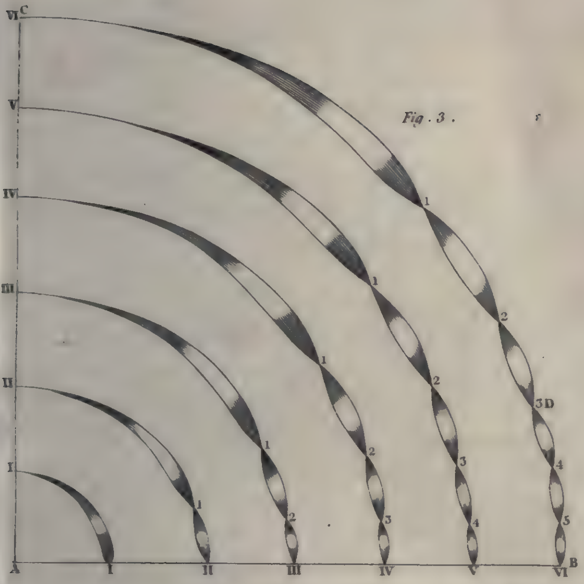

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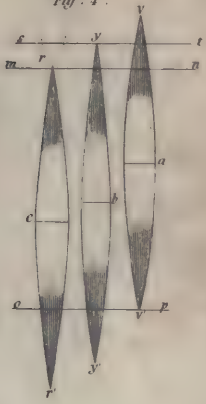

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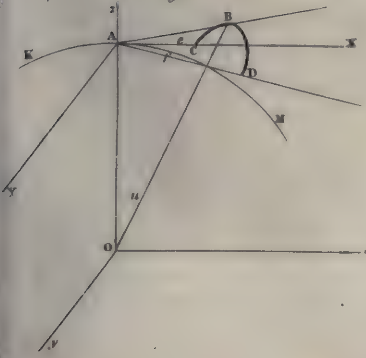

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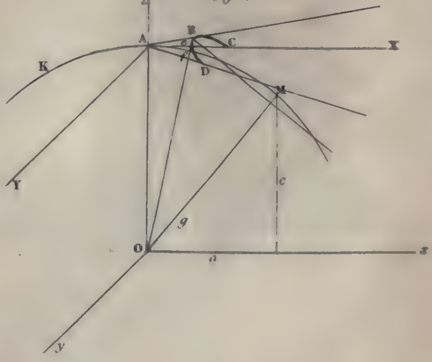

THE

EDINBURGH

JOURNAL OF SCIENCE.

ART. I.—*Biographical Notice of the late M. Le Baron Fourier*, Perpetual Secretary of the Academy of Sciences, and Member of the French Academy. By M. VIEILH BOISJOSLIN.*

SCIENCE has lost, in the person of M. Fourier, a geometer and a natural philosopher of the first order, Literature, a writer of superior talent, and France, one of the men who have done her most service in high situations, and who have honoured her most by their labours and their discoveries.

It is not his eloge that we propose here to give; this task, or rather this honour, can belong only to such of his colleagues as have followed him in his career; they also are able to appreciate his genius. We are desirous only, in this notice, to give details purely biographical; and those which we are about to present, having been collected in conversation with the illustrious author, in that of his friends, in the perusal of his works, and of the printed and manuscript writings which have been intrusted to us, will still receive a powerful interest from the subject to which they relate, and from their great accuracy.

* This interesting notice, which we have translated from the *Revue Encyclopedique* for June 1830, p. 552, is, with the exception of some changes and additions, the same as that which has been furnished by the author to the *Biographie Universelle et Portative de Contemporaines,* a work of which he is the editor.

Jean-Baptiste-Joseph Fourier, born at Auxerre, on the 21st March 1768, was descended of a family originally from Lorraine. His grand uncle, Pierre Fourier, reformer and general of the order of Regular Canons, did honour to the clergy by his great virtues, and instituted a congregation of women, adding to their three vows a fourth, which was not the least respectable, and certainly the most useful, that of instructing gratuitously the children of the poor. Several houses of this order have been preserved in France, and particularly in the capital.

M. Fourier was placed, when very young, at the Military School of Auxerre. He exhibited early a great degree of intelligence, and went through his classes with such rapidity, that he completed his course at the age of thirteen. It was then that he began to devote himself with ardour to the study of mathematics. This study, however, did not prevent him from pursuing literature, and he seemed to anticipate that literature, as well as science, might prove to him a source of distinction. Before he had reached his eighteenth year, he made several important mathematical discoveries, which are contained in a memoir, in which competent judges recognized the precocious genius of Pascal. About this time he was appointed professor of mathematics in the Military School at which he had been educated. A few years afterwards, when the Normal School was instituted at Paris, M. Fourier was sent to it by his department as one of the professors the most capable of cultivating the philosophical part of the sciences. It was soon found to be necessary to divide the auditory into several sections, for the purpose of scientific conversation among the pupils, and M. Fourier was chosen one of the directors of these conferences. More recently the *Central School of Public Works*, afterwards called the *Polytechnic School*, was organized on a permanent basis; Lagrange and Monge nominated Fourier one of the professors of this institution, for which Europe has so much envied France, and in which the sciences were then taught by those very persons who had extended their boundaries. The easy and graceful elocution of the young professor, the urbanity of his manners, the interest which he shed over science by the profound ideas with which he enriched his lectures, and the philosophical manner in which he presented them, made him generally beloved and respected by his pupils.

It was about this time that the useful and glorious conquest of Egypt had been planned in silence. The great man who directed this memorable expedition was anxious that war should become the means of civilizing conquered nations, and the Commission of Egypt was organized. The varied knowledge and talents of M. Fourier made him appreciated by the government. He was placed in the list of Savans who were to accompany General Bonaparte, and he was directed at the same time to prepare the pupils of the Polytechnic School who were to be joined to them. M. Le Comte de Chabrol, now prefect of the department of the Seine, was one of the pupils then named. This circumstance could not but have some influence over the career of this learned administrator, and, if this were the case, it would be a title which M. Fourier had long ago acquired to the gratitude of the city of Paris. The literary life of M. Fourier is intimately connected with this distant expedition, the object of which was then unknown, and which has become a celebrated epoch for the arts and sciences, as well as a brilliant episode of glory for our arms. After the submission of Cairo, the *Institute of Egypt* was created. M. Fourier was comprised in it. Experience had shown the necessity of establishing in learned societies perpetual secretaries. The Institute proceeded to this nomination, and M. Fourier was unanimously elected. On several occasions he presented important memoirs to this Institute; but political cares were soon mingled with the labours of the philosopher; M. Fourier was chosen Commissary of the French Army to the divan formed of the principal Ulemas of the town of Cairo and the provinces, after the prudent severity of the General-in-Chief had calmed the turbulence of the insurgents in the capital. Bonaparte had neglected no means for keeping up useful and familiar relations with the inhabitants, and the art of communicating with men, which M. Fourier possessed in a high degree, rendered him highly qualified for establishing a union between the civil administration and the army. The General-in-Chief at this time set out to counteract the great plot which was then organizing against him in Syria. M. Fourier was retained at Cairo. During the absence of the supreme chief the power of the administration increased, and, as M. Villemain remarked, the perpetual secretary of an academy found himself almost the governor of one-half of Egypt. Some time afterwards, the administration of justice was also confided to M. Fourier, and the miseries of war were then assuaged by the benefit of laws.

In quitting the army to return to France, Bonaparte had, with much foresight, left all the necessary orders for facilitating the noble excursions which the zeal of the French philosophers was about to resume in Upper Egypt. He had divided these ardent explorers into two sections, and had seen the necessity of appointing a chief for each of them. M. Fourier was elected one of these chiefs. Hitherto the French Savans had been but seldom able to advance into the southern provinces of Egypt. Victory having now opened this country to them, they visited more freely the magnificent ruins of Thebes, and each of them took a part in those discoveries which we may call conquests over the enemy; since, according to the expression of M. Fourier himself, they were made in perilous journeys, where the geometer, the artist, the pupil of Buffon, calculated magnitudes, designed monuments, and observed nature under favour of a victory, or during the interval of two engagements. They ascended the course of the Nile, and visited the mysterious island of Elephanta. It was in this celebrated voyage that M. Fourier collected on the spot those lively impressions, of which he afterwards gave so animated an account. If his zeal was surpassed, it was only by that of the indefatigable Denon, but in general, no person gave more effectual assistance than he did to the compilation of the great work on Egypt.

He conducted with no less boldness the high functions, the important duties which he had to discharge in the army. When Morad, dreading the departure of the French, offered to treat with Kleber, through the medium of his wife, the beautiful Scitty Nefiçah, whom this Bey had carried off from Aly, it was M. Fourier who concluded the treaty with that celebrated woman; an alliance which brought about that peace which was so much desired, but which lasted for too short a time. It was he also who expressed the sorrow of the heroic army of Egypt, when the sword of a fanatical assassin pierced the unfortunate Kleber. From the summit of a bastion M. Fourier, celebrated, in the presence of the whole army, the conqueror of Maestricht and Heliopolis. Upon uttering these words, "I take you to witness, ye intrepid cavalry, who ran to save him on the heights of Coraim," the army was affected, and the orator, partaking in the common grief, stopped,—interrupted by the noise of arms, and the lamentations of so many soldiers in tears. A few months after this sad solemnity the fate of General Dessaix, who had recently left Egypt, was known at Cairo. The orator of the army of the East had to celebrate the memory of this great captain, in the very place where he had honoured the remains of Kleber, and on this occasion too his eloquence rose to the height of his subject.

Detained in Egypt to the very end of the expedition, M. Fourier at last returned to France, with the small number of philosophers and warriors who had escaped from this hazardous expedition. After a conquest so daring, and after so many combats of glory, there remained the labours of science,—the map of the country, and the copy of its monuments. It was at least desirable that none of the valuable marks of our expedition to Egypt should be lost. But there was reason to dread that each individual savant would wish to make a separate use of what he had himself collected, and that the whole mass of the results would thus be disjointed. M. Fourier, when summoned before the First Consul on the subject of the portfolios brought from the East, availed himself of this circumstance to direct his attention to the subject. It was then agreed that all their riches should be collected, and that the work on Egypt should be published at the expence of the government. The Savans, to whom this charge was entrusted, unanimously selected M. Fourier to delineate the frontispiece of the temple, which they were about to rear to the glory of science and of the country.

The First Consul was desirous of rewarding an individual, who, without soliciting any distinction, had rendered him such eminent services. He wrote to Berthollet on the 18th Pluvioise 1801, to learn if the prefecture of the department of Isere would be agreeable to M. Fourier. He was appointed Prefect of Grenoble on the 2d January 1802. He was included in the legion of honour as soon as it was created, and he received the title of Baron, with a pension, in 1808. His administration, during the first fourteen years of it, does not seem to have suffered from his devotion to science; on the contrary, it was benefited by it. Great public works were completed; the draining of the marshes of Burgundy, which infected more than forty communes, was executed, and this vast and salutary enterprise, so often and so uselessly attempted, was terminated by the influence of an active administration full of wisdom and of firmness.

In the midst of such important functions M. Fourier at last completed the difficult task which had been entrusted to him. During the first eight years of his residence at Grenoble he wrote the discourse which forms the historical preface to the great work on Egypt, an eloquent and well arranged exposition, written, to use an expression of M. De Fontanes, with the graces of Athens, and the wisdom of Egypt, and in which he has recorded with a bold pencil the events of history, the observations of science, and political views. It is in this discourse, which is reckoned one of the finest monuments of the French language, that the author, invoking at the same time the authority of ages, and the speculations of genius, has thrown a bright light upon the enterprises which Europe may undertake for civilizing the East, and that we meet with some of those elevated views to which an illustrious author has more recently imparted new energies.

The Institute of France having proposed in 1806 the important and difficult problem of determining the laws of the propagation of heat in solid bodies, M. Fourier discovered new methods of resolving it; he verified them by very curious experiments made with the most accurate instruments; and in 1807 he gave a complete solution of this difficult question. His memoir obtained the prize, and placed its author in the very first rank of philosophers. M. Fourier sent to the institute a second memoir on the same subject, and these two papers form the substance of the great work which he afterwards published.

In 1815, when the Emperor Napoleon landed in France, and advanced towards Grenoble, M. Fourier, by the advice of the prefect of the Var, published, on the 5th March, a proclamation for preserving order, and causing the government of the king and of the constitutional charter to be respected. On the arrival of the conqueror he quitted the city, but Napoleon caused him to be brought back to Grenoble. In this difficult position M. Fourier was exposed to imminent danger; but he was saved from it by the affection of the inhabitants and by the tact of the emperor, to whom he was presented in the midst of an immense concourse of people, and who appointed him, on the 12th March, to the prefecture of the department of the Rhone. The principal inhabitants of Lyons, who knew all the advantages which might be expected from so able a magistrate in such critical times, anxiously desired that this office should be conferred upon him. M. Fourier found it impossible to decline it, but the principles of justice and moderation which always guided his conduct did not permit him to continue in this situation. He refused in writing to carry through the measures which the minister required of him, and he was recalled by a decree of the 12th May. Napoleon afterwards assured him that he understood his conduct and approved of it.

Thus left to himself, our celebrated geometer came to reside in Paris. In 1816, he read to the Academy of Sciences a memoir on the vibrations of elastic surfaces, which contained several new integrations of equations belonging to dynamics. In the same year, the Academy admitted him a member; but Louis XVIII., misled respecting his political conduct, refused to ratify his election by the royal sanction. In 1817, however, when he was a second time elected, the king, after an attentive examination of all the facts, approved of the election. A short time afterwards, M. Fourier was chosen perpetual secretary of the Academy for the Mathematical and Physical Sciences. The Royal Society of London and other academies hastened to do themselves the honour of admitting him into their body

In 1820, he added to his discoveries the solution of a very complicated question: It consisted in forming differential equations which express the distribution of heat in fluids in motion when all the molecules are displaced by any forces whatever, combined with changes of temperature. These equations belong to general hydrodynamics, and we are indebted to the author for having completed this branch of analytical mechanics.

It was in 1822 that this great geometer gave to the learned world his admirable treatise entitled *Theorie Analytique de la Chaleur*. The preliminary discourse, and particularly a passage in this discourse which has specially struck us, and which has perhaps not been sufficiently noticed, would alone be sufficient to place M. Fourier in the number of those philosophical geometers who are destined to wrest from nature some of her more hidden secrets. Before his time the effects of this universal element were unconnected with mechanical theories. The fixed laws which regulate its distribution are still unknown; valuable observations had been collected, but only partial results were known, and not the mathematical demonstration of laws which embrace them all. The illustrious author succeeded in discovering them, and exhibiting them in analytical formulæ, so that this theory will henceforth form one of the most important branches of general physics. His principles are deduced, like those of rational mechanics, from a few primordial facts, which geometers do not consider the cause, but which they admit are the results of general observation.

The principal results of this theory are the differential equations of the motion of heat in solid or fluid bodies, and the general equation which relates to the surface. These equations, like those which express the vibrations of sonorous bodies, or the last oscillations of fluids, belong to one of the newest branches of mathematics, and one which it is of much importance to improve. After having established these differential equations, it was necessary to integrate them, which consists in passing from a common expression to a proper solution subject to all the given conditions. This difficult research required a special analysis, which M. Fourier has created, and which is founded on new theorems, the nature of which we cannot here explain. It may be sufficient to say, that the method derived from it leaves nothing vague and indeterminate in the solutions, and that it conducts them to the last numerical applications, a necessary condition of every investigation, and without which we should only arrive at useless transformations. It deserves to be remarked, that these theorems are applicable to questions of general analysis and of dynamics, the solution of which has long been a desideratum. We may easily judge how important this new theory ought to be in the physical sciences, and in civil economy, and how great may be its influence on the progress of the arts which require the employment of the distribution of heat.

The theory of heat has also a necessary connection with the system of the world. A very important class of phenomena are produced in this system by the laws which regulate its distribution. It would be impossible to notice here all the unexpected results at which M. Fourier has arrived. We shall mention only some of the questions which he has been able to solve.

Why does the temperature of the earth cease to be variable at so small a depth in relation of the radius of the globe? What time ought to elapse in order that the climates may acquire the different temperatures that they have at present, and what causes may still change the mean heat? To what cause ought we to ascribe it that the globe has not entirely lost its own heat, and what are the exact laws of its expenditure? Independently of the two sources of heat in our globe, the one fundamental and primitive, and the other due to the presence of the sun, is there not a more universal cause which determines the temperature of the heavens in that part of space which the solar system now occupies? In this question, which is entirely new, what are the consequences of an exact theory? How can we determine this constant value of the *temperature* of space, and deduce from it that which belongs to each planet? If we add to these leading questions those which depend on the properties of radiant heat, and several others not less important, we may form an idea of the admirable investigation of this eminent philosopher.

The solution of these problems, which required the genius of a Newton, a La Grange, and a La Place, has shown that the temperature of the planetary spaces in our system is 58° below zero of Fahrenheit, the same nearly as that at the earth's poles, which our author's theory has also determined. We may now understand why the temperature of our globe is constant within certain limits, and how it happens that cold and heat do not become dangerously intense during the alternation of day and night, and during the variation in the earth's distance from the sun. We learn also that the incandescent mass which forms the interior of the globe ought to be about twenty leagues below its surface, and that the heat which emanates from it can no longer exercise any influence on the earth's temperature. Thus has disappeared that theory of the cooling of the surface of our globe to which the presence of a central fire gave an appearance of truth. The calculus has rectified all such errors, and those enormous planets which are situated at the confines of our system are found only to have a temperature of —58° of Fahrenheit.

Having computed the law of the cooling of our globe, originally in a state of incandescence, and having shown that ages were necessary to bring it to its present state, our readers will readily see how such questions bear upon many points of cosmology.

Of late years M. Fourier was occupied with very interesting experiments on the transmission of heat across different bodies, and the results which he obtained were conformable to his anticipations. Among other results, he found that the heat which traverses several plates of different substances superposed, varies according to the order of superposition, external circumstances remaining the same. Thus, in placing a sheet of copper between the skin and a piece of cloth, the transmission of the heat is facilitated; when the copper is placed between two pieces of cloth, the transmission is not changed; and when placed between cloth and marble, it is diminished.

For these experiments M. Fourier contrived his *Thermometer of Contact*,* with which he has made a great number of interesting experiments.

M. Fourier has likewise made several important improvements in the calculation of probabilities, some of which are contained in his work *On the Mean Results and on the Errors of Measures.* In an excellent work on the general resolution

* See the *Edinburgh Encyclopædia*,—Art. THERMOMETER.

of equations, this subject has been treated in a manner entirely new, and there will be found among his manuscripts, reflections as curious as they are philosophical, on the difficult points of elementary algebra, and on the theory of parallel lines.

It is not easy to understand how, in the midst of such profound studies, he found it possible to devote himself to the labours of literature as well as of science. M. Fourier often gave proofs of the possibility of this double effort, and it was always executed with an admirable pliability of talent. The fine eloges which he pronounced as the organ of the Academy of Sciences have placed him beside Fontenelle, Condorcet, and Vicq-d'Azyr. As ingenious as the first, but with more simplicity, he raises himself like Condorcet by the generality of his ideas and the universality of his knowledge, and he approaches the last by the harmony, the elegance, and the animated movements of his style.

In 1827, the French Academy wished to pay the debt of literature to this illustrious philosopher, and on the 17th April he was unanimously admitted into the Academy of Sciences. In the same year, after the death of La Place, M. Fourier succeeded him in the council for the improvement of the Polytechnic School, and in 1828, after the fall of Villele, he was named a member of the commission appointed to report to the government on the distribution of the prizes granted to science, literature, and the fine arts; and afterwards president of the commission of statistics established by the Minister for the Marine and the Colonies. He had refused the place of *Directeur General de la Libraire*, offered him by the new minister, in which he would have done much good, and it was on this account he regretted that his occupations and his health had not permitted him to accept of it.

It was in the middle of such labours and studies, and of duties fulfilled with rigorous exactness, that M. Fourier found time to give proofs of the most cordial friendship for his colleagues, and to receive and encourage every person who was recommended to him. Nothing could equal the charm of his conversation, at once gay, spiritual, and full of grace. These estimable qualities, and the goodness which he showed in his social relations, attracted to him as many friends as there were admirers of his genius.

He was for several years attacked with a nervous angina. This infirmity, recently aggravated by a fall, put an end to his life rather suddenly, on the 16th of last May, 1830, in the sixty-third year of his age. Philosophers will hasten to characterize what he has done for the progress of the sciences, which owe to him profound calculations and discoveries, that will render his name immortal. His obsequies were celebrated on the 18th May, in the church of Saint Jacques-du-haut-Pas. This mournful solemnity was attended by numerous deputations from the Institute and the Polytechnic School, by the members of his family in profound sorrow, by his friends and acquaintances, by a great number of academicians, by philosophers and men of letters, whom respect or sorrow had assembled round the grave of an illustrious academician, an excellent parent, and a sincere friend of the public liberties of his country. The pall was held by M. Geoffroy de Saint Hilaire and Bontemps Beaupre of the Academy of Sciences, M. Felitz, director of the French Academy, and Sylvestre de Sacy, of the Academy of Inscriptions. The procession walked to the cemetery of the East. Several discourses were pronounced over the tomb by M. Sylvestre, M. Cuvier, and by MM. Felitz, Girard and Jomard.

The following is a list of the principal writings of M. Fourier :—

1. *Mémoires sur la Statique*, containing the demonstration of the principle of virtual velocities, and the theory of Momenta, printed in tom. ii. of the *Journal de l'Ecole Polytechnique*, 1798.

2. *Mémoire sur la Resolution Generale des Equations Algebriques*, presented to the Institute of Egypt.

3. *Discours Préliminaire, servant de Preface Historique à la Description de l'Egypte.* Paris, 1810. 1 vol. folio.

4. *Rapport sur les établissemens appelés Tontines.* Paris, 1821. 4to.

5. *Theorie Analytique de la Chaleur.* Paris, 1822. 4to.

6. Several Reports on the Progress of the Mathematical Sciences. Paris, 1822 to 1829.

7. *Eloge Historique de Sir W. Herschel.* Paris, 1824. 4to.

8. *Eloge de Delambre.* Paris, 1823. 4to.

9. Two memoirs entitled *Theorie du mouvement de la Chaleur dans les Corps solides*, printed in the *Memoirs of the Institute*, tom. iv. and v. 1824 and 1826.

10. *Notice Historique sur la vie et les Ouvrages de Breguet.** 1826. 4to.

11. *Mémoire sur les Temperatures du Globe Terrestre et des espaces planetaires.* Paris, 1827. 4to.

12. *Mémoires sur la distinction des Racines Imaginaires, et sur l'Application des Theorems d'Analyse Algebrique aux Equations transcendentes qui dependent de la Theorie de la Chaleur*, printed in the *Memoirs of the Institute*, tom. vii. 1827.

13. *Eloge Historique de M. Charles.*

14. *Eloge Historique de M. de La Place.* Paris, 1829.†

15. *Mémoire sur la Theorie Analytique de la Chaleur*, printed in the *Memoirs of the Institute*, tom. viii. 1829.

16. *Analyse des Equations Determinées.* The author had printed the first six sheets of this important work, which he did not live to finish. "The work," says M. Navier, "contains a preface, an introduction containing the principal points of algebraic analysis, which serve as the basis of the work;—a synoptical exposition, containing a detailed explanation of the subjects which ought to be treated in the work, and from which we learn that it was to be divided into seven books. The manuscript of the exposition, and of the two first books, was found complete, and to all appearance ready for press. This is the part that ought to be published first and separately. There is every reason to believe that the materials of the last books exist among the author's papers, and that these new researches will not be lost."

Art. II.—*On various Improvements in the casting, working, &c. of Specula for Reflecting Telescopes, with sundry Hints to Amateur Opticians.* By R. Potter, Esq. Junior. Communicated by the Author.

This essay on the grinding, polishing, &c. of certain metals

* See this *Journal*, No. xiv. p. 201.

† See this *Journal*, No. ii. New Series, p. 193.

for the specula of reflecting telescopes, being intended only for the information of amateurs, if it should fall under the perusal of one engaged in the pursuit as a trade, before he has the opportunity to say there is nothing in it worth knowing which was not already known by the working opticians, I say, I do not pretend to teach those who have been initiated into the mysteries of the craft by a regular apprenticeship. The great excellence of their workmanship, and the inability of any amateur to instruct them, is duly proclaimed *by the superiority* of their reflectors over the achromatics produced by their brother artists at the present day. My intention is only to add my mite to what has already been given in print by Sir Isaac Newton, Smith, Mudge, Edwards, Herschel, Lord Oxmantoun, &c.—Amateurs all,—and, though not finding a place in this list for any name from those, who, having practised the art as a means of livelihood, must have had a tenfold opportunity and experience, I am not, nevertheless, inclined to blame the fraternity, Short and Company, but rather charitably to believe that they have had nothing more to communicate than what amateurs had before taught them, besides the dexterity acquired by experience, which could not be transferred by ink and paper.

In addressing myself, therefore, to amateur speculum and glass-grinders, I exhort them not to let want of success, adequate to their wishes, check their perseverance. The art is one in which they must not expect to attain perfection in their first attempt. I have heard of many who have commenced, but of very few who have produced, even tolerable specula from want of sufficient practice.

With myself, it has required the experience of nearly a dozen years in the largest part of my leisure hours from business, but in which I include about two years spent in the study of chemistry, which I undertook when I found I could not get so fine a polish on metal as I wished, with the putty (cream coloured oxide of tin) and coleothar of vitriol, (red oxide of iron,) which I purchased in the shops, and could not succeed in producing a good polishing powder of my own making, according to the directions given by Edwards. But after this course of study, during the greatest part of which I had the good fortune to be pupil to the chemist who has done more for the science than any other man without exception, when my attention was again drawn to my old hobby, I undertook the preparation of the polishing powder with very different results.

For many years I had continually asked myself—what is polishing? Is it only grinding, as some have supposed, carried to as fine a pitch as we can? The answer which often suggested itself was, if it is only fine grinding, the polish so produced artificially must at best be only an approximation, and infinitely inferior to the natural polish which of itself forms on the surfaces of glass, liquids, &c. From what I read on the polishing of precious stones, and from what I had seen, namely, that what would polish glass would grind metal, I at last concluded that polishing is a totally different process from grinding; that in the latter the material must be *harder* than the substance to be ground; and that all approximation towards polishing by it must at last be mere approximation; and that to produce a good polish, the polishing material must be of a nature *not harder* than the substance to be polished. Hence, in grinding, we proceed comparatively rapidly with a quantity of the emery, &c. running loose between the tool and the body we grind; but in polishing, the powder being softer. we never make any good progress until there is a close contact and strong adhesion between the surface of the polisher and the lens or other body in work. Thus, though the powder is too soft to penetrate and tear away the substance of the glass, &c. yet when this adhesion takes place, successive layers of the surface, if I may use the expression, slide away, and it is left the smoothest possible. We find also, that not only the polishing powder, but the polisher itself, or rather the surface of the polishing tool, must be of different hardness, according to the nature of the substance to be operated upon. Accordingly, diamond is polished on a surface of iron or steel, with diamond dust imbedded in it and a little oil, the various precious stones on laps of different metals, according to their hardness; glass on pitch, with a large proportion of rosin melted with it, or woollen felt pressed into a firm bed together with the powder; *hardened* steel and speculum metal on mixtures of pitch and rosin, but that for the latter with a smaller proportion of rosin.

These views led me to prepare the red oxide of iron in the manner hereafter to be described, so that it might have the same relative hardness to speculum metal that the putty powder of the shops has to glass; justly expecting, that by means of it as fine a surface might be obtained on the former as is easily prevented on the latter, and thus remove the great obstacle which I believed prevented reflecting telescopes, having only the liability to errors of workmanship on two surfaces, which achromatics have on four, besides other disadvantages, from showing a more decided advantage over them.

As I here only intend to give what I believe I have learned of better methods than are already in print, I shall point out to the amateur who is only commencing the pursuit, the works where he will meet with information on the subject, and afterwards submit my own observations. He will naturally refer first to the *Encyclopedias*, and in several of them he will find the greatest part of what he seeks under the heads—telescope, mirror, speculum, grinding, polishing, &c.; for specula, he may refer to the original paper of Dr Mudge, published in the *Phil. Trans.* for 1777, or Mr Edwards's essay, republished in the *Nautical Almanack* for 1787; but, above all, he should peruse Sir Isaac Newton's account, given in his Optics, of the method he followed for the first reflecting telescopes ever made. He will there find the process of polishing described, almost the same as practised at present, in a manner at once instructive and complete, yet concise.

As most amateur telescope-makers will wish to have their eye-glasses of their own workmanship, I adjoin a plan of a polishing lathe, which I have used in making the lenses of short focus in my eye-tubes, not having met with any account of a similar one. I have also found it very useful in *grinding* the small oval specula for the Newtonian telescope, and in *grinding and polishing* small specula for Baker's original reflecting microscope, which I can here say, for opaque objects, is a very effective instrument; and I may perhaps, through the medium of this *Journal*, some day lay before the public the plan on which I have found it most convenient to fit it up.

The frame of the lathe is of inch thick deal, the wheel being about twelve inches in diameter, See Plate I. Figure 1, and the speed pulley having several grooves in it to alter the velocity at will. Being thus of sufficient weight, it may be used conveniently on the knee in a sitting posture, while with one hand we turn the handle, and with the other manage the working of the lens in the cup. At *a*, screwed on the end of the spindle of the pulley, is shown the small cup in which the lens is to be ground, it being cemented firmly to the end of a wooden handle as at *b*, with sealing-wax or gum-lac. It is to be kept continually moving across the cup, backwards and forwards, to preserve a true spherical figure, which the rotatory motion of the lathe would otherwise soon spoil *in convex lenses.* In the operation of polishing, the cup having to be coated with pitch, I have used one of longer radius than the one I grind in; thus glasses of $\frac{1}{10}$ inch focus I have polished in the grind-cup for those of $\frac{1}{8}$ inch focus, those of $\frac{1}{8}$ inch in that for those of $\frac{1}{6}$ inch, &c. and pursuing this method judiciously, I have no doubt truer lenses of this small size are to be made than can be by hand alone, and also very much quicker. In grinding specula it is very useful when the metal is tender, which is often the case when they are otherwise good, and these may be ground with the lathe owing to the swift motion, while they would tear up on the face with the emery if worked by hand. With a lathe of the above dimensions, a surface of six to seven square inches may be ground, but it must then be worked on the hones, and polished by hand, the power of the lathe being insufficient for polishing a surface more than one to $1\frac{1}{2}$ square inches of either glass or metal. Though the rotatory motion of the lathe would spoil the figure of a *convex* lens or speculum, it may be made use of to give a figure approaching to an ellipse or parabola in *concave* ones, and is the method I have pursued in the metals for the microscope, which have a great diameter in proportion to their focal length.

In the casting of specula, my experience has been confined to casting about forty to fifty small specula of the oval ones, or the round ones for the miscroscope of $\frac{3}{4}$ oz. to $1\frac{1}{2}$ oz. each; but I can attest that with the most brilliant metal, viz. $14\frac{1}{2}$ parts tin to 32 copper, it requires considerable care and attention to get perfect castings even of this size, of good metal, free from contraction, cracks, flaws, &c.

I have fallen upon one thing in this department within the last few months which I think important, and to attain the same effect a contrary process is generally pursued ; but I have only yet proved it upon small metals, and must leave it to others to determine how far it is applicable to large ones. It is this ; without almost the least hope of its use, I placed in the sand of the moulding box an old steel speculum for the face of an oval one to be cast upon, and the casting was thus chilled as soon as formed. Two cast at the same time in the sand alone, and intended for the microscope, were so tender and rotten as not to bear being ground ; another oval one was better metal, but had flaws in it ; but I was agreeably surprised to find, contrary to my expectation, that the one which had been chilled was the best metal I had ever cast, and so hard and compact that its surface was hardly torn up when I had rubbed off the inequalities on a sand stone, and it polished beautifully. I have cast three others in the same way since, and the metal of all has proved equally hard and good. Experience must teach us the best proportion between the weights of the chilling metal and the speculum, and when of any considerable size, I think the castings will require to be annealed like glass ; but we may conclude, that, if annealing enables the metal to bear more lateral strain, it at any rate does not give the property necessary to grind and polish well. I expect that this method of chilling, combined with Lord Oxmantown's, of soldering the brittle metal to one of more tenacity, will prove a very useful process for large metals, and recommend it to his lordship's attention.

I should advise all amateurs, who are only commencing, to get their metals cast by some skilful bell-founder. One whom I employed to cast me two of about 4lbs each, got three good castings out of seven, and I found the metal to work excellently and take the highest polish. By ordering castings of the different sizes you are likely to want, to the amount of 10 or 12lbs in weight, most founders in bell-metal will take the order at 2s. per lb. the price they get in Manchester being only about 14d per lb. for the more expensive alloy, bell-metal.

The directions should be very particularly given to the founder not to overheat the metal in the melting, and to use, according to Edwards's plan, a large jit, (or hole through which the metal is poured,) that the castings may be free from holes in the back, by the contraction in cooling, speculum metal being the one which contracts, I believe I may say, more almost than any other. From measurements of the size of a metal of about $5\frac{1}{2}$ inches diameter, compared with others of the model it was cast from, I have found the contraction to be about $\frac{1}{57}$th part of the linear dimension of the model, so that it ought to be made that quantity larger than the metal is wanted; and in concave and convex specula the radius of the curve must be in the same proportion, as it is easily proved geometrically, by similar triangles, on the supposition of the metal contracting equally in every direction, that a circular arc will still remain perfectly a circular arc, but to a shorter radius, in proportion to the contraction.

I believe the most reflective metal to be that of Dr Mudge, viz. about $14\frac{1}{2}$ parts of tin to 32 parts of copper. Taking Mr Dalton's numbers for the relative atomic weights of tin and copper, it appears to be almost exactly two atoms of copper to one of tin. The compound metal has a much greater specific gravity than the calculated one from the proportions; and this, combined with the consideration of its colour and hardness, would have led one to suspect it a binary compound of atom and atom. Of thirty-four specimens which I have weighed hydrostatically at different times, the densities have varied from about 8.6 to 8.98, with often a considerable difference between different portions of the same castings, which will give an idea that it is a difficult metal to understand the management of.

A little arsenic has certainly a great effect in hardening the alloy. It communicates to it the property of being more sonorous, and the fracture is also very different. I have only polished two small metals with this addition, and finding no advantage in it, I never use it now. I have found it by measurements not to reflect more light than the same proportions of copper and tin without arsenic. It is too hard to polish with the powder about to be described, and too soft to take the highest polish with putty, and it has also the character of being more liable to tarnish.

In the working of both lenses and specula,—when they are small, and the surface is to be flat or but slightly curved,—the form of the handle is of great consequence to insure a correct figure. In making some plano-convex lenses, I was for some time unable to polish correctly the flat side, though I tried several contrivances which I thought promised well in theory. I found the only proper form of a handle to be a cone; and for flat surfaces I have used a cone of lead, with the sloping side of about equal length to the diameter of the base, similar to Figure 2, and found it answer well.

The only correct way, as is well known, to get a true plane, is to grind together three surfaces, alternately two and two, until they are all alike, when they must necessarily be plane. It is acknowledged to be the most difficult part of the art of the working optician to produce a lens or speculum with a good plane surface; and those amateurs who undertake the Newtonian telescope for astronomical purposes,—if they find their instrument when finished will not show difficult astronomical objects well, may satisfy themselves that it is a hundred to one the greatest fault lies in the small oval speculum not being truly plane; and this may be told from the figure of the planets, &c. appearing oblong in place of round; but if the eye-glass and metals are not correctly in position with respect to each other, or are what is called out of adjustment, a similar effect will be produced. It is then of the greatest importance in this telescope to have the plane metal as true as possible. I found, however, when the surface had been ground true in the manner described above, and then worked on a hone very carefully prepared, yet it was very liable to alter some little, particularly near the edges, in the succeeding polishing process. This caused me to adopt the following contrivance:—For an oval metal of about 1 inch in breadth, and $1\frac{1}{2}$ inches in length, I have a circular tool of speculum metal cast of about $2\frac{1}{2}$ inches diameter, and about $\frac{1}{4}$ inch thickness, with a hole in the middle a little larger than the oval, as at Figure 3.

Having placed the small speculum in this hole, I make the two hot, and cement them together by running gum-lac into the vacancy between them. They may now be ground and polished as one piece, and then the oval removed by heating its back over the flame of a candle, until the cement softens. In this manner, though we grind and polish four times as much surface as we want, yet the small metal is so much more likely to prove a good one, that it is worth more than ten times the additional trouble to follow this method. It is necessary to polish immediately after the tool and speculum have been reduced to a fine and true surface on the hone; for, if left for some time, the cement is liable, from the different expansion and contraction of it and the metal, to lose its contact, and the two surfaces of metal will be found to be no longer in the same plane.

As to what I have before said on the great consequence of having a high polish, it is clear, that whatever may be the power which exists at the surfaces of bodies and produces reflection,—if the surface be rough only in such a degree that it may require the highest power of the microscope to detect it,—yet a great proportion of the rays of light will be deflected from their proper course, and produce an image, though not palpably incorrect, yet still of a dull outline, and without sharpness. This constitutes what is called a white or silvery polish, but it is what I call only a good approximation towards a polish. We have only a good polish when the speculum being placed about an inch from the flame of a candle, the surface cannot even then be seen, but appears like a hole cut in the side of a close box. If metals will not bear this test, is there any cause to be surprised, that, though they have only one-half the liability to errors of workmanship independent of the incurable aberration of dispersion, telescopes formed of them should still stand second to achromatics? I have been informed by a gentleman in London, who had every means of knowing it, that some of the cleverest, if not all the opticians there, used putty powder for polishing their specula; and from what I have seen of the work, I judge it was produced with putty on a metal containing a little arsenic. The same person procured for me the direction to a shop where the opticians provided themselves with the article, but I found it, though good, yet not materially different from what I had before used. This putty powder may be procured in the country from marble masons, glass-cutters, &c. and some druggists keep it.

Edwards's direction for making a polishing powder is to calcine the green sulphate of iron (copperas of the shops); but it parts very reluctantly with the last portions of its acid, and requires a great heat, and that to be continued for a long time, to get quit of it entirely, and the remaining oxide then becomes too hard. This is readily avoided by precipitating the oxide of iron with an alkali, and then calcining. The best method I have tried is to dissolve a quantity of the sulphate of iron in water, and allow it to stand for a few days, that the impurities may settle; then pouring the clear solution into another vessel, add to it solution of ammonia (volatile alkali), until there is no further precipitate formed; and, by continuing to add the ammonia until it is in excess, which is told by the strong smell it gives, we insure that the precipitate is a true hydrated oxide, and free from carbonate of iron. It must now be collected on a filter of muslin and well washed; then, when settled into a thick mud, it must be put in that state into a crucible covered from the dust, and kept in the fire at a low red heat for ten minutes, when the powder is prepared. For metal which requires a harder powder, it must be kept a longer time heated; and we may, by this means, have it of any hardness we wish. If there happen to be any carbonate of iron, it will be found to corrode the speculum in a peculiar manner in the working, and for this reason ammonia is to be preferred to the carbonates of potash or soda as a precipitant. I have, however, prepared a good powder by precipitating with pearl ash, but it required to be heated several times, and water to be dropped each time upon the red hot carbonate, to reduce it entirely to the state of red oxide, and it had then become rather too hard for my metals.

As the progress of polishing out the fine scratches left by the bed of hones is but very slow with the fine oxide, I have for some time followed a plan which I call double polishing, which is, having two polishers prepared with the mixture of pitch and rosin. I first polish with putty, bruising it fine as I want it between two flat surfaces of copper with a little water, on one of the polishers, and then, having the other polisher in a fit state, I finish the metal off on it with the fine powder bruised in the same way.

To polish glass, the mixture for the polisher may be $\frac{3}{4}$ rosin to $\frac{1}{4}$ pitch ; and $\frac{3}{4}$ pitch to $\frac{1}{4}$ rosin is a good mixture for speculum metal. The pitch and rosin often contain a good deal of dirt, which renders it desirable to filter them. This is easily done by tying a piece of muslin loose over the mouth of an earthenware jar, and having put the pitch and rosin upon it, placing the jar in the culinary oven. As they melt they pass through the muslin, and give in the bottom of the jar a fine and clean mixture. It is unnecessary with glass to use soap in the polishing, but with metal it is almost indispensible. Soap causes the powder to imbed itself, and also causes the metal to work smoothly without *jerks*, which jerks are a sign that you are spoiling it. It must, however, be in a state of adhesion, moving stiffly over the polisher, which requires also that you have it neither too wet nor too dry. With putty bruised fine, and used on a polisher hard enough for glass, cast steel which has been hardened without being *again tempered*, polishes as well as glass, by taking care to apply continually soap and water in small quantities, for it is otherwise very liable to turn *gray* in the softer parts of the steel, where the ductility is not entirely destroyed by the hardening process.

The amateur, unless he aspires at absolute perfection, needs not trouble himself with thoughts of that *bugbear*, " a true parabolic figure." I can asure him that all the most interesting objects in astronomy, and many of what are called difficult ones, are to be seen well with a circular curve, when the diameter of the speculum bears no greater ratio to its focal length than is generally used in the Newtonian telescope. In a concave speculum of $5\frac{1}{2}$ inches diameter to fifty inches focal length, the difference between the versed sine of a circular arc and the abscissa of a parabolic curve, is only .0000071517 of an inch, or about the 350th part of the breadth of a hair of the head, taking it at the $\frac{1}{400}$ of an inch, which quantity should be worn away from edge of the circular to bring it to the parabolic figure. Will not every one agree that it requires great care and nicety in manipulation to get a circular arc true to this quantity ? Hence, to make a *fine* telescope, the bed of hones is an indispensible tool, but more particularly for short foci than long ones; because the dimension of the emery used in grinding bears a greater proportion to the former than to the latter.

The ratio of $5\frac{1}{2}$ inches diameter of speculum to fifty inches focal length is much greater than prescribed by Sir Isaac Newton, or used by Sir William Herschel in his seven feet telescope, with which he made the largest number of his discoveries. If we take that proportion, or about $6\frac{1}{4}$ inches aperture to 84 inches focal length, the difference between the circle and parabola at the edge, or where greatest, is only .0000025146 of an inch, or about $\frac{1}{1000}$th part of the breadth of a hair, or, taking the ratio of distinctness inversely as the area of the least circle of aberration on the retina, arising from this cause, with equal magnifying powers, the seven foot telescope has the advantage in the proportion of about ten to one. A circular curve may be made to approach towards a parabolic or elliptical one by the method of Mr Mudge, though I must differ from him in some measure in accounting for the manner in which it is effected. After polishing, he allowed the speculum to cool for a few hours on the pitch with a little water about it, and then worked it again in a particular manner for a few minutes; and he attributes the resulting parabolic figure to the manner of working, when it appears to me that a great part of the effect ought rather to the attributed to the metal having been heated by the hands in polishing, and when cool having contracted again; it in consequence embraced tightly the polisher, particularly near the edges, and these parts would be the first to be affected by the fresh working. The method I have pursued in polishing is as follows:—Having got the bruiser and speculum to the same circular arc on the bed of hones, before commencing the polishing, I warm the bruiser to about 100° of Fahrenheit, and lay it with a little water on the surface of the polisher for a few minutes. The bruiser having expanded with the heat becomes of a longer radius, and communicates the same figure to the polisher; and when the speculum is applied, the polish proceeds quicker at the edges than at the middle. Placing the heated bruiser on the pitch has a great use in smoothing down any small prominences, which, if they were left, would scratch the speculum. To prevent heating the latter, I polish curved metals with a pair of gloves on, that the figure may not be altered during the process. But the difference in the curvatures of the metal and polisher produced as above, is not to the amount necessary to produce a parabolic figure, even if the working did not tend to retain it a circular one; and yet with no further process, a speculum finished in this manner will show many difficult astronomical objects.

I do not think that machinery will be found of any use in polishing specula, or at least those of moderate size. We cannot in this case argue from the manner in which common lenses are manufactured; for with glass, if you make no progress in the polishing, you at any rate do no harm; but it is otherwise with metal; and we are often indebted only to the sense of touch for information, that we are not only doing no good, but also, that if we persist with our work in that state, we shall find it necessary to return to the grinding process again. Believing as I do, that the reflecting telescope is the one with which all astronomical discoveries will be made which require very critical distinctness and defining power, I feel it incumbent upon me, though the subject is foreign to the immediate purport of this essay, to remove a very incorrect idea which is universal in the scientific world on the comparative illuminating powers of reflecting and refracting telescopes. It is generally thought that a reflecting telescope with two specula has only about one-half the light that an achromatic one with a double object glass of the same aperture has. I am not aware that there is any other foundation for this opinion than the experiments of Sir William Herschel; but though he found very correctly the reflective power of his specula, he by some unaccountable oversight, or by the unfitness of his photometer, very much overrated the quantity of light transmitted by glass. Having engaged in photometrical measurements of the quantities of light reflected and transmitted by glass, I soon found that Sir William had made a great mistake; and that, instead of crown glass transmitting 94.8 rays of every 100 incident, I found, as Count Rumford had done before, that the clearest and best pieces of window glass transmitted only between 87 and 88 rays of every 100, and of course two pieces would not transmit more than about 77, whereas Sir William allowed two lenses to transmit 89.9. From the great thickness of the glass in achromatic object glasses, there is a considerable quantity lost in the glass besides the reflection at the surfaces, and particularly in the crown glass, on account of the colouring particles it contains.

It being hence very desirable to ascertain the actual quantity transmitted through an object glass of known excellence, I have here to acknowledge the obligation I am under to Lawrence Buchan, Esq. of Ardwick, near Manchester, for his kindness and politeness in allowing me with his assistance, and that of my friend John Blackwall, Esq. of Crumpsall Hall, whose zeal in promoting science is well known, both of the council of the Manchester Literary and Philosophical Society, to measure photometrically the light transmitted through the double object glass of his fine and almost new six foot achromatic by Dollond. From averages of eight measurements at each point, I found for the centre of the lens the proportion to be 80.93 to 100 incident, for about the middle radius 80.63, and for as near the edge of the lens as possible, safely, 81.92 to 100. These quantities have, of course, to be corrected for the concentrating power of the lens, which I found by the usual proposition for conjugate foci, and also by measurement to be as 10^2 to 9^2, and this gives about 66 rays for the quantity transmitted of every 100 incident.

This quantity may vary some little in different telescopes, from the different thickness of the lenses, and the goodness of the workmanship; but taking it in comparison with plane glasses, I have very little doubt but that it is very near the truth; and we thus see that an achromatic telescope with one object and one eye-glass has no advantage over a reflector in respect of light, with one speculum and one eye-glass of the same quantity of available reflecting aperture, which it has of refracting. And a Newtonian telescope of five inches will have the advantage over an achromatic of four inches aperture in light; and, who will doubt where the advantage of defining power will be? Mirrors kept with care will retain a long time a reflective power of sixty-five to sixty-six of every hundred. The deterioration in the small oval speculum in the experiments detailed in my former paper published in this *Journal*, cannot fairly be compared with the general usage of a telescope, for it was there of little import spoiling a speculum by hard rubbing, to the necessity of avoiding all risk of being deceived by the least film remaining on its surface.

As to preserving mirrors, I have lately adopted a plan for my best small oval ones, which I find will be very desirable with larger ones; it is, to keep them in an air-tight vessel which contains a quantity of quicklime, and they will then require very seldom any other cleaning than just wiping away the dust with a camel's hair brush. A wide-necked glass-jar, with a glass-stopper well ground to it, is a very convenient receptacle for the small sized specula.

SMEDLEY HALL, 15*th October* 1830.

ART. III.—*Account of the Habits and Structure of a Male and Female Orang-Outang, that belonged to* GEORGE SWINTON, Esq. Secretary to the Government, Calcutta. By J. GRANT, Esq. Presidency Surgeon, Calcutta. In a Letter to Dr BREWSTER.

DEAR SIR,

LONG ere this I had fully intended to have followed up the account of the orang-outang in Mr Swinton's possession, which you were good enough to give a place to in your excellent *Journal;* with some supplemental particulars respecting the same animal, and a short description of another that became his companion; but the pressure of buisness in a climate where the sin of procrastination is too apt to beset one, has occasioned a delay in carrying my intention into execution, of which a memento in the number of *the Journal of Science* for October last, brought to my notice by a friend, has made me rather ashamed.

Before the receipt of this, you will, I presume, have heard, that the *Maharajah*, as we used to call the male orang formerly described,* is dead. There is a convenience in designating

* No. xvii. *Edin. Journal of Science.*

the creature by this name, which induces me to adhere to it, as it gives a certain individuality to a description.

During the year previous to his death, he had grown considerably, as may be seen by a comparison of the subjoined with his former measurements. His appearance became much more robust, and he seemed to have assumed a greater degree of boldness and consequence. His teeth at this time equalled in number those of the orang-outang described in Dr Abel's *Voyage to China*. The pectoro-laryngeal sacs, too, had assumed that pendulous and pursy form which is conspicuous in Dr Abel's plate, but was not perceptible when I formerly addressed you. Within fourteen months of his death, however, they had increased so much, that the collar which fitted him when I first took his measurements, would not buckle round him in its greatest circumference. The following are his measurements as taken on the 16th December 1828.

	Feet.	In.
* Height from vertex to heel, - -	2	8
Length from the acromion process of the scapula to the end of the middle finger, - -	2	1
From the top of the sternum to the pubis, - -	1	3
From the groin to the tip of the second toe, -	1	$5\frac{5}{8}$
From the wrist to the end of the middle finger, -	0	$6\frac{6}{8}$
Length of the palm of the hand, - -	0	$3\frac{6}{8}$
——— of the sole of the foot from the heel to the end of the middle toe, - - -	0	$7\frac{4}{8}$
From the knee to the sole of the foot - -	0	8
From nipple to nipple, - - -	0	$6\frac{3}{8}$
From between the eyes to the insertion of the head on the neck, - - - -	0	$8\frac{5}{8}$
Greatest circumference of the thigh, - -	0	$11\frac{2}{8}$
Circumference of the foot close to the roots of the toes,	0	$6\frac{5}{8}$
———————— round the shoulders, - -	2	$\frac{3}{8}$

* This, as compared with his former measurement, give an increase in about fifteen months of *six* inches. This leads me to suspect that there must have been an error in my *former* measurement. Of the accuracy of the last I have not a doubt.—J. G.

To the best of my recollection, he gained three inches in the above interval.—G. S.

	Feet.	In.
Circumference under the arm-pits, - -	2	0
——— round the loins, - -	1	$5\frac{5}{8}$
——— at umbilicus, - - -	1	$11\frac{4}{8}$
Greatest circumference of the leg, - -	0	$5\frac{6}{8}$
——— of the hand over knuckles,	0	$6\frac{3}{8}$
——— of the head above the eyes,	1	$3\frac{3}{8}$
——— from ear to ear round occiput,	0	$7\frac{2}{8}$
Greatest circumference round the chin over vertex,	1	$7\frac{2}{8}$
Length of the ear, - - - -	0	$1\frac{4}{8}$
Breadth between eyes, - - -	0	1
From the symphysis to the ramus of the jaw, -	0	$5\frac{1}{8}$
Length of the arm from the acromion process of the scapula to the olecranon, - -	0	$9\frac{2}{8}$
From the elbow to the wrist, - -	0	$9\frac{2}{8}$
From the tip of the thigh to the knee, - -	0	$6\frac{2}{8}$
From knee to heel, - - - -	0	7
Length of perineum from the scrotum to the verge of the anus, - - - -	0	$2\frac{2}{8}$

Weight (taken in September 1828) thirty-five pounds six ounces, Avoirdupois, giving an increase in a twelvemonth of thirteen pounds, six ounces.

At this time he had twelve teeth in each jaw, viz. six double teeth, four incisors, and two canine.

In a state of confinement the orang-outang appears to be subject to obstruction of the bowels, the consequence, probably, of want of exercise, and of the fruits and other food he feeds on in his native woods, and sometimes, as in the human subject, of dentition. Early in December 1828, the Maharajah was taken ill, and, as he was cutting his two last molares at the time, the irritation arising from this was, in all likelihood, the cause of his indisposition. It being pretty obvious at any rate, that he was suffering from constipation, some castor oil was offered to him, but he refused to take it. He was then laid down on his belly, and, with a Reid's patent syringe, an enema, consisting of castor oil and spirits of turpentine, with some warm water, was administered. This operation he submitted to with comparatively little resistance, considering its novelty. In this way nearly two quarts had been thrown up and retained, but not producing the desired effect, he was put into a warm bath. He submitted quietly to this also, and seemed to enjoy the pleasant warmth. After removal from the bath, and having been carefully wiped and dried, another enema was given. The repeated administration of the remedy, to the extent of several quarts, brought away a large quantity of indurated fæces, the evacuation affording evident relief. Eight grains of calomel were then given him in some milk, and he was allowed to wrap himself up in his blanket to take his repose. During the night, he was again copiously moved, and at day-break was sufficiently recovered to leave the bed which had been made for him in one of the rooms of the house, and to go to his own usual place of abode. All that day he evinced a disrelish for food, appeared languid, and retired early to rest. In the course of a few days, he seemed as well as ever. Independent of the humane design of relieving the poor creature from suffering, it was of considerable importance to preserve the animal's life if possible, for the purpose of solving an interesting zoological question. Those who perhaps may be apt to smile at any thing like a particular reference to the ailments and medical treatment of a species of monkey, should bear in mind that the diseases of animals, no less than their appearance and habits, merit the attention of the student in natural history, and ought not, therefore, to be overlooked when opportunities occur of properly adverting to them.

During the illness of the Maharajah, the woeful expression of his countenance very much resembled that of a human patient, and made the natives around him apparently forget his order of being. Indeed it was amusing to observe them while the enema was being given, coaxing and speaking to him in their language, as they would to a sick child, stroking and soothing him with—"Rajah Sahib! Rajah Sahib! Gently now Rajah Sahib!" when the creature happened to be a little restive or impatient.

I have now to introduce to your acquaintance another individual of the * orang outang kind, which arrived in Calcutta

* This is the animal of which a notice has been given in this *Journal* for October 1829.

in the early part of 1828. She (for the creature was considered a female) had lived with a family at Singapore for upwards of a twelvemonth, and was accustomed to play with the children. Her disposition was remarkably mild, and she had been taught to stand up and walk erect,—a position, which, in Calcutta, she frequently and habitually assumed of her own accord. She was procured from the family alluded to by Captain Hull of the Bengal Military Establishment, who presented her to Mr Swinton as a companion for the Maharajah. Like him, she soon obtained a name, and was called the *Rannee.* It would be quite superfluous to enter into a minute description of her appearance, since the account of the Maharajah in most particulars will apply to her. She seemed much of the same age and height, but more slender, and her features and limbs were more delicately formed. The hair of her head was of a finer texture, and her eye-lashes were larger and of a more silky appearance. The following is a memorandum of her measurements taken at the same time as the Maharajah given above.

	Feet.	In.
Height from vertex to heel, - -	2	$6\frac{5}{8}$
Length from the acromion process of scapula to end of middle finger, - -	2	$1\frac{2}{8}$
From the top of the sternum to the pubis, -	1	$1\frac{5}{8}$
From the groin to the tip of the second toe,	1	6
From the wrist to the end of the middle finger,	0	$7\frac{1}{8}$
Length of the palm of the hand, - -	0	$3\frac{5}{8}$
Length of the sole of the foot from the heel to the end of the middle toe, - -	0	$5\frac{1}{8}$
From the knee to the sole of the foot, -	0	8
From nipple to nipple, - -	0	7
From between eyes to the insertion of the head on the neck - - -	0	$7\frac{7}{8}$
Greatest circumference of the thigh,	0	9
———————— of the foot close to the roots of the toes, - - -	0	$6\frac{6}{8}$
Circumference round the shoulders, -	2	2
———————— under the arm pits, -	1	$11\frac{5}{8}$
———————— round the loins, - -	1	4
———————— at umbilicus, - -	1	6

Circumference above umbilicus, - -	1	10
Greatest circumference of the leg, - -	0	$6\frac{5}{8}$
——— ——— of hand over knuckles, -	0	$6\frac{1}{8}$
——— ——— of head above eyes, -	1	3
——— ——— from ear to ear round occiput,	0	$6\frac{5}{8}$
——— ——— round the chin over the vertex,	I	$6\frac{6}{8}$
Length of the ear, - -	0	$1\frac{4}{8}$
Breadth between eyes, - -	0	$\frac{6}{8}$
From ramus to symphysis of the jaw, -	0	$4\frac{4}{8}$
From acromion process to the olecranon, -	0	$9\frac{5}{8}$
From elbow to wrist, - - -	0	$9\frac{5}{8}$
From tip of the thigh to the knee, - -	0	$8\frac{1}{8}$
From knee to heel, - - -	0	$8\frac{4}{8}$
Perineum, - - -	0	$1\frac{2}{8}$

Weight (taken in September 1828,) twenty-nine pounds four ounces, Avoirdupois.

From the above, it appears that her limbs, in comparison with the head and trunk, were longer than the male's. Her hands too were longer, but her feet shorter. In walking she appeared rather taller, in consequence of her being actually more slender, and from holding her head and person much more erect than the Maharajah. She had ten teeth in each jaw, viz. four incisors, two canine, and four double.

Her sex for a time was rather questionable, some asserting that it was a male. Careful examination, however, which was afterwards proved by dissection after death, showed that the creature was really female, with the external organs of generation defectively evolved, an accidental circumstance, it is to be presumed, peculiar to the individual. Like the greater number of young female orangs that have been noticed, she had no nail on the great toe of either foot.

During the examination as to sex, the poor creature appeared to be excessively alarmed, moaning and crying in a pitiable manner, although of course she was handled as tenderly as possible under the circumstances. The appearance of the external organs, as stated already, was rather equivocal. There was no vulva, and a small penis-like body hung flaccid at the usual place. That it was, however, not a penis, became obvious on finding that it was imperforate and without a prepuce.

On raising it there appeared within about half an inch of its root, a small round opening, scarcely large enough apparently to admit the end of a common bougie, and through this the urine flowed. Whether this, however, was the proper urinary passage, or a common canal leading to a vagina and urethra, was a point not to be determined during the life of the animal.

Some curiosity was naturally entertained to observe the result of the first interview between her and the Maharajah. On her part, the timidity natural to the female sex marked her whole demeanour. His was less reserved, and he displayed an inquisitiveness evidently repugnant in some cases to her delicacy. Neither on this occasion, however, nor any other during their domestication together, did he ever evince the slightest indication of sexual passion.

Although they often played and wrestled together, they never quarrelled, but always continued on excellent terms. The Rannee appeared conscious of her inferior strength, and, as the weaker vessel, generally gave way. It is but justice, however, to the memory of the Maharajah to declare, that he paid her the deference due to her sex. Even in the article of food, if she chanced to get first possession, he never would snatch it from her. In this respect, biographical veracity constrains me to state, that *she* seemed to be more selfish, evincing but little disposition to share with him whatever she could manage to appropriate entirely to herself. For instance, durcing his illness, she appeared at first to sympathize in his sufferings, sitting beside him and bestowing the orang kiss. This she did by projecting her lips in the shape of a hog's snout into his mouth, a caress which was kindly taken; but the night being cold, she afterwards rather unfeelingly stripped him of his blanket, as an additional covering for herself.

She appeared, too, to be more mischievously inclined than the Maharajah, and somewhat foolishly so, for it was her great delight to untie the ropes attached to the top of some high poles fixed in the ground to afford them the exercise of swinging, of which both were exceedingly fond. No sooner had the ropes been again tied with all the knots that Gordian ingenuity could devise, than she would clamber up to the top of the pole, and never desist until with teeth and fingers she had disengaged them. While she was thus occupied the Maharajah would exhibit himself to the native spectators, whom his appearance was wont to attract daily in a crowd round the gate. Occasionally he would catch hold of some individual and unloosen his cummerbund or girdle cloth, in the corner of which the natives usually tie up their copper pice or half-pence, and the bettel which they may keep for chewing. Of this part of the office of the cummerbund he was perfectly aware, and when once master of the cloth, would set himself deliberately to untie the knot at the end of it, and extract the contents. These, whether pice or pawn,* he would immediately transfer to his own mouth. This petty larceny was usually gone through with the greatest solemnity of manner, which heightened the effect of the scene, and afforded infinite amusement to the admiring crowd. The Rannee never attempted to take purses in this direct and open manner; and if not engaged in swinging at the time, or untying the swing rope, would take her station on the top of the gate, quietly looking on while the Maharajah was levying his contributions as stated. As soon, however, as he had succeeded in making himself master of any booty, she would hasten to aid him in examining and tasting the articles.

For many months before, our satyrs had never been chained, but notwithstanding they evinced no desire to run away. During the mornings and evenings they were allowed to play in the Compound or Court yard, and the gate was their favourite lounge. In the middle of the day, to protect them from the excessive heat, they were shut up in an outer house adjoining the place where the palanquin bearers resided, and found amusement in clambering up and down the bamboos which were nailed up in front to prevent their getting out.

Their diet was extremely simple, consisting of fruit and milk. The Rajah was fond of wine, particularly champagne, also of country beer,† and of all spirituous liquors, which the Rannee, on the other hand, would not taste.

* A kind of condiment which the natives are very fond of chewing, composed of bits of areca nut, cloves, and chalk, wrapt up in a double fold of bettel leaf so as to form a small triangular packet.

† An Indian hot weather beverage, somewhat resembling ginger-beer.

Their disposition was mild and docile, and they never attempted to bite, but were apt to take one's hand in their mouths as a mode of testifying kindness; and with children they were uniformly gentle. The Rannee frequently walked in the erect posture. The Rajah did so but seldom, always preferring to swing himself forwards, resting on the back of his hands and wrists, or vaulting over on his head in a succession of somersets.

In January 1829, the Rannee was taken ill, apparently with a cold and defluxion of the lungs. Gradually the cough (which was attended with quick pulse and fever) got worse, and she grew daily more emaciated. She appeared very sensible to the effects or alternations of external temperature, and for the most part would remain in a recumbent position under a blanket. Various remedies were tried, but they were of little avail, and the poor creature died in the month of March following. The body was examined in a cursory manner by my friends Mr Breton and Dr Adam, and myself. We were much struck with the strong resemblance of the viscera generally to those of man. The cause of death was very extensive inflammation, followed by considerable adhesion and effusion in the thorax and abdomen. The urinary and generative system underwent a minute examination. A director having been introduced ito the external aperture, an incision was made carefully upon it down the perinæum. Two orifices then became visible, and the canal of each was traced to its source, the upper one leading to the bladder, and the other to the uterus. The latter or vaginal canal was evidently dilatable; in its undilated state it was large enough to admit a common pencil case. It was about an inch in length, and the blunt probe introduced along it was felt with the finger in the cavity of the pelvis, where it met with obstruction to its further progress from the *os tincæ*. Owing to the smallness of the parts, it required some little trouble to demonstrate them, but the existence of the uterus, with its Fallopian tubes and ovaries, was satisfactorily exhibited in the end. After this hasty inspection, the remains were put up in spirits and forwarded to the Zoological Society of London.

The Maharajah was not long destined to survive her, for on the 26th of June 1829, he also died. During the six months immediately preceding that event, he had had repeated attacks of fever, which were not ushered in, as far as could be judged, by any rigors, and the poor fellow in consequence gradually wasted away. At times he seemed to be free from feverish symptoms, when he would appear to revive, and resume his accustomed food of plantains and milk. For the most part, however, he lay down in a languid state, and evinced what in him appeared a most unusual degree of sluggishness. Mr Breton informs me that a quantity of loose green-coloured paper lying about his room, (for on his illness he was removed to Mr B.'s house,) such as is used for making pamphlet covers, &c. the Maharajah several times gathered them up and covered himself completely over with them. Did their green colour and lightness lead him to suppose that these pieces of paper were the leaves of trees? or may we infer from the above fact, that when they feel cold, orang-outangs, in a state of nature, cover themselves up with the leaves of trees? During his illness calomel, castor oil, and enemas were occasionally given, and they always seemed to afford relief.

You will easily imagine how disappointed we all felt at a circumstance which completely frustrated an expectation we at one time had entertained, that this orang-outang might be destined to solve the interesting question, whether it was a young one of the gigantic race, of which an individual was killed on the island of Sumatra by Captain Cornfoot's party; * or of a smaller species, supposing the orang proper to consist of a *Patagonian*, and a middle sized, or pigmy race? This question, it is to be feared, cannot be set at rest unless the observer was stationed for a sufficient length of time at Sumatra or Borneo, where the animal would enjoy its own native climate; since it is pretty well ascertained now that it cannot live long in any other.

Mr Breton, Mr Egerton, and myself examined the body of the male orang, but I cannot aver that the autopsy gave us a greater insight into the cause of death than we had before. The creature had literally pined and wasted away under a spe-

* *Asiatic Researches*, vol. xv. and *Edinburgh Journal of Science*, No. viii.

cies of irritative fever, attributable most probably to general insalubrity of climate. There were no signs of high and extensive inflammation, and no traces of disorganization, as in the case of the female.

A good outline of the anatomy of the orang-outang, as far as it goes, is to be found in an account of the dissection of one by Dr Jefferies of New York, * which tallies in almost all respects with what we observed in the one under consideration. In a climate like this, (the thermometer in the dissecting-room stood at 90°,) where animal matter proceeds with the utmost rapidity to a state of decomposition, zootomical research must always labour under great disadvantages, for which due allowance ought to be made.

The thorax having been laid open by an incision through the sterno-costal cartilages, and the sternum reflected back, the pericardium was exposed to view. This having been pierced, about two ounces of sanguineo-serous fluid flowed out —the pericardium having been slit up, an opening was made into the left ventricle of the heart, and the arterial system filled with common wax (red) injection. A dark injection having been prepared for the venous system, the nozzle of the syringe was introduced into the vena cava superior, and the fluid forced up. Unfortunately, however, the parts were ruptured in course of the operation, so that a great portion of the hot injecting ingredients escaped. This accident had not happened before the manifestation at the median vein (laid bare for the purpose) of the partial success of the process. As the subject, however, was becoming every moment more and more decomposed, we determined to lose no more time in injecting, so that we did not middle with the inferior venous system.

We next proceeded to open the head. The dura mater was found remarkably thick and strong, much more so than in man, nor was it apparently the effect of disease, but the natural state of the structure. On slitting it open, a little venous effusion was found between it and the pia mater. The meningeal artery having received a full complement of the injection, made a beautiful arborescent appearance. Reflecting

* See *New London Mechanics' Register,* vol ii. of Second Series.

back the meninges, we examined the longitudinal sinus, and found it filled with the black injection. The appearance of the brain was on the whole very human, but it did not exhibit by any means so many convolutions as in man. It was rather high at the coronal region, but fell off as compared with man at that part of the sinciput which phrenologists term the region of causality. The *corpus callosum* was well developed, but no raphé was observed. The choroid plexuses were very large, and well marked. The torcular herophili was found injected with the black fluid, which is a rare occurrence in the human subject. The different ventricles were distinct, as were the *corpora quadragemina*. We looked most carefully for the pineal gland, but there was none to be found, and whether this be generic or an individual peculiarity we could not determine.

The usually rough seat of the gland was found perfectly smooth. In Dr Jefferies's subject, unfortunately, the brain was not dissected, so that we can derive no analogical light from that quarter.

The nerves rise from and leave the orang brain in the same way as in the human, and are similar in appearance. The optic nerves were large, and the pathetics very small, resembling the finest white silk thread. The circle of Willis was found beautifully injected, and had a most human appearance. The same observation applies to the *pons varolii*, the *crura cerebri*, the *medulla oblongata*, the basilar artery, and the internal carotid generally.

The cerebrum throughout was very firm in its texture. There was not such a marked distinction, we thought, between the medullary and cortical substance as in man. The former appeared to predominate more than in man, and to be of a more clayish hue.

The cerebellum was very soft, and the *arbor vita*, consequently, somewhat indistinct. The cerebrum and cerebellum together, detached from their membranes, weighed eleven ounces and a-half, from which, perhaps, half an ounce may be deducted, on account of the injected matter.

The abdomen, as in Dr Jefferies's subject, presented a view so similar to the human, that it required some attention to note any peculiarity. The stomach, with its well-defined pylorus, was in situation and figure like that of man. The arch and sigmoid flexure of the colon, with the *caput cæcum*, and the *appendicula vermiformis* (four inches long) very much resembled the human. The liver, spleen, and duodenum were also in appearance very like the human, but smaller comparatively, especially the spleen.

The cystic and hepatic ducts were very distinct, until they became, as in the human subject, a common canal entering the duodenum. The organs of generation and of urine (including the kidneys) were also similar to the human, but smaller. The testes had not descended into the scrotum, and the spermatic artery was very small, and had received none of the injected matter.

The thoracic viscera bore the same striking general resemblance to those of man that the abdominal and cranial did. The breast was much smaller than one would expect it to be in a child of three or four years old. The chief superior arteries arose from the aortic arch, as in man, but the whole arterial system seemed on a more reduced scale, the aorta throughout not being so large as in a human child. The lungs, too, did not appear to be so distinctly lobulated as in the human subject.

The mouth resembled the human organ in a less degree than other parts of the animal. Its snout-like projection gives a more acute facial angle than is to be found in a human head, save in the lowest of the species, as the Carib Indian and the New Hollander. The dimensions of the mouth are also much greater than in man, to say nothing of the peculiar action or appulse of the lips, which is never witnessed in the most wild human savage. The colour of the roof of the orang mouth, too, (at least of the subject under consideration) is dark and not red or reddish flesh-like as in man. There was no uvula observable. The glottis, epiglottis, pharynx, and the *os hyoides*, with its connections, were much the same as in man. Two valve-like apertures leading into the pectoral sacs of Camper, are situated in the larynx, between the *os hyoides* and the thyroid cartilage, and were large enough to admit the end of a full-sized bougie. Through these passages the animal inflates these pouches at pleasure; for what purpose * is as yet a matter of uncertain speculation, unless it be, as Dr Jefferies surmises, to assist in supporting the animal when swimming,—a species of exercise, by the way, which the creature, I should imagine, can scarcely be much called upon to have recourse to, since neither Sumatra nor Borneo, I believe, affords large rivers or lakes for such a purpose. The pectoro-laryngeal sacs extend from under the chin, from the edge of the *platysma myoides* and skin on each side, down the neck and over the breast, and in the full-grown gigantic orang must be of enormous size.

The anatomy of the axilla much resembled the human in every respect, in the course of the artery and vein, and the disposition of the meshes of the nervous plexus.

The submaxillary glands were of an enormous size, much more so than in the human subject.

The anatomy of the heart and aorta is extremely like the human; and the great systematic trunk gives off its visceral branches in the abdomen precisely in the same manner.

The femoral artery high up gave off the *profunda* as in man, then pursued it course as a large trunk, and above the knee divided into two large branches; one being not a *ramus anastomoticus magnus*, as in the human subject, but the *tibialis anticus*, which gives out branches to the knee-joint. The other large branch, or more properly speaking, the popliteal, after running two inches and a-half, divided into the posterior tibial and interosseous, giving off small branches to the muscles.

Of the animal's osseous structure, I need say nothing, even if I had a sufficiently long and convenient opportunity to consider and reflect duly upon it, which I had not. The bones, however, have been sent to the Zoological Society of London, where, no doubt, they will attract the attention of competent judges. It is to be hoped that the illustrious Cuvier may have an opportunity of comparing them with those of others of the *Simia satyrus* race; more especially, the skeleton of Wurmb's Pongo, which is, if I recollect right, still preserved in one of the Royal Museums of Paris.

* Has the frog any sacs of this kind? The croaking sound given forth by the orang-outang is very like that of the frog.

Of the exact age of the animal, I possess no decisive data. Its great youth was manifested by the general appearance of the teeth, and of the bones, &c. The roots of the upper molares were only about half-formed ; * and the cartilaginous epiphyses of the bones, the non-descent of the testes, and the absence of sexual desire, irrefragably demonstrate its infantile adolescence. But whether its age was between four or five, or seven and eight years, I really cannot take it upon me to say.

From the formation of the teeth, the animal would appear to be omnivorous. In its docile state, it readily eats meat ; nor do I conceive it to be a very violent conjecture that, in the wild state, it may at times prey upon the lesser animals and birds. Indeed, I suspect that there are few quadrupeds that could resist the attack of a gigantic orang like Captain Cornfoot's. As is the case with other *Simiæ*, they are, perhaps for purposes of mutual convenience and protection, gregarious. On a former occasion, I adverted to the testimony of M. Foucher D'Obsonville, who states, on information procured on the spot, (at Sumatra,) that the orang-outangs wander in the woods, or upon mountains of difficult access, where they live in small societies, and take precautions for their subsistence and defence. Nor is it improbable to suppose that they live in pairs. It would, indeed, be most interesting if we could trace them to their secret haunts, and get accurate ideas of their general and *domestic* economy. We might then be able to determine whether, like foxes and other animals, orang-outangs are genuine troglodytes or not ; whether, like the beaver, they construct habitations for themselves, seeing that nature has given them hands to execute, if not sagacity to contrive ; whether, like the field-mouse or the ant they lay up stores of food ; whether the males are polygamists, or otherwise ; whether the female, at the time of bringing forth her young, retires to any particular place ; and whether, in that state, she is looked after, and supplied with food and drink by the male, or allowed to shift for herself ; whether the old and decrepid are allowed to perish from neglect, or, as has been witnessed among rats, they are tended by others of the community ; whether, supposing them to have a particlar cave or habitation, they remove

* The incisores and canine teeth were permanent.

the carcases of the dead, and what do they do with them; or whether, upon a casualty, they remove to other quarters, thus leaving the dead as in a tomb? In short, what are the habits of the animal in a state of nature, seeing that of its private history we absolutely know nothing? That they have some sort of habitation or other, is no difficult point of belief, since we have analogy for it; and that they generally betake themselves to such secret places as caverns, or hollow trees, to die in, I am inclined to think evinced by the circumstance of their bones or carcases being seldom or ever found, at least so far as I am aware. If the creature possesses no higher intellectual powers than the other wild denizens of the forest, of course, his habitation or den, or whatever it may be, will afford no evidence of ingenuity or forecast; since a higher degree of intelligence than his other sylvan neighbours would be thrown away upon him.

Orang-outangs, it has been remarked, have exhibited no greater degree of intelligence than a dog. This, generally speaking, is, I believe, a correct enough observation, but then let us bear in mind the comparative advantages, in relation to his connection with human society, that the dog possesses over the orang-outang! Companionship with man is to the dog a state of nature and gratification; he is "to the manner born." Not so the poor orang-outang; left, perhaps, when an infant or very young, and unable to provide for itself at some spot, while its mother wanders in another direction, with the intention of returning by-and-by to lead him *home.* A Sumatran or Bornese forester passing that way swoops him off; and the little creature that had been accustomed to active gambols in the wild wood, (to say nothing of change of diet, and climate, and water,) is henceforth transferred to, and confined to a small inclosure, where its movements are circumscribed, where he is perhaps chained; and never like the dog, solaced with the society of its kind; where, in short, his whole system and habits must undergo a change consequent on slavery; and where its faculties have not their fair field for developement. How is it to be expected, under such circumstances, that an orang-outang child (for all the orangs to descriptions of which I have had access, were supposed to be very young,) should be *more* intelligent than the most intelligent of all the inferior animals, the full-grown dog, in the prime of his faculties and strength, naturalized to a state of connection with human society, and unhappy save under such circumstances? The orang-outang, however, without being taught, will do what a dog, I suspect, cannot be taught to do, and untaught, cannot think of doing: he will untwist or unravel his chain or cord. If the dog is chained, and the chain becomes in any way jammed between things lying about, or twisted upon itself, the animal drags hard at it, *away* from the point of entanglement, perhaps increasing the evil,—becomes alarmed—cries out, and never thinks of slackening the chain, and returning back to see what the cause of the inconvenience is. Not so the orang-outang; the moment such an accident occurs, he deliberately sets about putting matters to rights. He does not drag away from the point of resistance, does not insist on running forcibly counter, but instantly slackens his chain, as a human being would do under the like circumstances, and goes back to *see* what occasions the obstruction. If the chain has got entangled with a box or any other article of furniture, he disengages it; if it has become twisted, he considers the matter, and untwists it. It may perhaps be said in reply, that the possession of hands gives the orang advantages that the dog has not, in the instance referred to, and so undoubtedly it does; but it is not natural for an orang to be chained, and the whole process evinces that he thinks or reflects upon the predicament he has got into, which the dog apparently does not, but looses his presence of mind. I have a monkey chained in my compound, *(Simia entellus,)* but when his chain becomes entangled or twisted, he does not get himself out of the scrape like the orang-outang, but, like the dog, makes matters worse by dragging impetuously.

Will you permit me, with due deference to the opinion expressed in your note upon my former communication, once more to touch upon the question of the orang-outang's mode of progression? The anatomical structure of the tribe, it is stated, precludes the *possibility* of their walking erect. Surely their going erect in a state of confinement sufficiently answers the question as to possibility? Then again, with reference to the fact cited of the gigantic orang-outang killed by Captain Cornfoot's party having been seen to move erect with a waddling gait; it is to a certain extent very justly observed in your note, that "many of the mammalia, when acting on the defensive,* rear upon their hinder legs, or sit upon their haunches; and it was quite natural for the large specimen killed by Captain Cornfoot, to *approach* his assailants in the erect position, this leaving, besides his *teeth*, his two arms free, to be used for his protection."—Now, my dear Sir, if you will refer again to the passage, you will observe that the animal was *running away* from his pursuers, so that his teeth and arms at the time were not in a state of defence; his attitudes altogether was one of sheer *escape*, not of defence or resistance, the position of all others which he was in on level ground, when running for his life, being the erect one! True Dr Abel observes, that "his motion on the ground was plainly not his natural mode of progression." This, however, has not, I think, been quite demonstrated. I admit that the climbing, bounding, or sitting attitude, is the most natural one of the creature among trees; but when he has to travel over a level surface, between copse and copse, or clump and clump, I do not think it has been unanswerably made out, that the erect is *not* his natural mode of progression. At any rate, my wavering notions on this subject have been rather confirmed since I made my former communication, by a similar leaning of opinion on the fact of Mr Breton, and other scientific friends, as well as by some remarks apposite to the subject made by Dr Jefferies in the paper already alluded to, no less than by what I have witnessed of the movements of several gibbons. The orang-outang, however, does not so much walk as *shamble* onwards, the knuckles of its long arms resting on the ground, forming as it were crutches for his body to swing between, so that the body is always very nearly erect. The long armed gibbon, again, rarely ever goes, except quite upright; it is merely when he wants to move a pace or two only, that he swings between his hands; when he means to go several paces he stands up and *runs*. But, remarks Dr Jefferies, with respect to the orang-

* The common brown bear for instance, or, to come to a more familiar illustration, the common goat.

outang, "the articulation of the femur, with the acetabulum, is almost exactly like man's; the neck of this bone forms about the same angle. In quadrupeds this forms a distinguishing characteristic, being in them nearly a right angle. The inspection of this joint is alone sufficient to satisfy a naturalist, of at least the facility, if not the natural disposition, of the satyrs, to walk erect." Although the orang-outang and the gibbon, however, can certainly proceed for a considerable or given distance in the erect position, it is evident, that they could not, like man, keep up that mode of progression for many hours, or to a great distance over level ground, save with most fatiguing exertion and many halts on the way. All the orangs and gibbons I have seen proceed in the erect position, always impressed me with the idea that they did so, somewhat in the manner of a child when it first learns and essays to walk, moving rather totteringly forward, as if afraid to fall.

The patella of the orang-outang is like that of man, as is indeed the whole knee-joint. The ankle is also like the human, and the *os calcis* is broad, and sufficiently projects behind to support the erect posture.

Dr Jefferies, from a view of the whole structure of the individual described by him, notes the peculiarities which he deems will enable us to form an opinion of his natural mode of progression; and with these, permit me to conclude what I apprehend you must think a very rambling, prolix, and unsatisfactory communication.

First, Going on all-fours, he would find inconvenience from the elbow-joint; for, when the hand is placed upon the ground flat, the flexion of the joint would be contrary to that of quadrupeds, by bending back towards the body instead of forward, which would rather impede than assist progression. It is not, however, as difficult for the *satyrus* to turn the joint forwards as it would be for man, on account of the curvature of the bones of the fore-arm, and the free motion which existed in all the joints.

The roundness of the chest, and the scapula sitting so far back, would make it difficult for him to bear weight upon the hands. Quadrupeds have * the chest flat, and the scapula far forwards upon the ribs.

The articulation of the hip would make it more easy for him to go erect, on account of the little angle made by the neck with the body of the femur.

Secondly, In walking erect he would derive advantage from the extension of the *os calcis* and the length of the foot; and also from the position of the arms so far back, and from their length, which would enable him to balance the body by them.

Thirdly, From the structure of the viscera, he seems to be peculiarly formed for an erect posture.

The pericardium being united extensively with the diaphragm, would prevent it from being drawn down by the weight of the liver and abdominal viscera. In quadrupeds this is not necessary, for the pressure of the abdominal contents assists expiration; and, if the pericardium was attached to the diaphragm, as in the *satyrus* and in man, inspiration would be impeded.

The exit of the spermatic cord is another difference from quadrupeds. It does not pass out directly from the abdomen as in the dog, but perforates the peritonæum and muscles obliquely, as has been described, thereby giving that admirable structure to fortify the groin from rupture, which exists in man.

The viscera of the abdomen were suspended to bear weight in the erect posture, particularly the liver, which had its ligaments very strong.

From these and other circumstances, apparent from an examination of the skeleton, I think we must conclude the erect posture to have been most natural. At least, if it is humiliating to dignify him with the title of a biped, he stands acquitted from that of a quadruped, from the peculiar formation of his lower extremities. We must then denominate him, as true naturalists have done, a quadrumanous animal.—I remain, &c.

Calcutta, J. Grant.
May 8th, 1830.

* That is to say, I presume, the sides of the chest.

ART. IV.—*An account of a Peculiarity not hitherto described in the Ankle, or Hock-joint of the Horse; with Remarks on the Structure of the Vertebræ in the Species of Whale, entitled Delphinus Diodon.* By ROBERT J. GRAVES, M. D., M. R. I. A., King's Professor of the Institutes of Medicine, Honorary Member of the Royal Medical Society of Berlin, of the Medical Association of Hamburgh, &c. &c. *

BEING engaged in the dissection of the horse, on examining the hock-joint, I found that any effort to flex or bend the limb at that joint, was counteracted by a considerable resistance, which continued until the limb was bent to a certain extent; after which, suddenly and without the aid of any external force, it attained to its extreme degree of flexion. In attempting to restore the extended position of the limb, I found that a similar impediment existed to its extension, until the same point was passed, when the limb suddenly, as it were, snapped into its extreme degree of extension at this joint.

At first I conceived that this phenomenon depended on the tendons of the flexor and extensor muscles of this joint; but on removing all these muscles and their tendons, it was not diminished, and it therefore became clear that it depended on some peculiar mechanism within the joint itself.

Before I enter into the details of this mechanism, it is necessary to remark, *that it is evidently connected with the power this animal possesses of sleeping standing*, for it serves the purpose of keeping the hock-joint in the extended position, so far as to counteract the oscillations of the body, without the aid of muscular exertion; and in this respect it resembles the provision made to effect a similiar purpose in certain birds, as the stork, and some others of the grallæ, which sleep standing on one foot. It will appear also in the sequel, that not only is the effect produced the same, but the mechanism is in many respects similar, if the account given by Cuvier, and also by Dr. Macartney, in *Rees's Cyclopædia*, article *Birds*, be correct.

Sheep and cows are not provided with ankle-joints of a si-

* From the *Royal Irish Transactions*, vol. xvi. Read July 5, 1830.

milar structure, and it is well known that these animals do not possess the power of sleeping standing. Another circumstance which adds additional interest to this peculiarity of structure, is, that it may possibly be connected with the disease termed *String-halt*, in which the limb is at each step suddenly flexed, to a degree far beyond that required in ordinary progression. Whether this is owing to a sudden and jerking flexion of the whole limb, or to flexion of the hock-joint alone, I have had no opportunity lately of determining. If the latter be the case, it is probably connected with the structure of the hock-joint, which I am about to describe. It may be right to observe, that not even a probable conjecture has been advanced, concerning the nature and cause of string-halt, a disease to which the sheep and cow are not subject, and we have already observed, that in these animals the structure of this joint presents nothing remarkable.

The hock-joint is a good example of what is termed the hinge-like articulation, and is formed between the tibia and astragalus, which latter bone presents an articulating surface, with a nearly semicircular outline, and divided into two ridges, including between them a deep fossa. The tibia is furnished with depressions which ride upon the ridges of the astragalus, and has anterior and posterior projections, which, moving in the fossa, are received into corresponding depressions in the astragalus, at the moment the limb arrives at the greatest degree either of flexion or of extension.

The shape of the surfaces of the astragalus concerned in the articulation, is not that of a given circle throughout, for towards either extremity, the *descent is more rapid*, or, in other words, answers to an arc of a smaller circle. Hence, when one of the projections of the tibia has arrived at its corresponding cavity in the astragalus, which happens when the limb is either completely flexed or completely extended, the rapid curve of the articulating surface presents a considerable obstruction to change of position. Thus, the form of the articulating surfaces, in itself, to a certain degree explains the phenomenon, but its chief cause is to be found in the disposition and arrangement of the ligaments.

The external malleolus of the tibia is divided by a deep groove, for the passage of a tendon, into an anterior and posterior tubercle; from the latter of which and close to the edge of the articulating surface, arises a strong and broad ligament, that is inserted into the *os calcis.* Under this lies another ligament, which, arising from the anterior tubercle, is also inserted into the *os calcis.* It is to be observed, that the origin of the latter is anterior to that of the former, but its insertion posterior, so that these lateral ligaments cross each other in the form of an ×. The external articulating protuberance of the astragalus on which the tibia revolves, has, as has been already stated, a nearly circular outline, and the attachments of the ligaments just described, are at points on the outside of the os calcis, which would lie nearly in the circumference of that circle, were it continued from the articulating surface; so that each of these ligaments has one of its extremities fixed in a certain point of the circumference, while its opposite extremity revolves during the motion of the joint, nearly in the circumference of the same circle. This observation applies likewise to the two lateral ligaments on the inner side of the joint, which have nearly the same relation to each other, and to the general contour of the joint, as that just described; so it is obvious, that during the rotation of the joint, as the origins of these ligaments move along the same circumference in which their attachments are fixed, the ligaments will be most stretched when they correspond to diameters of that circle.

Now it is so arranged that this happens at the same time for all, and consequently the ligaments on each side correspond not merely as to direction, but as to the point of time they become most stretched, which is nearly at the moment that the joint has no tendency to move either way, and at that moment, it is to be observed, that although the ligaments are most tense, and of course react on their points of attachment with greatest force, yet this produces no motion, as the force is exerted in a direction perpendicular to the circumference; but as soon as the tibia is moved beyond this point of inaction for the ligaments, the latter, no longer representing diameters, by their contractile force evidently tend to accelerate the motion, and as they all act in the same direction, and are assisted by the shape of the articulating surfaces, a sudden motion of flexion or extension is thus produced.

The preceding explanation supposes the ligaments of this joint to possess, contrary to the nature of ligaments in general, a certain degree of elasticity, which was evidently the case in all, *but particularly* in the most deep-seated of those on the inner side of the joint, which, therefore, appears most concerned in producing the sudden motion, whether of flexion or extension.

In the autumn of 1829, two of the species of whale called *Delphinus diodon*, by Hunter, *Hyperoodon*, by La-Cepede, and *Ceto-diodon*, by Dr Jacob, were captured near Dublin, one of which, measuring about sixteen feet in length, I procured for the purpose of preparing its skeleton.

After the spinal column had undergone maceration for a few days, I found that the intervertebral substance could be easily detached from the bodies of the vertebræ, and that it carried with it, firmly attached to each of its extremities, a flat circular bone, about a quarter of an inch in thickness, and exactly corresponding in the extent and shape of its surface, to the surface of the body of the vertebra, from which it had been separated.

The separation was effected with facility, and took place spontaneously and completely when the maceration had been continued some time longer.

The surface of the flat bone, where it had been adherent to the body of the vertebra, was of a spongy texture, afforded a passage to many blood-vessels, and was marked by numerous sharp projections and deep furrows, diverging from its centre, and answering to similar projections and furrows on the denuded extremity of the vertebra; of course the surface of these bones varied in shape and size with the extremities of the vertebræ to which they were attached, being from five to six inches in diameter at the dorsal, and not more than one inch at the last caudal vertebra.

The substance of these bones towards the intervertebral substance was of much harder and closer texture than that of the bodies of the vertebræ themselves, and where it was adherent to the intervertebral substance, it had a smooth surface, marked with a great number of concentric lines, answering to the arrangement of the fibres in the intervertebral tissue, which adhered to this face of the bone with great strength. This marking was deficient towards the centre where the intervertebral substance is fluid.

The facility with which these bones are detached, is the reason why we never find them adhering to the vertebræ of those young whales which have been wrecked on our coast, and whose skeletons have been exposed to the action of the waves and the weather. Their flat shape too renders them liable to be covered by the sand, and hence I have never known them to be found separately, even when the vertebræ and other bones of this species of whale were scattered along the coast in great numbers, as happened at Dungarvan some years after several of these animals had been captured and dragged ashore by the fishermen.

The bones I have described must evidently be considered in the light of terminal epiphyses of the bodies of the vertebræ, and are deserving of notice on account of the facility with which they can be detached, *even in very large, and of course not very young animals of this species,* as I observed in the two skeletons preserved in the College of Surgeons, one of which measures thirty feet in length; so that when the skeleton has been artificially prepared, they resemble separate intervertebral bones rather than vertebral epiphyses. In the land mammalia the consolidation takes place much more rapidly, and a few years are sufficient to efface all traces of former separation between the epiphyses and the body of the vertebra; the comparative slowness of this process in the whale, is probably referable to the longevity of the animal, and the greater length of time necessary to complete its growth. A knowledge of this fact puts us in possession *of a new and useful mark of the animal's age, independent of its size,* and it is for this purpose I have brought it forward, for although not noticed by any author I have seen on the Anatomy of Whales, it must nevertheless have been known to several. If we find that the terminal epiphysis has become completely united to the body of the vertebra, we may be assured that the bone, whether large or small, belonged to an animal arrived at maturity; but if not, we may conclude that it had not yet attained to its greatest size. To facilitate this inquiry, I may remark, that a very slight examination of a vertebra is sufficient to determine, whether the epiphysis has, or has not been detached; as in the former case the surface is *marked by deep ridges and furrows diverging from the centre towards the circumference;* whereas in the latter, if the animal was of moderate size, the marking consists of concentric lines, answering to the attachments of the intervertebral substance; and if the individual was very large, these concentric lines are exaggerated into concentric furrows; and whether the attachments of the intervertebral substance be marked by concentric lines or by concentric furrows, *a considerable portion of the central part of the bone, where it had been in contact with the internal substance of the intervertebral ligaments, is quite destitute of this marking*, and presents a striking contrast to the rest of the surface.

I am not aware that the true cause of this remarkable difference between the markings on the extremities of the vertebræ of the cetacea has been before explained.

It may not be uninteresting to add, that the cranium of the *Delphinus diodon* in my possession, and both those in the Museum of the College of Surgeons, present, in a very remarkable manner, the want of symmetry between the right and the left sides of the cranium, which was first observed by Meckel in the skulls of the Cetacea.

Note.

Since the preceding notice concerning the hock-joint of the horse was submitted to the Academy, I have had an opportunity of examining two horses affected with string-halt, and am inclined to attribute the disease to a spasmodic affection of the flexors of the limb generally, rather than to any derangement in the structure of the hock-joint. It may be right to mention that the following authors on Comparative Anatomy, and the Anatomy of the Horse, have been searched, but they contain no notice of the peculiarity in the structure of the hock-joint, above described.—Macartney, Cuvier, Carus, Blumenbach, Meckel, Clater, Blaine, Stubbs, Percivall, Boardman, White, Lawrence, Osmer, Home, Bourgelat.

Art. V.—*An Account of Experiments to determine the reflective powers of Crown, Plate, and Flint-Glass, at different angles of incidence; and an investigation towards determining the Law by which the reflective power varies in transparent bodies possessing the property of single refraction.* By R. Potter, Esq. Junior. Communicated by the Author

As soon as I had sent my former paper on the reflective powers of metals to the President of the Royal Society, when considering the cause of reflection I naturally asked myself the question, "If this cause, as Sir Isaac Newton, and philosophers generally after him, have supposed, is the same principle existing at the surfaces of all bodies; then, if metals reflect *less* light when incident *more* obliquely, how is it that with the same principle transparent bodies should reflect more?"

We have no other alternative than to allow that there must be other forces concerned besides the one which is the immediate cause of reflexion. In our inquiries into the connection existing between the different physical properties of matter, our only safe and legitimate course is by induction from experiments. It is a much less laborious one to speculate in the closet on the isolated facts we have observed, and to frame theories to account for them, but every page of the history of science warns us against placing too much reliance on abstract reasoning. We there see that the speculations of the greatest minds have been oftener found wrong than right when they have ventured into depths beyond the fair induction from phenomena observed; and hence the motive which induced me to undertake the patience-trying investigation of the reflecting and transmitting powers of crown, plate, and flint-glass at different angles of incidence, which is contained in the few following pages. Their possessing the same properties of density, refractive power, and capacity for heat in different degrees, gave me reason to hope, that, if the forces which cause the phenomena of reflection depended on any of these, it would be manifest in the different results.

The proof advanced by Sir Isaac Newton, that the reflection at the surfaces of bodies cannot be caused by the particles of light striking the solid particles, *(say solid ponderable particles,)* must, I think, be allowed by every one to be sufficient; but if other proof were necessary, it might be derived from the fact, which is familiar to every working optician, that both glass and metal polish equally well in every possible direction, and no figure bounded by planes, (which is essential,) can be assigned to the ultimate particles of matter, which will present the same face in every direction. The theories which have been built on the supposition of light and heat being a mere *condition* of matter, as consisting in undulations or vibrations, are quite insufficient to account for the phenomena of reflection. The laws incontrovertibly point out that the effect is governed by the physical properties of bodies, and this takes the cause entirely away from all purely mechanical rules, and metaphysico-mathematical calculations. When we can account for double decomposition and elective chemical affinity by mechanical hypotheses alone, we may then with propriety apply them in optical physics, but not until then. The whole phenomena of light and caloric exhibit them possessed of properties analogous in affinities and repulsions to what we observe in ponderable matter, and the distance between the subtile gaseous matters, such as hydrogen and nitrogen, and the heavy inoxidizable metals, seems scarcely less than the distance between them, and those subtile matters which acknowledge no subjection to the influence of gravity.

The phenomena of the reflection of light not allowing us to compare it with the echo of sound, by its impinging against the hard *ponderable* particles of bodies, philosophers have of necessity considered it as the effect of some subtile power at the surfaces of bodies. The great advancement of chemical science gives us an advantage in our inquiries which the opticians of a century ago were almost entirely without, and which we must not overlook. The discovery of Dr Black of the attraction between caloric and ponderable matter, combined with other discoveries, has induced some philosophers to consider light and caloric as the same matter in different circumstances, and reflection as caused by an atmosphere of caloric retained around bodies by this attraction. This is the most probable theory that we can adopt on attentively considering the facts within our knowledge; but it has not yet received that proof which would be desirable. My experiments, with both metals and glass, tend to support it; and if it should be found to hold in *one* other metal I should consider it as established.

The word atmosphere is perhaps as correct as any we have to give an idea of an elastic medium held in its place by the attraction between it and the mass it surrounds, and the experiments on the flexion of light show us that it extends to a distance from the surface, and that its density is some function of the affinity and distance. This consideration of an atmosphere enables us to account for the angle of incidence being always equal to the angle of reflection, by the path of a ray thrown into such an atmosphere which resists its entrance by a repulsive principle, being similar to the path of a projectile acted upon in a converse manner by the force of gravity; and the depth to which any ray would penetrate by its own endeavour alone, must depend on its projectile force compared with the repulsive force of the atmosphere. This repulsive power appears to be sufficient to reflect back every ray, even if incident perpendicularly with the velocity that we find it possessed of if there were no attraction between the rays of light and the ponderable matter. On these attractions, combined with the projectile force of the rays, depend the laws which govern the effect we find by experiment. In the simple undecompounded bodies of the metals the reflective power is the greatest; but my experiments show that the attractive force in them increases with the angle of incidence comparatively with the reflective force, and the law of the effect shows that they both vary as the sine of incidence. When we come to examine the two in transparent substances, we shall find proof of the same attractive and repulsive force in them, with another attractive force in addition, which varies according to a different law. The mode of action of this latter force enables us to identify it with the refractive power, and it exhibits itself closely connected with the electro-chemical constitution of transparent bodies. In the consideration of an atmosphere or a fluid, which is indispensable in every theory of light, it cannot be defined as a congregation of hard homogeneous particles in contact with each other. We should never call a heap of sand a fluid. The idea implies a constitution which allows the particles free motion amongst each other at the same time that they are subject to an attraction which keeps them together. If, then, the particles of light and caloric do act upon each other in this manner, as they certainly do, we are driven to the necessity of an intermediate agent, as in all ponderable fluids within our cognizance, possessed of satellite properties, and attending on the base of light or caloric, as caloric attends on ponderable matter, and gives to it the solid, liquid, or gaseous form, according to its presence in intensity, and the affinity between them. I believe that all our researches will confirm this view, and that, so far from its being a fanciful hypothesis, we cannot be led to any other conclusion when we consider with a careful and earnest attention the phenomena of light. This constitution of a base, combined with satellite matter, if I may so call it, shows us the cause and mode of action of the elastic or repulsive properties, and gives us a clew to the solution of those singular and highly interesting modifications which light is capable of.

The measurements of the light reflected and transmitted by glass, in the experiments of which the results are about to be given, were taken with the same photometer as the former ones with metal, but with such alterations as the great difference in the reflective power of glass at different incidences rendered necessary; and I used a small instrument furnished with a plumb-line, which could be placed on, and was moveable round the same point which carried the arm with the reflector, to insure correctly the incidence, and the position of the reflecting substance under examination.

When I first commenced the experiments, they appeared to give more light, as reflected, when incident nearly perpendicularly than when rather more obliquely; I readily saw that this arose from the extraneous light reflected by the different parts of the apparatus situated near the lamp; and when the lamp and reflector had to be brought very near the screen, it bore a very considerable ratio to the whole light reflected. Whilst this extraneous light remained undetermined, I was convinced that the measurements would be comparatively worthless, and for some time I despaired of finding any method of determining it; but, however, at last I hit upon the following plan. I found that glass ground rough with coarse sand on one side, in transmitting the light incident on it, disperses it pretty evenly without losing too much of it for the purpose, and considered, that, if a certain area of this ground surface was placed together with the left hand lamp, so as to give an equal illumination with the gross light reflected, then the area required to give an equal one with the extraneous light, being compared with the whole area, would give the ratio between the gross and extraneous light, the apparatus being in the same position as in the measurements, but with the light from the reflector turned off or on the thin paper, as required. This method is quite efficacious, and the extraneous light in the tables may *generally* be considered as determined to less than a tenth or twelfth part. The quantities up to 70° were measured, but at 80°, and 85° being so small in proportion to the whole, they are only estimated.

The pieces of window-glass were selected very carefully from amongst large quantities, and of all I examined, I only found one piece of $4\frac{1}{2}$ inches in length, and $1\frac{7}{16}$ in breadth, which was flat on both sides, and of good clear surface. In the larger portion of window-glass there is a slight mistyness to be seen, when it is wiped very clean, which is on the surface of the glass, and which, when examined with a lens, appears to have arisen in the manufacture, the surface being torn in small spots, by its becoming solid before the interior.

The pieces of plate and flint-glass were ground flat and polished for the purpose. The reflection at the second surface was prevented, when required to be so, by a coating of black varnish; that used for the crown and plate-glass was black sealing-wax dissolved in alcohol; and that for the flint was a similar solution of balsam of Tolu, with some fine soot from the flame of a candle mixed in it. I have not yet examined the refractive powers of the glass used; but in the plate-glass it will be rather *higher* than in the crown, because, when varnished as above, a faint blue reflection was perceptible at the second surface, which was not the case with the crown glass. The flint-glass also exhibited a similar one with its varnish.

Measurements of the Light reflected at the first surface of crown glass, specific gravity 2.541, three pieces used of $1\frac{7}{8}$ by $1\frac{7}{16}$—$1\frac{13}{16}$, by $1\frac{1}{2}$ and $4\frac{1}{2}$, by $1\frac{7}{16}$ inches, and thickness about $\frac{1}{17}$ inch.

Incidence.	Gross reflected of every 100.	To be deducted as extraneous.		Reflected by the glass.
10°	4.59	$\frac{102}{500}$ths,	=.93	3.66
20°	4.54	$\frac{80}{500}$	.72	3.82
30°	4.69	$\frac{56}{500}$	.52	4.17
40°	4.94	$\frac{46}{500}$	.45	4.49
50°	5.68	$\frac{38}{500}$	.43	5.25
60°	8.11	$\frac{22}{500}$	.35	7.76
70°	13.95	$\frac{9}{500}$	.25	13.70
80°	34.00	$\frac{4}{500}$	.27	33.73
85°	54.49	$\frac{2}{500}$	.21	54 28
85°	55.08	$\frac{2}{500}$	.22	54.86

Measurements of the light reflected at both surfaces of crown glass, three pieces used of $1\frac{7}{16}$ by $1\frac{3}{8}$, $1\frac{3}{8}$ by $1\frac{3}{8}$, and $4\frac{1}{2}$ by $1\frac{7}{16}$ inches.

10°	7.67	$\frac{70}{500}$ths,	=1.07	6.60
20°	7.88	$\frac{66}{500}$	1.04	6.84
30°	8.56	$\frac{61}{500}$	1.04	7.52
30°	8.52	$\frac{61}{500}$	1.03	7.49
10°	7.66	$\frac{70}{500}$	1.07	6.59
20°	7.86	$\frac{66}{500}$	1.03	6.83

30°	8.34	$\frac{61}{500}$	1.01	7.33
40°	9.34	$\frac{54}{500}$	1.00	8.34
50°	10.57	$\frac{41}{500}$	.86	9.71
60°	13.85	$\frac{32}{500}$	.88	12.97
70°	23.58	$\frac{10}{500}$	.47	23.11
70°	23.27	$\frac{10}{500}$	.46	22.81
80°	42.50	$\frac{1}{120}$	.35	42.15

The above measurements with crown glass of the reflection at the surfaces are, I think, as correct as I could expect to determine them; but those of the transmitted light which follow, show a liability to error to the amount of about $\frac{1}{40}$ of the whole, and yet I have little hope of getting a better set, without great practice, on account of the colour which the glass gives to the light. The eye often becomes prejudiced when judging between a green and an orange.

Measurements of the light transmitted by crown glass to determine the quantity lost in the glass and dispersed.

Incidence.	Total transmitted of every 100.	Average.	Reflected and lost.	Reflected at surfaces.	Loss.
0°	86.16 88.04 86.30	86.83	13.17		
10°	87.49 86.72	87.10	12.90	6.60	6.30
30°	86.03 84.01	85.02	14.98	7.40	7.58
50°	81.64 81.41	81.52	18.48	9.71	8.77
70°	70.79	70.79	29.21	23.00	6.21

Measurements of the Light reflected at the first surface of plate glass, spec. gravity 2.511, one piece used 2 inches by $1\frac{5}{8}$ and thickness $\frac{1}{8}$ inch.

Incidence.	Gross reflected of every 100.	To be deducted as extraneous.		Reflected by the glass.
10°	4.54	$\frac{92}{500}$ths, =	.83	3.71
10°	4.40	$\frac{85}{500}$	.74	3.66
20°	4.32	$\frac{68}{500}$	.58	3.74
30°	4.56	$\frac{52}{500}$	.47	4.09
40°	4.78	$\frac{40}{500}$	.38	4.40
50°	5.92	$\frac{30}{500}$	.35	5.57
60°	7.55	$\frac{23}{500}$	.34	7.21
* 60°	8.33	$\frac{20}{500}$	.33	8.00
70°	14.34	$\frac{10}{500}$	.28	14.06
80°	34.29	$\frac{4}{500}$	.27	34.02
80°	34.84	$\frac{4}{500}$	.27	34.57
85°	54.80	$\frac{2}{500}$	.21	54.59

Measurements of the light reflected at both surfaces of plate glass.

10°	7.72	$\frac{63}{500}$ths, =	.97	6.75
10°	7.76	$\frac{63}{500}$	.97	6.79
20°	7.91	$\frac{57}{500}$	.90	7.01
30°	8.16	$\frac{47}{500}$	.76	7.40
40°	8.96	$\frac{39}{500}$	.69	8.27
50°	10.48	$\frac{32}{500}$	.67	9.81

* This was a careful re-measurement, when I found reason to suspect some error in the other.

50°	10.45	$\frac{32}{500}$	.66	9.79
60°	14.27	$\frac{24}{500}$	.68	13.59
70°	25.02	$\frac{14}{500}$	.70	24.32
70°	24.89	$\frac{14}{500}$	.69	24.20

Measurements of the light transmitted by plate glass, to determine the quantity lost in the glass and dispersed.

Incidence.	Total transmitted of every 100.	Reflected and lost.	Reflected at surfaces.	Loss.
0°	91.42	8.58		
10°	90.84	9.16	6.77	2.39
20°			7.01	
30°	90.64	9.36	7.40	1.96
40°	89.36	10.64	8.27	2.37
50°	87.51	12.49	9.80	2.69
60°	83.94	16.06	13.59	2.47
70°	74.64	25.36	24.26	1.10
80°	54.83	45.17		

Measurements of the Light reflected at the first surface of flint-glass, spec. gravity 3.225, one piece used by $1\frac{5}{8}$ by $1\frac{1}{16}$ inches, and thickness $\frac{1}{13}$ inch.

Incidence.	Gross reflected of every 100.	To be deducted as extraneous.		Reflected by the glass.
10°	5.08	$\frac{124}{500}$ths, =	1.26	3.82
20°	4.94	$\frac{82}{500}$	.81	4.13
30°	5.05	$\frac{60}{500}$	.60	4.45
40°	5.29	$\frac{43}{500}$	.45	4.84
50°	6.73	$\frac{32}{500}$	.43	6.30
60°	9.37	$\frac{26}{500}$	.48	8.89
70°	17.44	$\frac{11}{500}$	.38	17.06
80°	35.96	$\frac{4}{500}$	.28	35.68
85°	57.49	$\frac{2}{500}$	.22	57.27

Measurements of the Light reflected at both surfaces of flint glass.

Incidence.	Gross reflected of every 100.	To be deducted as extraneous.		Reflected by the glass.
10°	8.56	$\frac{29}{500}$	.49	8.07
10°	8.56	$\frac{29}{500}$	.49	8.07
20°	8.81	$\frac{37}{500}$	.65	8.16
20°	8.81	$\frac{37}{500}$	.65	8.16
30°	9.87	$\frac{38}{500}$	.75	9.12
40°	10.90	$\frac{31}{500}$	.67	10.23
50°	12.46	$\frac{24}{500}$	.59	11.87
60°	16.48	$\frac{16}{500}$	.52	15.96
70°	27.71	$\frac{9}{500}$	.49	27.22

Measurements of the Light transmitted by flint-glass, to determine the quantity lost in the glass and dispersed.

Incidence.	Total transmitted of every 100.	Reflected and lost.	Reflected at surfaces.	Loss.
0°	87.85	12.15		
10°	86.97	13.03	8.07	4.96
30°	86.25	13.75	9.12	4.63
50°	83.42	16.58	11.87	4.71
50°	83.99	16.01	11.87	4.14
70°	71.05	28.95	27.22	1.73
80°	52.97	47.03		

When I first undertook the investigation of the law by which the reflective power varies in glass, the most promising manner in which I could construct geometrically the experiments, seemed to be that of considering the various rays of light as falling upon one point in a reflecting plane at different incidences; and this idea has eventually enabled me to discover the law of the variation of the reflective power at different incidences both for glass and metal. When I tried in this manner the quantities of light reflected by window-glass at the first surface, from a rough preliminary course of experiments, and before I had tried the measurements previously taken with metal, the quantities being set off on the perpendiculars they took a curved form, which seemed to resemble an hyperbola, and I was struck with the similarity between it and those given by Sir Isaac Newton, as produced by inflection.

On a closer inspection, however, the curve appears not symetrical, and so not correctly an hyperbola, or any line of the second order; but we shall find that this appearance arises only from the vertex of the curve not being on the chord of the quadrantal arc; and when we have found the position of its assymptotes, it proves to be a true hyperbola of the second order of lines; and from its properties we may calculate the light reflected at every incidence, when we know the values of certain constants which enter into the equation, and which have different values for different sorts of glass. On applying the formula to Bouguer's experiments on the reflection by water, I find a very near agreement there also, so that I have no doubt that this formula is true for the first surface of all transparent bodies, both liquid and solid, which have only the property of single refraction.

The geometrical construction being made in a similar manner to the one detailed in my former paper on the reflective power of metals, we obtain a figure similar to Plate I. Fig. 4. and, taking the point O for the origin of the co-ordinates, we have $y =$ the quantity of light reflected, $x =$ the sine of incidence, $r =$ radius, and the quantity of light supposed to be incident, and the equation of the reflective power is $y = a + \frac{c^2}{r + b - x}$ where a, b, and c, are indeterminate constants, having different values for different singly refracting transparent bodies, depending on the peculiar property of each.

This equation is derived from that of the rectangular hyperbola between the assymptotes $x' y' = c^2$, where $y' = y - a$ and $x' = r + b - x$, $a =$ the distance of the assymptote ef, from the radius $O\,Q$, and b the distance of the other assymptote from a perpendicular drawn to the extremity of that radius.

Taking the formula $y = a + \frac{c^2}{r + b - x}$ and $r = 100$, I find the values of the constants, as near as I can determine them from the measurements, to be about as follows, viz. for crown glass, spec. gravity 2.541, $a = 2.7$, $b = 1.04$, $c = \sqrt{76}$; for plate glass, spec. gravity 2.511, and refractive power rather higher than crown, $a = 2.58$, $b = 1.13$, $c = 9$; and for flint glass, spec. gravity 3.225, $a = 2.63$, $b = 1.44$, $c = 10$. On these data we have the quantities of light reflected as in the following table; and we find them to agree as near with the experiments as can be expected; and few experimenters will be able to obtain the measurements nearer, *generally*, to the calculations, until they have had very great practice in photometry.

Table of the quantities of light reflected by glass at the first surface, calculated from the formula $y = a + \frac{c^2}{r + b - x}$

Incidence.	Crown. 2.541	Plate. 2.511	Flint. 3.225
0°	3.452	3.380	3.615
10°	3.608	3.546	3.819
20°	3.837	3.790	4.117
30°	4.189	4.164	4.574
40°	4.767	4.778	5.320
50°	5.810	5.882	6.656
60°	7.964	8.155	9.369
70°	13.448	13.891	16.015
80°	32.396	33.155	36.422
85°	56.202	56.204	57.559
90°	75.776	74.261	72.074

Before we can investigate the law for the second surface, it will be necessary to find the amount of the first reflection at that surface proportionally to the quantity incident on it; but, as will be seen by Fig. 5, Plate I. that the quantity of the reflection at both surfaces, as determined by the measurements, is the sum of the reflection at the first surface $e\,r$, and of the reflections $e'\,r'$, $e''\,r''$, $e'''\,r'''$, &c. from the second surface. Now, if there were no light lost in the glass, we might determine the first reflection at the second surface by simply finding the value of $\frac{1}{x}$ in the series $\left(\frac{1}{x} - \frac{1}{x^2}\right) + \left(\frac{1}{x^3} - \frac{1}{x^4}\right) +$ &c., which will be readily seen by the figure to represent the reflections $e' r'$, $e'' r''$, $e''' r'''$, &c. when we put $\frac{1}{x}$ for the proportion of light first reflected at g. Of this series we know the sum, it being the difference between the whole reflection and that for the first surface. Putting $s =$ this sum, we have $\frac{1}{x} = \frac{s}{1-s}$.

This value of $\frac{1}{x}$ requires still to be corrected for the quantity lost in the glass and dispersed; but if we divide the *whole* loss proportionally between the light which should be transmitted and reflected at g from the series, we may make use of this proportion without any sensible error, as the difference between it and the true quantity is trifling in comparison to the limit which must be allowed for error in the measurements.

When we know the law of the variation in the loss and reflection, it would be better to use the calculated quantities, in applying them to another course of experiments than the experimental ones, as we then avoid the liability to twofold errors. The formula for the first surface we have already, and I now proceed to discuss that for the absorption or loss. The formula for finding the loss of light sustained in passing through any thickness of a transparent medium is this; if a part p of the whole light passes through a thickness a, then for any other thickness $n\,a$, the proportion passing through will be p^n. Now, if upon a review of the measurements, we can agree upon the quantity as lost at a perpendicular incidence, we know the proportion passing through the known thickness, and for every other incidence, the proportion passing through, would be p^n, where n is the tabular secant of the angle of refraction. If we take 6.4 as lost in every 100 in the crown glass of the experiments, 2.3 as lost in the plate, and 4.9 as lost in the flint, when incident perpendicularly, we have the calculated loss on the light, which *traverses the thickness* of the glass, as per the following table:

Calculation of the Light which would be lost in glass at different incidences from the differences in thickness.

Incidence.	Crown.	Plate.	Flint.
10°	6.45	2.32	4.96
20	6.57	2.37	5.05
30	6.77	2.45	5.20
40	7.01	2.55	5.37
50	7.27	2.66	5.56
60	7.56	2.79	5.75
70	7.78	2.84	5.92

On a first view of this table we see that we cannot avail ourselves of it. Though there is no doubt of the correctness of the formula for different thicknesses when the incidence is perpendicular to the first surface, it appears that we cannot compare different incidences together; and it shows, that the modification impressed upon light even affects its disposition to be absorbed in singly refracting substances, which I believe has never been before discovered. Though the liability to double error, arising from comparing two sets of measurements, prevents us ascertaining the law of the variation, yet the small loss at the highest incidence in every set leaves no doubt of the fact and its cause. This cause affects also the reflection at the second surface; and, therefore, *any law for the second surface of plates must have this effect considered as an essential part.* The only correct method is to measure the light reflected with prisms, where the light at each incidence enters and emerges perpendicularly to the surface; but this will require time and labour which I shall not be able to devote to it.

In the following table I have calculated the first reflection at the second surface from the series $\frac{1}{x} - \frac{1}{x^2} + \frac{1}{x^3} - \frac{1}{x^4} + \frac{1}{x^5}$, &c. adding the proportion of loss from the measurements. We see that from it we have no reason to reject the law, that *the reflective power at the second surface is equal to that at the first, when the incident and refracted rays have the same angular deviations.*

Table for the first reflection at the second surface of glass plates, calculated from the measurements at both surfaces.

	Crown.				Plate.				Flint.			
Incidence.	Value of $\frac{1}{x}$	Or of every 100 incidt.	Loss to be ad	1st reflect. at 2d surface.	Value of $\frac{1}{x}$	Or of every 100 incidt.	Loss to be ad	1st reflect at 2d surface.	Value of $\frac{1}{x}$	Or of every 100 incidt.	Loss to be ad	1st reflect at 2d surface.
10°	$\frac{1}{31.24}$	3.20	.22	3.42	$\frac{1}{28.86}$	3.46	.08	3.54	$\frac{1}{21.63}$	4.62	.25	4.87
20	$\frac{1}{31.05}$	3.22			$\frac{1}{28.87}$	3.46			$\frac{1}{22.67}$	4.41		
30	$\frac{1}{29.41}$	3.40	.28	3.68	$\frac{1}{28.58}$	3.49	.07	3.56	$\frac{1}{19.97}$	5.00	.25	5.25
40	$\frac{1}{25.60}$	3.90			$\frac{1}{26.20}$	3.81	.09	3.90	$\frac{1}{18.28}$	5.47		
50	$\frac{1}{23.15}$	4.31	.44	4.75	$\frac{1}{23.01}$	4.34	.12	4.46	$\frac{1}{16.88}$	5.92	.29	6.21
60	$\frac{1}{17.37}$	5.75			$\frac{1}{15.88}$	6.29	.17	6.46	$\frac{1}{12.73}$	7.85		
70	$\frac{1}{8.23}$	12.15	.92	13.07	$\frac{1}{7.30}$	13.69	.17	13.86	$\frac{1}{6.49}$	15.40	.31	15.71
80	$\frac{1}{5.29}$	16.89										

The irregularity in these measurements for both surfaces is more than I expected to have found it, and I intend trying a few of them again; and hope, in the next Number of this *Journal*, to be able to give them, with also a further analysis of the law for the first surface, and an examination of the connection between the values of the constants in the formula, and the physical properties of *solid* transparent bodies.

It will have been observed, that, as in metal, so in glass, only *a portion* of the light is reflected, even when the incidence is the greatest possible; and the quantity $r - x$ being the co-versed sine of incidence, may be examined as a function of the sine and cosine, or of the parallel and perpendicular motion of the rays of light to the surface.

I must here beg to correct an omission in my former paper on the reflective power of metals. When speaking of the general opinion being, that all substances reflected light most copiously when incident most obliquely, I ought to have mentioned the exception as to bodies of rough surface, which has been known since Bouguer's experiments with silver, Paris plaster, and paper

Art. VI.—*Mineralogical, Geological, and Chemical Notices.* Communicated by Dr Charles Hartmann, of Blankenburg, on the Harz, M. W. S. &c.

1. *On the Natural not Oxyded combinations of Antimony and Arsenic.* Extract from a paper of Professor Henry Rose of Berlin, in the xv. vol. of Poggendorff's *Annalen der Physic und Chemie.*—According to Professor H. Rose the following minerals belong to this class of substances.

a. *Zinkenite* from the Wolfsberg in the eastern Harz. The analysis of this ore is already communicated in No. xii. of this *Journal,* and the external description from Professor Gustavus Rose, in No. xi. The composition is the following:

Sulphur,	22.58
Antimony,	44.39
Lead,	31.84
Copper,	0.42
	99.23

Chemical formula $\overset{\prime\prime\prime}{F}b + \overset{\prime}{P}b$. *

b. *Miargyrite* (from ἄργυρος, silver, and μειων, less, because it contains less silver than the red silver,) or the hemiprismatic rubyblende of Mohs, (Haidinger, *Treatise,* vol. iii. p. 42.) from Bräunsdorff in Saxony. It contains:

Sulphur,	21.95
Antimony,	39.14
Silver	36.40
Copper,	1.06
Iron,	0.62
	99.17

Chemical formula $\overset{\prime\prime\prime}{F}b + \overset{\prime}{A}g$.

c. *Jamesonite* from Cornwall. The analysis of this ore is already communicated in No. xii. of this *Journal.* It contains:

Sulphur,	22.15
Antimony,	34.40
Lead,	40.75
Copper,	0.13
Iron,	2.30
	99.73

Chemic. Form. $2\,\overset{\prime\prime\prime}{F}b + 3\,\overset{\prime}{P}b$.

* The ′ on the letters of the formulæ signifies an atom of silver; in like manner . signifies an atom of oxygen.

d. *Plumose Grey Antimony* (Federerz) from Wolfsberg in the eastern Harz, in capillary crystals, consists of:

Sulphur,	19.72
Antimony,	31.04
Lead,	46.87
Iron	1 30
Zinc,	0.08
	99.01

Chemical Formula $\overset{\prime\prime\prime}{F}b + 2\,\overset{\prime}{P}b$.

e. *Red Silver*, a light variety from Joachimsthal in Bohemia, with the specific gravity = 5.552. Composition:

Sulphur,	19.51
Antimony,	0.69
Arsenic,	15.09
Silver,	64.67
	99.96

Arsenic and Antimony are isomorphous in the red silver.

f. *Brittle Silver Glance* from Schemnitz in Hungary, crystallized in six-sided prisms and with a specific gravity = 6.275 Composition:

Sulphur,	16.42
Antimony,	14.68
Silver,	68.54
Copper,	0.64
	100.28

Chemical Formula $\overset{\prime\prime\prime}{F}b + 6\,\overset{\prime}{A}g$.

g. *Bournonite* from the Pfaffenberg Mine in the eastern Harz, with the following composition:

Sulphur,	20.31
Antimony,	26.28
Lead,	40.84
Copper,	12.65
	100.08

Chemic. Form. $\underline{\overset{\prime}{C}u}^3\,\overset{\prime\prime\prime}{F}b + 2\,\overset{\prime}{P}b^3\,\overset{\prime\prime\prime}{F}b$.

h. *Polybasite* (from πολύς, much, and βάσις, basis, because it contains the greatest quantity of the basis,) a new species. This mineral was confounded with the brittle silver glance. The external description is given by Professor G. Rose. Crystals: Regular six-sided prisms, ordinary, low, and tabular, terminated by planes, which are perpendicular to the axis. The surface of the lateral planes is streaked across, the surface of the terminal planes parallel to the planes of a equilateral triangle, or parallel to the alternate terminal edges of the six-sided prism. In consequence of this the crystals must be rhombohedral. Cleavage is not observable; fracture uneven; colour iron black; lustre metallic; streak unchanged; sectile. Hardness = 2.0...2.5. spec. gr. of the variety from Guarisamey in Mexico = 6.214. Chemical composition:

Sulphur,	17.04
Antimony,	5.09
Arsenic,	3.74
Silver,	64.29
Copper,	9.93
Iron,	0.06
	100.15

Chem. Form. $\underline{\acute{Cu}}^{9} \left\{ \begin{matrix} \overset{'''}{Fb} \\ \overset{'''}{As} \end{matrix} \right\} + 4\,Ag^{9} \left\{ \begin{matrix} \overset{'''}{Fb} \\ \overset{'''}{As} \end{matrix} \right.$.

The polybasite is found crystallized, massive, and disseminated in silver veins, in Guanaxuato and Guarisamey, in Mexico, and in the Mine Morgenstern at Freiberg in Saxony.

i. *Grey Copper* or *Fahlerz*. Composition:

Varieties.	Sulphur.	Antimony.	Arsenic.	Iron.	Zinc.	Silver.	Copper.
1. From St Marie aux Mines in Alsace,	26.83	12.46	10.19	4.66	3.69	0.60	40.60
2. Gersdorf near Freiberg,	26.33	16.52	7.21	4.89	2.76	2.37	38.63
3. Kapnik in Hungary,	25.77	23.94	2.88	0.86	7.29	0.62	37.98
4. Dillenburg in Nassau,	25.03	25.27	2.26	1.52	6.85	0.83	38.42

5. Mine Zidda at Clausthal,	24.73	28.24	——	2.27	5.55	4.97	34.48
6. Mine Wenzel near Wolfach, Baden,	23.52	26.63	3.72	3.10	——	17.71	25.23
7. Mine Kabacht, near Freiberg,	21·17	24.63	——	5·98	0.99	31.29	14.81

When sulphuret of antimony and sulphuret of arsenic is signed with $\underline{\overset{\prime\prime\prime}{R}}$, sulphuret of iron and sulphuret of zinc with $\acute{R}$, and sulphuret of copper with $\underline{\acute{R}}$, the composition of the varieties 1—4, which contains only a small quantity of silver, may be expressed by

$$\acute{R}^4 \underline{\overset{\prime\prime\prime}{R}} + 2 \underline{\acute{R}}^4 \underline{\overset{\prime\prime\prime}{R}}.$$

The composition of the varieties which contains silver is more difficult to explain.

k. *Nickeliferous Grey Antimony.* This consists of:

Sulphur,	15.98
Antimony,	55.76
Nickel,	27.36
	99.10

Formula $Ni\ S^2 + Ni\ Sb^2$.

2. *The Atomic Weight of the Lithium,* Mr R. Hermann of Moscau has found, oxygen = 100, = 152.1.—Poggendorff's *Annalen,* vol. xv. p. 483.

3. The same chemist has detected under the radiated talc from the Ural a new species called *Pyrophyllite.* Before the blowpipe it exfoliates flabelliform to a great mass. The chemical composition is the following

Silica,	- -	59.79
Alumina,	-	29.46
Magnesia,	-	4.00
Oxide of iron,	-	1.80
Water,	-	5.62
		100.67

This corresponds to the formula $\dot{M}^3\, \ddot{Si}^2 + 3\, \overset{\cdots}{\underline{Al}}^3\, \ddot{Si}^6 + 10\, \dot{\underline{H}}$.

Poggendorff, vol. xv. p. 592.

4. In a potter's furnace at Oranienburg near Berlin, Professor Mitscherlich has found artificial crystals of oxide of iron which corresponds to tabular forms from the rhombohedron and the terminal face perpendicular to the axis.—Poggendorff, vol. xv. p. 630.

5. The *atomic weight of Titanium* is found by Professor H. Rose of Berlin,—Oxygen = 100, = 303.686; and according to *Dumas*, = 353.554.—Poggendorff, vol. xv. p. 145.

6. Professor Gustavus Rose of Berlin has found that the glassy feldspar from Laachek on the Rhine, and from Vesuvius, forms a distinct species. The observed crystals have nearly all the faces of the Adularia, and the same parallelism of edges. Twin-crystals like them, Fig. 80 and 81 from Moh's *Treatise on Mineralogy*, vol. ii. indicate that the edge between *o* and *M* and the face *P*, have not the same inclination to the axis. Professor Rose gives the following measurements of the angles:

T on $T = 119° 21'$.	x on $P = 129° 26'$.
P on $T = 112° 19'$.	x on $o = 153° 20'$.
T on $y = 134° 34'$.	M on $n = 134° 43'$.
P on $M = 89° 59', 5$.	P on $o = 124° 41'$.

The vertical axis is inclined to the fore horizontal under an angle of 88°, 56′.

The specific gravity is found by Professor Rose, on the variety from Vesuvius, - - = 2.553.
On a variety from the Eissel, - - = 2.519.
By Professor Breithaupt of Freiberg, - = 2.582.

According to the optical researches of Professor Mitscherlich it is in this respect different from the Adularia.

The chemical composition of the glassy feldspar is also different from that of the Adularia; and Professor Rose will prove this by an analysis after his return from a journey to the Ural Mountains, where he and Professor Ehrenberg of Berlin, (the Egyptian traveller,) accompany his excellency, the privy counsellor, Alexander de Humboldt. These notices on the glassy feldspath, which he called *Ryakolithe*, (from ῥύαξ, lava, and λίθος, stone,) are only a part of a treatise on the minerals of the feldspath family.—Poggendorff, vol. xv. p. 193.

7. Analysis of the *titaniferous oxidulated iron*, or *iron sand* from Egersund in Norway, by Professor H. Rose.

Oxyde of iron,	42.70
Protoxide of iron,	1357
Titanic acid,	43.73
	100.00

Formula, $3\,F^2\,\ddot{T}i + 4\,\dddot{F}^2\,\ddot{T}i^3$

Poggendorff, vol. xv. p. 276.

8. Analysis of the *Scheererite* or *Naphtaline résineuse prismatique*, from Uznach, near St Gallen, in Switzerland, by Macaire-Prinsep:

Carbon,	73.0	or	1 atom.
Hydrogen,	24.0	or	2 atoms.

Poggendorff, vol. xv. p. 298.

9. *Okenite*, a new mineral species, detected by Professor Kobell of Munich, among the zeolitic minerals from Greenland, and named in honour of the celebrated naturalist, Professor Oken of Munich.

The mineral is found at Kudlisat in Disco Island, and forms an amygdaloid of a fibrous or thin radiated structure. Colour white, yellowish and bluish-white; translucent; lustre pearly. Hardness between that of feldspar and fluor; spec. gravity = 2.28.

Before the blowpipe it melts with intumescence into a white enamel. Composition:—

Silica,	55.64
Lime,	26.59
Water with a little ammonia,	17.00
Alumina with a little oxide of iron,	0.53
	99.76

Formula $C\,S^4 + 2\,Ag$.

Kastner's *Archiv.* vol. xiv. p. 333.

10. *Analysis of the Nickel-Glance,* from the mine Albertine, near Harzgerode, on the Harz, (See No. xviii. of this *Journal,* p. 364.) by Mr Bley of Berenburg.

Arsenic,	35.635
Sulphur,	22.581
Nickel,	23.613
Cobalt,	00.444
Iron,	9.282
Silica,	0.750
Moisture,	7.500
	99.805

This result corresponds to the formula :

$$2\left\{\begin{matrix} Te \\ Ni \end{matrix}\right\} S^2 + 3\,Ni\,AS^2$$

11. *Seleniuret of Silver* is now found in greater massive specimens at Tilkerode in the Harz, and not merely in thin veins in the seleniuret of lead.

12. *Pure Selenium* is obtained in the lead and silver works near Harzgerode, in the eastern Harz, according to the method of Professor Mitscherlich, from the seleniuret of lead in very great masses, and sells in the mining-factory at Harzgerode, for four Louis d'ors the ounce.

13. The system of crystallization of the *Zinkenite* from Wolfsberg in the eastern Harz, is prismatic and not rhombohedral, as I am convinced from an examination of fine specimens of this mineral found this summer. Mr Haidinger, " *Library of Useful Knowledge,*" makes the crystals rhombohedral.

14. In the neighbourship of the old castle of Reinstein near Blankenburg, which stands on the top of a picturesque series of rocks which belongs to the green-sand, or Guadersandstein formation, in a sandland, there have been found this summer very fine and long *vitreous tubes,* (Blitzröhren in Germany.) From a trunk in the upper part, two branches go off, some of

which are ten feet long, and from these proceed three little branches.

15. *Geological Map of the north-western part of Germany, in twenty-four leaves*, projected by Professor Frederic Hoffmann, (now in Berlin.) Berlin, by Simon Schropp and Company.—This beautiful map, which contains the countries between the Elbe and the Rhine, and from the Thuringia mountains to the great plain north of Hanover, with all details in the limiting of the rocks, is without doubt the best that exists, and better than the great maps of England by Greenough and Smith. A general geological map of the above countries, accompanied by sections and a "geological description" of that part of Germany in three volumes, also by Professor Hoffmann, will appear soon, and the whole will form one of the most important geological works. It may be recommended to the British geologist on account of the formations of Germany being similar to those in England.

16. Privy Counsellor Dr Karsten of Berlin has begun a new series of his very valuable *Archiv. für Bergbau und Hüttenwesen*," (the first is finished with the 20th vol.) under the title, "*Archiv. für Mineralogie, Geognosie, Bergbau u Hüttenkunde*," or "Archives of Mineralogy, Geology, Mining and Metallurgy." The first number contains, besides many other valuable papers, a geological description of the Scottish islands of Skye and Egg, by the Barons of Dechen and of Oeynhausen.

17. An enlarged and improved German translation of the third edition of Bakewell's *Introduction to Geology*, has just appeared, by the author of these notices.

Art. VII.—*Experiments on Ocular Spectra produced by the action of the Sun's Light on the Retina.* By Sir Isaac Newton.

The following very interesting experiments, though communicated in a letter to Mr Locke on the 30th June 1691, and made many years before, were never published by their author, and were first given to the world in 1830, in Lord King's *Life* *of Locke.* The similarity of some of the results to those published by the Editor of this work in the article Accidental Colours, in the *Edinburgh Encyclopædia*, is very remarkable. Although we have made numerous experiments on ocular spectra produced by strong lights, we have never been able to call up the impression by the influence of the imagination, in the manner described in the following paper, which is an extract from one of Newton's Letters to Mr Locke.

"The observation you mention in Mr Boyle's book of colours, I once made upon myself with the hazard of my eyes. The manner was this; I looked a very little while upon the sun in the looking-glass with my right eye, and then turned my eyes into a dark corner of my chamber, and winked, to observe the impression made, and the circles of colours which encompassed it, and how they decayed by degrees, and at last vanished. This I repeated a second and a third time. At the third time, when the phantasm of light and colours about it were almost vanished, intending my fancy upon them to see their last appearance, I found, to my amazement, that they began to return, and by little and little to become as lively and vivid as when I had newly looked upon the sun. But when I ceased to intend my fancy upon them, they vanished again. After this, I found, that, as often as I went into the dark, and intended my mind upon them, as when a man looks earnestly to see any thing which is difficult to be seen, I could make the phantasm return without looking any more upon the sun; and the oftener I made it return, the more easily I could make it return again. And at length, by repeating this without looking any more upon the sun, I made such an impression on my eye, that, if I looked upon the clouds, or a book, or any bright object, I saw upon it a round bright spot of light like the sun, and, which is still stranger, though I looked upon the sun with my right eye only, and not with my left, yet my fancy began to make an impression upon my left eye, as well as upon my right. For if I shut my right eye, or looked upon a book or the clouds with my left eye, I could see the spectrum of the sun almost as plain as with my right eye,* if I did but intend my fancy a little while upon it; for at first, if I shut my right eye, and looked with my left, the spectrum of the sun did not

* Though seen *when the left eye was open,* it may still have been the spectrum on the right eye.—Ed.

appear till I intended my fancy upon it; but by repeating, this appeared every time more easily. And now, in a few hours time, I had brought my eyes to such a pass, that I could look upon no bright object with either eye, but I saw the sun before me, so that I durst neither write nor read; but to recover the use of my eyes, shut myself up in my chamber made dark, for three days together, and used all means to divert my imagination from the sun. For if I thought upon him, I presently saw his picture, though I was in the dark. But by keeping in the dark, and employing my mind about other things, I began in three or four days to have some use of my eyes again; and, by forebearing to look upon bright objects, recovered them pretty well, though not so well, but that, for some months after the spectrum of the sun began to return as often as I began to meditate upon the phenomena, even though I lay in bed at midnight with my curtains drawn. But now I have been very well for many years, though I am apt to think, if I durst venture my eyes, I could still make the phantasm return by the power of my fancy. This story I tell you, to let you understand, that in the observation related by Mr Boyle, the man's fancy probably concurred with the impression made by the sun's light, to produce that phantasm of the sun which he constantly saw in bright objects. And so your question about the cause of this phantasm involves another about the power of fancy, which, I must confess, is too hard a knot for me to untie. To place this effect in a constant motion is hard, because the sun ought then to appear perpetually. It seems rather to consist in a disposition of the sensorium to move the imagination strongly, and to be easily moved, both by the imagination and by the light, as often as bright objects are looked upon.

ART. VIII.—*On the Mean Temperature of* TWENTY-NINE *different places in the State of New York for* 1829.

IN No. xvi. of this *Journal*, and in No. ii. of the New Series, we have given abstracts of the Returns of Meteorological observations made to the Regents of the University of the State of New York for 1826 and 1828. Having been favoured by Mr Greig of Canandaigua with the Report for 1829, we shall proceed to give an abstract of the observations it contains.

The observations were made at the same hours as formerly, viz. 6h A. M. three hours P. M. and an hour after sunset.

The following table contains the position of the places of observation, and their heights above the sea.

	N. Lat.	W. Long.	Elevation of place of ob. above tide.
Albany, - -	42° 39′	73° 47′	130
Auburn, - -	42 55	76 35	650
Cambridge, (Washington co.)	43 02	73 22	
Canandaigua, - -	42 50	77 15	
Cherry-Valley, - -	42 48	75 06	1335
Clinton, - -	41 00	72 19	
Dutchess, - - -	41 41	73 54	
Erasmus-Hall, - -	40 37	73 58	
Franklin, - -	42 30	77 13½	
Hamilton, - -	42 48	75 32	1127
Hartwick, - -	42 38	75 04	
Hudson, - -	42 12	73 45	150
Johnstown, - -	43 00	74 08	
Kingston, - -	41 55	74 06	188
Lansingburgh, - -	42 48	73 38	30
Lowville, - -	43 47	75 25	800
Middlebury, - -	42 49	78 10	800
Montgomery, - -	41 32	74 10	
North Salem, - -	41 20	73 37	
Onondaga, - -	43 02	76 10	410
Oxford, - -	42 26	75 38	961
Pompey, - -	42 56	76 05	1300
Schenectady, - -	42 48	73 56	225
St Lawrence, - -	44 40	75 00	394
Union Hall, - -	40 41	73 56	
Utica, - -	43 06	75 12	473
Washington, - -	43 08	73 17	
Newburgh, - -	41 30	74 05	150

In 1826 the mean temp. of 10 of the above places was 49°. 4
In 1828 the mean temp. of twenty-three places was 49 .99
In 1829 the mean temp. of twenty-eight places is only 46 .45

The following table contains the mean monthly temperature of the twenty-eight places above-mentioned, the annual mean temperature, the annual range, and the highest and lowest during the year.

Academies.	Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Annual mean.	Highest year.	Lowest year.	Annual range.
Albany -	23.47	19.96	32.92	48.10	64.45	68.12	68.57	68.97	57.04	51.27	39.19	36.45	48.20	92	—10	102
Auburn, -	23.05	19.26	29.97	44.27	60.03	65.61	67.29	67.16	55.01	50.38	36.29	38.06	46.36	91	— 5	96
Cambridge, -	22.08	17.57	30.05	45.38	59.47	65.66	67.73	66.23	54.29	48.19	37.50	36.53	45.89	91	—25	116
Canandaigua,	24.09	19.61	30.71	46.32	60.18	65.49	64.19	67.93	55.13	48.27	36.29	36.15	46.22	90	0	90
Cherry-Valley,	19.70	15.74	26.65	41.67	58.59	64.75	65.10	65.14	53 52	47.00	34.04	40.06	44.33	91	—15	105
Clinton, -	29.89	25.84	33.14	44.49	54.85	62.10	65.68	68.19	59.17	50.30	42.96	41.03	48.14	84	— 1	85
Dutchess, -	28.45	25.46	36.63	52.76	64.29	70.29	73.29	72.92	61.79	55.95	41.21	38.83	51.82	94	— 6	100
Erasmus-Hall,	30.26	25.93	36.25	48.68	59.98	66.45	69.62	70.65	59.75	52.09	44.27	42.07	50.50	87	4	83
Franklin, -	20.56	12.12	29.57	44.87	60.67	65.12	66.06	67.12	52.85	48.32	33.47	34.69	44.61	96	—14	110
Hamilton, -	25.18	20.20	28.41	44.42	55.13	62.92	63.86	64.70	53.06	48.16	34.38	33.34	44.48	93	—20	113
Hartwick, -	22.52	19.25	30.24	42.24	58.08	63.60	65.41	65.30	53.95	48.65	36.09	40.61	45.49	91	—15	106
Hudson, -	23.55	19.86	32.08	48.56	65.38	71.67	69.58	71.22	60.65	51.09	38.25	36.68	49.05	93	— 7	100
Johnstown, -	20.58	18.35	29.98	42.95	61.26	64.76	65.98	65.21	55.51	52.19	38.18	37.33	46.02	89	—11	100
Kingston, -	25.96	21.77	30.71	47.92	62.69	67.92	69.20	69.80	58.77	51.92	37.56	36.96	48.43	96	—12	108
Lansingburgh,	22.12	18.26	30.84	45.92	63.05	69.55	69.52	70.30	57.41	50.78	38.23	36.44	47.70	94	—17	111
Lowville, -	16.76	15.68	26.67	43.20	58.50	64.17	65.27	64.59	52.47	48.03	32 86	32.34	43.38	92	—28	120
Middlebury, -	25,07	19.59	29.86	46.81	59.24	64.80	65.71	66.26	55.12	50.87	35.39	37.57	46.35	94	— 4	98
Montgomery,	26.64	21.96	33.24	46.82	63.67	66.79	69.94	71.63	59.21	50.58	39.46	38.45	40.03	98	— 6	104
North-Salem,	28.45	22.87	34.96	49.10	62.92	69.01	72.57	72.49	62.82	54.96	38.90	38.72	50.64	86	— 3	89
Newburgh, -	26.92	21.62	34.24	46.97	60.66	66.05	69.77	70.16	58.76	51.98	38.95	37.87	48.66	95	— 3	98
Onondaga, -	22.82	22.80	31.60	49.69	63.98	65.73	67.16	68.21	55.82	51.96	37.00	36.97	47.81	91	— 8	99
Oxford, -	22.87	18.93	29.57	43.81	58.65	64.21	64.04	66.84	53.88	47.63	35.36	34.16	44.99	98	—15	113
Pompey, -	20.09	16.60	27.28	41.65	57.88	62.92	64.80	65.71	51.43	46.30	32.27	31.87	43.23	88	—11	99
Schenectady,	23.07	18.78	31.95	46.67	61.36	67.80	64.65	68.23	55.37	50.42	37.84	35.16	46.77	91	— 9	100
St Lawrence,	22.56	13.16	27.32	42.73	58.83	63.56	65.89	65.51	53.95	49.09	34.01	32.11	44.06	89	—23	112
Union-Hall,	27.74	23.15	33.70	46.63	57.77	65.56	68.91	69.92	57.42	50.67	40.52	40.14	48.51	90	0	90
Utica, -	21.21	18.12	30.14	43.14	60.35	62 33	65.77	64.53	52.05	47.81	34.95	33.64	44.50	86	—11	97
Washington,	21.07	15.50	30.19	42.98	57.73	64.60	67.30	66.76	53.65	45.62	35.19	33.65	44.52	89	—28	117

The following table shows the quantity of rain and snow which fell in the state of New York in 1829.

	In.		In.
Albany, -	38.07	Lansingburgh, -	38.34
Auburn, -	30.54	Lowville, -	28.07
Cambridge, Washington	39.04	Middlebury, -	29.80
Canandaigua, -	30.20	Montgomery, -	31.45
Cherry-Valley, -	39.93	Newburgh, -	32.54
Clinton, -	42.56	Onondaga, -	27.01
Erasmus-Hall, -	48.62	Oxford, -	36.71
Franklin, -	26.16	Pompey, -	27.23
Hamilton, -	33.26	Schenectady -	34.85
Hartwick, -	40.83	St Lawrence, -	27.71
Hudson, -	33.47	Union-Hall, -	45.83
Johnstown, -	36.59	Utica, -	36.16
Kingston, -	38.99		

	Inches.
In 1826 the mean rain of nine places was	36.34
In 1828 the mean rain of twenty-five places was	36.74
In 1829 the mean rain of twenty-five places was	34.88
Mean,	35.96

Miscellaneous Observations.

Aurora Borealis noticed.

Jan. 27, at Cambridge.

Jan. 30, at Cambridge.

Jan. 31, at Cambridge.

April 4, at Utica,

April 5, at Lowville.

April 8, at Lowville, resembling a bright cloud, and exhibiting near the horizon, a steady light of several hours' continuance.

May 29, at St Lawrence.

May 31, at Schenectady, Utica.

June 1, at Cambridge, Franklin, Middlebury, Pompey, Utica. Very brilliant, appearing in distinct arcs, one above the other, the highest subtending an angle of about 75°. (St Lawrence.)

June 2, at Cambridge, Utica.

June 7, at Schenectady, brilliant all night.

June 14, at St Lawrence.

June 21, at Poughkeepsie.

August 25, at Poughkeepsie.

August 26, brilliant, at Auburn, Cambridge, Schenectady, Utica.

Sept. 18, at Albany, Poughkeepsie, Utica.

Sept. 19, at Albany, Clinton. Describing an arc on the plane of the horizon, of about 65°, and rising in distinct spires towards the zenith. Noticed between 8 and 11 P. M. at St Lawrence.

Sept. 26, About half-past 9 P. M., a brilliant aurora borealis was observed. It was composed of beams occasionally shooting up as high as the pole star from a point of the horizon, a little to the west of the true north, at Albany.

Oct. 21, brilliant, at Cambridge and Utica.

Oct. 24, at St Lawrence.

Oct. 27, About 8 P. M., three perpendicular columns of light appeared in the north, the central one extending about 20°, and the two external about 15° above the horizon, at Delaware.

Nov. 9. At Lowville, with brilliant and brisk coruscations. Half-past 4 A. M.

Nov. 19, at St Lawrence. Faint, with occasional spires extending towards the zenith.

Dec. 19, at Schenectady.

Dec. 28, at evening, between 7 and 8 o'clock, several persons observed a light in the west, a little south, proceeding in rays from a centre, and shooting sparks so bright, that the trees could be distinctly seen at Hartwick. Aurora borealis very brilliant at North-Salem.

Haloes, &c. —March 15. An extraordinary halo round the sun at half-past 5 P. M. Next to the sun was a coloured circle, whose diameter was equal to a chord of 30° of the sensible horizon. At the north, south, and east points of this circle were luminous spots of considerable magnitude, the west part of the circle was below the horizon. North and south of the sun (one on each side) were two arcs of a circle, answering to a radius, about twice as great as that of the inner circle. These larger arcs were coloured, as the rainbow, the red being next to the sun. East of the sun or above it, and at the same distance therefrom, as the north and south arcs, was another arc coloured like them, (though less bright) but manifestly convex towards the sun. Directly between this last mentioned arc and the sun, and externally tangent to the eastern part of the inner circle, was a luminous and broad parabolick curve. Lowville.

April 8. Halo round the moon, at Pompey, Utica.

May 15. Bright lunar halo, at Utica.

June 8. Brilliant circle round the moon, at 11 P. M., with a mock moon on the right of the circle, at Utica.

Sept. 2. A brightparhelion seen near sunset, about 8° from the sun, at Erasmus-Hall.

Sept. 9. A beautiful large halo round the moon, at Delaware.

Nov. 6. Large lunar halo, at Lowville.

Dec. 2. Lunar halo, at Lowville.

Dec. 30. Lunar halo, at Cazenovia.

Meteors.—June 14. A splendid meteor noticed about 8 P. M. at Delaware. Bright meteors seen at Hartwick.

Oct. 2. A meteor passed from north to south, 6 P. M., at Erasmus-Hall. Meteors seen S. E. before dark at Hartwick.

Oct. 5. Brilliant meteors passed N. W. to S. E. 6½ P. M., a little east of the meridian, apparent size about 9 inches, at Erasmus-Hall.

Oct. 24. A brilliant meteor seen in the east at an angle of 40° above the horizon. It moved south, and in a few seconds disappeared. Lansinburgh. A bright meteor passed at 10 P. M., easterly of the village from north-east to south-west. Utica.

Storms, &c.—On Sunday, the 12th July, at three o'clock, while the bells were ringing for church, a storm of wind and rain, accompanied with some thunder and lightning, from the south-west, passed over the city. This storm is worthy of notice, particularly in relation to an observation made by Dr Franklin, that in this country all the N. E. storms begin first in point of time in the south-west; or, in other words, that the progress of the storm, over the face of the country, is in a direction opposite to that of the wind at the time. According to the New York papers, the above-mentioned storm was much more violent in that city than in Albany; and began some time after the preaching of the sermon in the different churches had commenced. It must therefore have happened between three quarters of an hour, and an hour and a-half, later at New York than at Albany. Dr Franklin concluded that the storms observed by him, receded in this manner about one hundred miles in an hour. [See Franklin's *Works*, vol. 3, page 284.] Albany.

Sept. 25. Two thunder storms, P. M. On the same afternoon, the papers mentioned a violent thunder storm passing over part of Dutchess county, which killed a number of sheep. —(Albany.)

Some time in the latter part of September, there was a violent *tornado* in the north part of this and the adjoining town of Fenner. Its course was about S. E. by S., and its extent ten or twelve miles. Many out-houses were overturned or unroofed, trees, three feet in diameter, twisted off, and large limbs hurled five or six hundred feet in the air. Its path, the whole of the above distance, was visible by its effects when I visited it some two months afterwards. In some parts, it urged its way through stripes of woodland, leaving nothing but an occasional tree dismembered of its branches,—in others, it was more elevated, as was determined by the limbs of trees and dust carried with it, and consequently less destructive. It succeeded a sudden change of the wind from south to N. W. by west.—(Cazenovia.)

Rain and Snow.—June 22. During 28 days, less than $\frac{1}{4}$ of an inch of rain has fallen.—(Canandaigua.)

Oct. 5. A rainbow appeared in the N. E. from clouds which brought no rain.—(Delaware.)

Feb. 25. Snow three feet deep in the south-west part of this town.—(Lowville.)

Winds.—The situation of Homer village, in a long and somewhat deep valley, produces a fluctuation and incessant vacillation of the currents of air, such as I never witnessed in any other place.

To give an example: in the morn or evening, it would be no unusual occurrence, in looking at half a dozen vanes, or the smoke ascending from as many chimneys, to see them veering in as many different directions at the same moment; and all this within thirty rods distance of the observer. Such a variety may be seen almost any day, and, occasionally, at any time in the day; but is most frequent near the time of sunset or sunrise. So that when the current is strong and steady, perhaps all the day, on the neighbouring hills, either east or west of us, a person in this vale would be utterly unable to pronounce from what point of the compass the wind was that moment blowing, in a general course, in this section of the country.

From four years' experience and much observation on this subject, in this vicinity, I am well assured, that while upon the high grounds in this region the wind is blowing steadily from the east or west, the currents are here between S. E. and S. W., or between N. E. and N. W.

In the greatest severities of weather in winter, even when the mercury stands below zero in Fahrenheit's scale, the movement of the air in this valley is almost imperceptible; but when perceived in the morning, its course is almost always from the S. or S. W. Such a phenomenon I never noticed in any other place.—(Cortland Academy.)

The prevalence of westerly, and the entire absence of northerly winds at Utica, is explained by an attention to the features of the country. Our observation must first be directed to the vast extent of level country surrounding and stretching far west and north of lake Oneida; over which the prevailing winds of our country, the west and north-west, sweep without any particular obstruction, till they are compressed by the high hills of the Black river country on the north, and the hills on the westerly and southerly side of this lake, approaching near each other in the towns of Floyd and Rome, or Whitesborough; and forming a deep opening or gap, through which runs the Mohawk valley, in a direction a few degrees south of east. Immediately on passing this gap, opens to the south, the extensive vale of Oriskany and Saquoit, which may be termed the Oriskany basin;—bounded on the west by the high hills towards Augusta, and on the south by a chain of hills extending from Madison county, in an easterly direction, to the river

Mohawk, at a point a few miles below the village of Utica, and rising to an elevation of more than a thousand feet. The northerly side of this basin is bounded by the Floyd hills, which extend with a bold elevation along the margin of the Mohawk to some distance below Utica. The current of wind after entering the Oriskany valley spreads in a southerly direction, till on reaching the barrier on the south, it is reflected to the east, and continuing its course along the hills by the village of New-Hartford, it passes the village of Utica, situated almost in the south-eastern extremity of this basin, in an easterly direction; cutting the Mohawk valley a little transversely, till it passes through a second gap, a few miles below. Hence the winds, which in other parts of our country are called *north-west* winds, and which blow so uniformly from that direction when not particularly obstructed, as in this place, that they constitute a distinguishing feature in the climate of our country, are in this part of the Mohawk valley, almost unknown. For similar reasons a north-east wind is seldom observed.—(Utica.)

Temperature of Wells.—Examined in August, 51°.—(Albany.) In September, 51°.—(Albany.)

Examined Sept. 2, at 3 P. M., stood at 49° in a well eighteen feet deep, and the thermometer immersed ten feet beneath the surface of the ground. On the following morning, at sunrise, the thermometer in the same situation remained at 49°.—(Lowville.)

ART. IX.—*On the Direction of the Diluvial Wave in the Shetland Islands.* By S. HIBBERT, M. D., F. R. S. E., &c. Communicated by the Author.

IF we would estimate the more important direction of the diluvial wave, which, in sweeping over the less elevated lands of the British Islands, has dispersed an immense mass of clay and boulders far from their native beds, it has often struck me, that the most satisfactory investigations might be expected from the appearances presented in the north isles of Scotland, where the course of the wave would be less likely to be modified by such causes as must have conspired to change its direction in encountering the headlands of more southerly coasts. I regret that, when twelve or thirteen yesrs ago, I made a minute survey of the Shetland Islands, the geological questions which transported boulders involved had then little recommended themselves to the attention of geologists; whence I was less disposed to pay attention to such sites favourable to their deposit as were likely to convey the information which might be desired. At the same time, several diluvial indications were not quite lost upon me; and when Dr Buckland had made public his interesting speculations on this subject, I hastened to reperuse the notes which I had made four or five years previously. These I shall make the foundation of the present paper.

I would, however, previously observe, that if we here seek for the large diluvial deposits which are to be found in England or Scotland, we shall be much disappointed. Such beds of very deep depression as might be conceived favourable to the detention of transported fragments are concealed from human investigation by the deep *voes* or inroads of the ocean. It is therefore from the presence of the transported relics which more sparingly occur on the sides of declivities, or are strewed upon the higher plains, that we can infer the evidence of a diluvial wave, and can speculate upon the point of the compass whence it had its origin.

The following, then, are the facts which led me to infer the more important direction of the diluvial currents, which, as I have stated, swept over the less elevated lands of the British Islands;—I say *less elevated* lands, from the belief, that the wave failed in overtopping the summits of very lofty mountains.

In examining the Island of Papa Stour, situated on the west of the Shetland groupe, I was struck with the fact, that, though this small spot, of about two or three miles in length and breadth, is composed of sandstone and secondary porphyry, numerous fragments, even in the interior of the island, might be found of a peculiar and very beautiful hornblende schist and actinolite schist, which is no where to be met with in this archipelago, except at Hillswick Ness; a distance, when measured in a straight line across the Bay of Saint Magnus, of at least twelve miles. I also found, that, besides these fragments, relics of other distant primary rocks were strewed about, less easily, however, to be identified than those which I have described. At first I was inclined to believe that these transported materials might have been due to the inhabitants of Papa Stour, who brought them over as ballast, &c.; but this suspicion was not only discountenanced by the contradiction of the natives themselves, but was in other respects a most improbable conjecture, particularly from their being found in the centre of the islet, a mile at least from the water's edge, as well as from their quantity, which was far too considerable to be referred to such a cause. The observations made by Sir James Hall on the Scottish debacle, as well as those of Saussure on the detached boulders of the Jura Mountains, then suggested themselves to my mind, and I had the wish to record the fact; but, being aware that I had not hitherto pursued this particular investigation to the extent that it deserved, I waved for the time the intention I had formed. Nor, indeed, do I publish at the present day the detached observations which I had made, except as a hint to promote the research of some future visitor of the northern islands of Scotland.

On the supposition, then, that I am correct in referring these fragments to a removal by diluvial causes from Hillswick Ness across the Bay of St Magnus to the Island of Papa Stour, the diluvial wave which transported them a distance of twelve miles must have propelled them from a point of the compass bearing from their place of lodgment about N. 47° E; and hence, if we suppose a very distant prolongation of this line of bearing, it would nearly fall in with the north-westerly bounding line of the Norwegian coast. A suggestion of some importance is thus afforded for the future inquiry of the geologist, in connection with the curious fact, that no little share of the diluvial boulders observed on the coast of Yorkshire, has been assigned to the transported materials of Norwegian rocks.

Such are the phenomena which incline me to imagine, that the great diluvial wave which swept over the low elevations of the whole of Scotland and England had in the latitude of Shetland a north-easterly origin, or, in other words, that it had a south-westerly direction.

This conclusion may receive some additional support from another appearance which I have given in my Description of

Shetland, without, however, hazarding an explanation of it. I have there stated that about a mile or two to the north of the mansion of Lunna, on the east of Shetland, are to be found several remarkable detached rocks, named the Stones of Stefis, the largest of which was about twenty-three feet in height and ninety-six in circumference.—Now, regarding these astonishing fragments, I did not then venture to express a decided opinion; I merely observed, that, in the *first* place, they did not seem to have undergone any very distant removal, since they reposed on rocks of precisely a similar kind; that, *secondly*, they did not appear to have been loosened from rocks of a greater altitude; and, *thirdly*, that, if we were inclined to consider them as the detached remains of pre-existing masses, having escaped a decomposition by which the rest of the rock of which they had formed a part had been removed,—we should still be compelled to admit that a disintegrating process of this nature must to any great extent have long since ceased, since the gneiss in question was little prone to decomposition.

Such was the guarded language in which twelve years ago I described an appearance upon which I should now be disposed to pronounce with some degree of confidence. These immense boulders are to be found in an elevated situation upon a very narrow tongue of land, three or four miles in extent, which, having jutted out into the ocean in a north-easterly direction, would be opposed to the direct force of the diluvial wave. The extremity of the headland being much broken, an indication is thereby afforded of the site whence these stones have been dislodged, and by diluvial currents in a south-westerly direction hurried along. The distance, however, to which they have been detached cannot be estimated at the most more than a mile or two; nor is it reasonable to give to the diluvial power an almost incalculable force, or to conceive of its effects without assigning to it due limits. We must reflect that we are contemplating displaced fragments of the height of twenty-three feet, and of nearly a corresponding breadth.

But, besides these diluvial indications, many others might be found, though they are perhaps scarcely of equal weight with those which I have described.

On some parts, for instance, of the east coast of the Island of Yell, which is composed of gneiss, many large fragments may be detected of serpentine and euphotide, which have evidently been drifted some miles from the islands of Unst and Fetlar.

Again, in ascending the hill named Roeness Hill, composed of red granite, which has an elevation of 1447 feet above the level of the sea, I was struck with the immense quantity of boulders of a primary greenstone or trap which appear to have been removed from a site two or three miles off, and to have been rolled in a southerly or south-westerly direction up a gradual ascent of three or four miles.

On the summit of Hillswick Ness, also, we meet with a surprizing block, mantled from age with grey moss, which is composed of a granite removed from a rock, the nearest site of which is about two miles north. It is far too large to authorize us in supposing that it was transported from its bed and rolled up this high hill by the ancient inhabitants of the country. It is many feet in dimensions, and every thing which the ancient inhabitants were likely to effect would be to place it on an edge previously to the dedication of it to The Thunderer. This *Thorstone*, as it was anciently called, evidently owed its original displacement to diluvial torrents.

Lastly, I may remark, that it is possible to find in Shetland some fragments of stones which are strangers to the country, and which have probably been transported from foreign shores. This is the case with some detached boulders which we meet with in Soulam Voe, an unsheltered harbour open to the Northern Ocean. One of these, to the best of my remembrance, about three or four feet high, is a variety of granite, quite unknown in the country, which probably had its upright position given to it by human exertions. Its distant emigration even vulgar tradition has recognized, by affirming that it was thrown here by the devil, as he stood on some high hill in a neighbouring parish.

I shall not trouble the reader with any more proofs to show that the influence of diluvial currents was experienced in the Shetland Islands. I cannot, however, avoid remarking, that it would be interesting to inquire if indications of the presence of the diluvial wave, and of its south-westerly direction, are afforded in islands even north of Shetland. I have not visited Feroe, and can therefore only express my opinion in the affirmative, from the following remark of Landt in his account of this country. "Besides the large collections of stones," he observes, "already mentioned, which are occasionally found on the hills, there are seen sometimes in the vallies single stones, six, eight, or ten feet in diameter, but in places where it is impossible they could have fallen down from the hills. Such stones are found also here and there at a considerable height on the hills, where there is no other eminence in the neighbourhood from which they might have rolled down. On the sides of many of the hills, and particularly on the lower projecting declivites, there are often found great heaps of stones, among which there are some large ones; but it may be plainly perceived that these have been thrown down from the higher projections, in the fissures of which the rain water lodges, and when it freezes in winter it splits the rock by its expansion, and on a thaw taking place these fragments tumble down, and by their fall destroy the green plots below. But the stones thrown down in this manner are different from those before mentioned; for the latter have two sides, which stand at a right angle, or, at least, they have one or more flat surfaces, whereas the former are in general round."—Landt's *Description of the Feroe Islands.* Trans. London 1810, page 8.

The foregoing is a most interesting notice, and quite sufficient to excite the attention of the geologist who shall hereafter visit this archipelago. Nor will a similar inquiry in Iceland be probably less interesting.

Having thus endeavoured to show the probability that the great currents which deluged the British islands, as well as some parts of the continent, had in Shetland a north-easterly origin, or a south-westerly direction, I might, as a continuation to this account, endeavour to point out the modifications which the wave underwent in its direction during its progress farther south. This would, however, lead me to a very extended inquiry; nor have we yet sufficient data for the purpose. The modifications in the direction of the diluvial wave in Yorkshire have met with some notice by Mr Phillips; and I might mention, that, on the north-west coast of England, the numerous fragments of rocks which have been propelled from the neighbourhood of the Lakes in Cumberland, and dispersed over the low lands of Lancashire and Cheshire, show that the current in its progress through the Irish channel, after having encountered the various headlands of Scotland, England, and Ireland, has had its direction changed from a south-westerly to a south or south-easterly course. But, for the present, I quit the prosecution of these researches. It is possible I may resume them on another opportunity.

ART. X.—*Memoir on Barometric Instruments acting by Compression, considered particularly in their application to the Measurement of Heights; including some new Trigonometrical Determinations.* By JAMES D. FORBES, Esq. Communicated by the Author.

PART I.—*On the Defects of the Sympiesometer.*

MY former paper in this *Journal* for April 1829, upon Mr Adie's sympiesometer, contained the results of some long previous experiments, among the first, indeed, of an analytical character which I ever undertook in physical science. I considered that they carried with them sufficient proof of their general accuracy, from their bearing out so completely in their details the theory of an error, which in making them was not even remotely in view; but I obtained very speedily a confirmation of the results I had arrived at, from a source every way satisfactory. After my paper was in the press, Dr Brewster kindly communicated to me two very interesting letters from Professor Schumacher, the distinguished German astronomer, written more than eight years ago, and complaining of the very same defect as I had indicated. What is more remarkable, he suggests the identical mode of correction which I proposed in my former paper. This may be seen from the sketch in Plate I. Fig. 6, which is a fac-simile of Professor Schumacher's.

Having been induced to revive my old series of observations, which for some years had been laid aside, I was naturally desirous to re-examine the sympiesometer, and to verify them under a variety of circumstances, especially as they had been depreciatory of an instrument of acknowledged elegance and ingenuity. I need hardly say, that I had no reason to change my opinion as to the correctness of my original observations, otherwise I should, long ere this, have acknowledged my mistake; but the views which are to be developed in this paper would long since have been published, (indeed they were in some forwardness a year ago,) had not the investigation, at first apparently simple, extended itself so much on all hands, and, by being considered both in frequent practice and rigid theory, opened so many questions for solution, that, even had the observations been sufficiently accumulated for the purpose, I could not till now have found leisure to reduce and classify them.

Having got my sympiesometer put into thorough repair by Mr Adie, I commenced regular observations with it in May 1829, and since that time have made at least *two thousand* accurate and recorded observations, with proper data for comparison and correction. Perhaps it may be asked whether the instrument was worthy of so much labour, especially as the detection of its inaccuracies was the principal object of my inquiry? But it is a sufficient reply to observe, that I began to perceive germs of excellence in this species of barometer almost as soon as I recommenced my inquiries, and likewise the means of obviating its defects, so as to render the ascertainment of these an object of primary importance. The confined situation of the ravine which was the scene of my first experiments, was so far fortunately chosen, that it showed strongly the sources of error of which I was in search, (and which will be shown in the course of this memoir to have been independent of the defects of the spot for measurements by the common barometer); but experience proved, that in favourable situations, and under favourable circumstances, these errors, though never null, are often greatly reduced; and the want of portability in the mountain barometer, which, after much consideration, I begin to think an insurmountable evil, certainly reflects no small merit upon an instrument which could supply its place, with a degree of commodiousness to which I can bear ample testimony, having made it the constant companion of my hilly walks and geological excursions, alike under a midsummer sun, or among December snows. This alone, had its defects been more insurmountable in appearance than they are, would have furnished me with a sufficient motive for prosecuting my inquiries: it farther requires no stand, the steadiness of which in the barometer renders it a most cumbrous appendage, and it readily bears shocks which, from the weight of mercury in the latter, would at once prove fatal.

My extended experiments with the sympiesometer not only enabled me to observe the nature of the stations where it was best calculated to act, and in which the locality of my first observations was peculiarly unhappy, but they pointed out the practical artifices by which the errors might be reduced to a minimum. I found, as might be expected from the theory I developed in my former paper, that a free current of air of equable temperature, and the absence of reflected heat, was of paramount importance; hence the instrument performs much best on insulated summits and in cloudy weather. I have found an umbrella stuck into the ground by far the best support, furnishing a useful degree of shade from direct sunshine. When approaching a station I swing the instrument freely at some distance from my body, in order to permit it nearly to acquire an equality of temperature in its different parts, before fixing it for observation. The observations too, which will hereafter be detailed, had generally the very important advantage of having the horary variation between leaving and returning to the lower station, ascertained or corrected by the barometer. But these topics belong rather to another part of this memoir. Let us, in the first place, and before proceeding to details, notice briefly, for the benefit of those less familiar with barometers acting by compression, their history and most improved construction.

Considerably above a century ago, Dr Hooke seems to have thought of applying the error of the air thermometer occasioned by pressure, to the measurement of that source of irregularity in its motion, by the simple means of eliminating the effects of temperature, for the measure of which the instrument had at first been solely adapted. This was readily accomplished by attaching a thermometer acting by the dilatation of non-elastic fluids. The value of the barometer as a prognosticator of weather has been sufficiently known to render it an object to introduce its use at sea, which Hooke accordingly proposed; and his instrument, like all others which indicated the density of the air under the combined influence of temperature and pressure, was called the Manometer, a name which, perhaps, it would be well to retain. Halley afterwards mentioned this instrument with applause, * and stated the objection which had been found to it from the absorption of the inclosed air by the confining fluid. Varignon seems also to have contrived a manometer similar to Hooke's. †

The objection arising from absorption was got over, though certainly at the expence of the simplicity of the instrument, by using mercury for the confining fluid in a manometer hastily got up for the Arctic Expedition of Commodore Phipps, by Mr Ramsden; and from the account given of its performance under very unfavourable circumstances, we are led to wonder that it should once more have fallen into oblivion. In the account published of the voyage it is said, "This instrument, though far from complete, having been constructed in a hurry, for the purpose of a first experiment, and liable to some inaccuracies in the observations, from not having the thermometer with which it was compared attached to it, seldom differed from the marine barometer one-tenth of an inch. Should it be improved to the degree of accuracy of which it seems capable, it will be of great use in determining refractions for astronomical observations, as well as indicating an approaching gale at sea."‡ I shall not notice the manometers of Roy § and Davy, || which were intended merely for laboratory experiments; but pass on to the sympiesometer of Mr Adie, of which, (though from not knowing what already had been done, he had the merit of the invention,) the real novelty consists in finding means of preventing that absorption of air by the inclosed fluid which occurred in the instrument of

* *Philosophical Transactions,* vol. xxii. p. 791.

† *Mémoires de l'Academie Royale,* 1705.

‡ *Phipps' Voyage towards the North Pole.* Appendix.

§ *Phil. Trans.* vol. lxvii.

|| *Nicholson's Journal,* 8vo. vol. iv.

Hooke, without employing mercury, as was done by Ramsden. This he found to be effected by using almost any gas but atmospheric air, excepting such as happened to be immediately absorbable by the inclosing fluid. * To enable those not intimate with the instrument to follow the succeeding details, I shall give a figure of Mr Adie's sympiesometer in its most improved and portable form, such as I have always employed. It is inclosed in a brass cylinder, which is contrived to close upon itself. A glass tube B C, Plate I. (Fig. 7,) of considerable length, terminates above in an elongated bulb inclosing hydrogen gas, and the lower part being filled with oil as an indicator of the bulk of the gas, terminates in a cistern D, provided with a stopple pushed down by a pin at I. The bulk of the gas in the bulb A, being affected both by temperature and pressure, the former is eliminated by moving the zero of the sliding-scale G H, down to a point on the fixed-scale F, (graduated for the effects of temperature only,) which is determined by the attached delicate mercurial thermometer E. The barometric indications are simply indicated on the sliding-scale.

In this part of the memoir, I propose to lay aside as much as possible theoretical considerations, and, taking the sympiesometer as the most improved form of manometric instruments, to examine its *practical* defects. A complete investigation of the theory of these instruments will be given in the sequel, with a consideration of the mode of graduation, in which, I believe, the sympiesometer may be materially improved; and I shall then detail the new forms of the instrument which I propose for the diminution or correction of the present errors. These errors, which it is the object of the present paper to examine, are of three kinds,—one arising from an inherent inaccuracy in the scale of the instrument, and capable of estimation; another of continued and variable decrease in the absolute pressure indicated, and the third, variable and irregular, depending upon external circumstances. Each of these must be separately considered.

* The absorption complained of cannot be of a merely mechanical nature, otherwise it would arrive at a speedy limit. Its phenomena seem to have been little attended to. Mr Leslie found at first similar difficulties in the construction of the differential thermometer, which he successfully removed by the employment of sulphuric acid,—a fluid which would not answer in the present instance, where one portion must necessarily be exposed to the free atmosphere, from which it rapidly attracts moisture.

§ 1. *On an Error in the Graduation of the Sympiesometer.*

To understand this rightly we must have recourse to a little of the theory of the instrument. The elastic fluid in the gaseous bulb is affected in its bulk by temperature and by pressure. The effect of increase of temperature is by dilatation to enable the gas to occupy a larger space under the same pressure, or, in other words, to increase its elasticity. Thus, if a volume of gas under a pressure of 30 inches of mercury be represented by 1, and by an increase of temperature, its volume becomes 1.1, it will yet support the same pressure of 30 inches. Now taking in the element of pressure, we know from the law of Mariotte, that the volume of an elastic fluid is inversely as the pressure it sustains; therefore, as the pressure sustained by the gas at both the temperatures above supposed was 30 inches, the volume at 29 inches would, in the first case, be $1 \times \frac{30}{29}$, in the other $1.1 \times \frac{30}{29}$, or $\frac{30}{29}$ and $\frac{33}{29}$, and subtracting the volumes as affected by temperature simply from each, we have for the extent of scale corresponding to a change of an inch of pressure, in the first case, $\frac{30}{29} - 1 = \frac{1}{29}$; in the second $\frac{33}{29} - 1.1 = \frac{1.1}{29}$. Hence, *every volume assumed by the gas on account of a change of temperature must be considered as a new unity of volume, and to each a different scale of inches of pressure belongs.* Therefore, no sliding scale can satisfy the condition of the problem,—a circumstance overlooked by Mr Adie, as his mode of graduation sufficiently proves, which consisted in placing the instrument under varied circumstances of temperature and pressure respectively, the other element remaining constant.

We may investigate this more concisely by symbols, and those employed by M. Biot* are as convenient as any we can use. Let V represent the volume of gas at the freezing point, and under a pressure p; and V' that at a temperature t degrees above the freezing point, and under a pressure p'; δ being the dilatation of gas for 1°, the unity of volume being

* *Traité de Physique*, tom. 1. It is a frequent defect in inquiries like the present, that the investigations given are wholly popular, or wholly mathematical. Both should be given where detection of error is the object, for conviction is most effectually brought home to some by the one method, and to some by the other; and it would save some errors in theory did the analyst oftener condescend to clothe his argument in words, where the subject is one of proof, not of discovery.

at the freezing point. Then from the laws above-mentioned we have

$$V' = V.(1+\delta t).\frac{p}{p'}$$

but where the effects of pressure and temperature are taken separately, as employed by Mr Adie, we have $V' = V.(\frac{p}{p'}+\delta t)$, the error of which consists in the omission of the effect of changed pressure upon the excess of the new volume caused by the alteration of temperature, or in employing δt as a multiplier, in place of $\frac{p}{p'}\delta t$. As in Hooke's instrument, like Adie's, a sliding scale was used, it was liable to the same error, which in Ramsden's was avoided by the substitution of calculation for a sliding scale. We may in the meantime state that the correction in round numbers amounts to the increase of the barometric divisions on the sliding scale, by one hundredth for every 5° of excess of temperature above that for which the unity of volume was taken.

§ 2. *On a gradual change in the absolute Height of the Sympiesometer.*

While by the principles of construction of the sympiesometer it was conceived to be freed from all index error, by the author of the article Meteorology in the *Edinburgh Encyclopædia*, it was thought that the absorption of the inclosed air which spoiled the instrument of Hooke had not been got free of, and that an *additive* error gradually accumulated;—the production of extensive and satisfactory experiments will justly be considered necessary, if I shall assert the existence of a contrary error by which a gradually decreasing pressure is indicated. The table in the article Meteorology, just referred to, was merely given as pointing out a general tendency to an additive error, and the comparisons with the barometer were only continued for a short period. The cause of the apparent additive error in the sympiesometer is probably mainly attributable to the effect of temperature on the barometer, which we have no reason to believe was corrected; and as even in the most constantly inhabited rooms, the temperature falls several degrees in the months of October and November, when these observations were made, the uncorrected height of the barometer being lowered, the apparent deviation of the sympiesometer would have an additive sign, supposing it stationary.

When, in April 1829, I had my sympiesometer repaired by Mr Adie, I found the index error to amount to — .08 inch, at a temperature of 60°. The cause of this discrepancy I shall not at present inquire into. In *both* his instruments, Professor Schumacher found it to be + .14, at 50°. In a notice I have met with of the sympiesometer, as used in the Russian service, I find the index error stated at as much as — .60. * Being desirous of examining not only the constancy of its level, but its general performance and supposed great sensibility, I immediately commenced a register of comparison of the sympiesometer with the barometer. Being then engaged in an inquiry, which for some years I have been carrying on, into the horary oscillations of the barometer, I compared the two instruments every time I observed the latter, or *five* times a-day. This extensive register was pursued with slight interruption, till August 1830, and presented an ample collection of facts and data, amounting to many hundred observations, of which the following pages present an abstract; each individual observation having registered along with it the temperature by the attached thermometer of each instrument. That attached to the sympiesometer having been broken in August 1829, a difference of a few tenths of a degree might alter the indication of index error. I have therefore commenced my reduction of the observations with their resumption in October. I have contented myself with reducing the first five days of copious comparisons in each month, so as to show the index error at these intervals as far as till March in Table I.; but after March I deduced *daily* the mean results of all the comparisons; a practice which cannot be too much recommended; and I have given them in full. I will therefore only request the reader to observe, that though he finds comparisons given for every day in Table II. these are not insulated observations, but generally the result of *five* comparisons, and deserving of proportionate weight. He will therefore suspend his judgment as to any oscillatory irregularities which appear from one period to another; and confine his attention chiefly at present to the *general* results of the monthly comparisons. In Table I. the successive columns present, 1*st*, the day of the month; 2*d*, the barometer; 3*d*, the

* *Admiralty Memoirs of St Petersburg*, vol. x.

attached thermometer; *4th*, the height of the barometer reduced to 32°; *5th*, the sympiesometer; *6th*, its attached thermometer; *7th*, the index error of the sympiesometer, or the difference of columns four and five. In Table II. for conciseness, column four is omitted; and the *rough* difference of the barometer and sympiesometer is given, which, for the first five days of each month, is corrected in the final column for temperature,—a correction which may readily be applied to any of the others.

TABLE I.

1829.		Barom.	Att. Ther.	Bar. corrected for temp. *a*.	Symp. *b*.	Att. Ther.	*a*—*b*.
Oct.	1,	30.045	63.5	29.965	29.916	64.0	0.049
	2,	29.728	64.5	29.644	29.596	65.0	0.050
	3,	29.309	63.4	29.229	29.158	64.1	0.071
	4,	29.282	64.0	29.201	29.141	64.4	0.060
	5,	29.098	63.2	29.019	28.949	64.4	0.070
						Mean	0.060
Nov.	1,	29.799	58.6	29.731	29 683	59.8	0.048
	2,	29.806	59.8	29.734	29.679	61.0	0.055
	6,	29.232	59.6	29.163	29.092	60.7	0.071
	7,	29.483	58.7	29.415	29.339	59.7	0.076
	8,	29.576	57.6	29.511	29.481	59.3	0.030
						Mean	0.056
Dec.	5,	29.806	61.0	29.732	29.663	62 2	0.069
	6,	29.987	59.2	29.917	29.830	60.4	0.087
	9,	29.987	58.3	29.920	29.825	60.0	0.095
	10,	29.728	56.7	29.665	29.595	58.7	0.070
	11,	29.350	53.8	29.294	29.251	55.4	0.063
1830.						Mean	0.077
Jan.	1,	30.401	53.6	30.345	30.271	54.8	0.074
	2,	30.315	53.5	30.259	30.172	54.7	0.087
	3,	30.194	54.7	30.136	30.052	55.5	0.084
	5,	30.016	55.2	29.957	29.865	56.3	0.092
	6,	29.558	57.0	29.495	29.409	58.4	0.086
						Mean	0.085
Feb.	3,	29.930	50.0	29.884	29.813	51.3	0.071
	4,	29.704	50.0	29.658	29.612	52.0	0.046
	5,	29.505	50.7	29.457	29.392	51.7	0.065
	6,	29.164	50.6	29.117	29.058	52.4	0.059
	7,	28.750	53.0	28.698	28.638	54.5	0.060
						Mean	0.060

1830.	Barom.	Att. Ther.	Barr. corrected for temp. *a.*	Symp. *b.*	Att. Ther.	*a—b.*
Mar. 2,	29.955	64.2	29.972	29.743	65.3	0.129
4,	29.667	62.7	29.589	29.478	62.8	0.111
5,	29.719	61.5	29.644	20.520	62.7	0.124
6,	29.803	59.2	29.733	29.638	60.9	0.095
7,	29.761	60.2	29.689	29.610	61.3	0.079
					Mean	0.107

TABLE II.

1330.	Barom. *a.*	Att. ther. *c.*	Symp. *b.*	Att. ther.	*a—b.*	*a—b* corrected for *c.*	
Apr. 1,	29.633	58.7	29.462	58.7	.171	0.103	Mean, 0.100
2,	29.475	55.2	29.316	56.0	.159	0.101	
3,	29.466	55.0	29.306	55.8	.160	0.102	
4,	29.752	55.0	29.604	57.0	.148	0.090	
5,	29.426	55.8	29.264	56.8	.162	0.102	
6,	29.312	59.6	29.100	60.2	.212		
7,	29.314	60.8	29.087	62.0	.227		
8,	29.166	61.7	28.915	62.5	.251		
9,	29.001	63.0	28.800	63.3	.201		
10,	29.081	62.6	28.880	63.6	.201		
11,	29.143	62.2	28.878	62.7	.265		
12,	29.050	62.7	28.786	64.3	.264		
13,	29.516	62.3	29.263	63.2	.253		
14,	29.464	63.0	29.220	63.6	.244		
15,	29.325	62.8	29.068	64.1	.257		
16,	29.151	65.5	28.845	65.5	.306		
17,	29.009	62.7	28.728	63.0	.281		
18,	29.313	62.2	29.062	63.6	.251		
19,	29.164	60.6	28.941	62.0	.223		
20,	29.412	60.6	29.170	61.3	.242		
May 5,	29.782	64.2	29.501	64.3	.281	0.200	Mean, 0.172
6,	29.570	65.4	29.296	65.3	.274	0.190	
7,	29.340	62.0	29.117	64.2	.223	0.147	
8,	29.352	59.6	29.095	60.2	.257	0.187	
9,	29.390	55.4	29.195	56.2	.195	0.136	
10,	29.564	58.8	29.366	58.8	.198		
11,	29.604	59.0	29.390	59.5	.214		
12,	29.626	58.2	29.427	58.4	.199		
13,	29.794	58.8	29.574	59.4	.220		
14,	29.762	60.8	29.533	60.2	.229		
15,	29.756	60.4	29.536	60.5	.220		
16,	29.799	60.8	29.560	60.9	.239		
17,	29.683	62.7	29.436	62.4	.247		
18,	29.447	63.6	29.171	63.1	.276		

May 19,	29.549	61.8	29.295	61.9	.254		
20,	29.660	60.6	29.425	60.7	.235		
21,	29.635	59.2	29.407	59.3	.228		
22,	29.628	54.7	29.425	55.3	.203		
23,	29.627	59.6	29.412	60.2	.215		
24,	29.386	59.8	29.182	60.3	.204		
25,	29.038	60.5	28.770	61.2	.268		
26,	28.917	60.6	28.659	61.0	.258		
27,	29.221	59.4	28.987	59.7	.234		
28,	29.614	61.6	29.386	60.4	.228		
29,	29.479	61.2	29.221	60.4	.258		
30,	29.222	64.0	28.920	64.8	.302		
31,	29.179	63.0	28.877	62.9	.302		
June 1,	29.525	60.5	29.270	60.8	.255	0.183	Mean, 0.190
2,	29.698	65.6	29.417	63.5	.281	0.197	
3,	29.438	60.7	29.184	61.5	.254	0.181	
4,	29.380	62.0	29.112	62.4	.268	0.193	
5,	29.635	61.7	29.365	61.7	.270	0.195	
6,	29.540	61.2	29.276	61.6	.264		
7,	29.664	63.6	29.393	64.0	.271		
8,	29.952	61.8	29.666	62.2	.286		
9,	30.026	62.0	29.735	63.1	.291		
10,	29.693	62.0	29.383	62.1	.310		
11,	29.451	64.2	29.152	64.2	.299		
12,	29.304	62.2	29.016	62.5	.288		
13,	29.230	62.2	28.930	62.9	.300		
14,	29.353	62.6	29.072	62.3	.281		
15,	29.477	60.6	29.217	60.7	.260		
16,	29.640	61.2	29.402	62.4	.238		
17,	29.585	63.6	29.293	62.3	.292		
18,	29.218	63.8	28.914	64.0	.304		
19,	29.183	60.3	28.895	61.1	.288		
20,	29.294	63.2	29.002	63.8	.292		
21,	29.294	61.7	29.005	62.3	.289		
22,	29.329	60.3	29.633	60.5	.304		
23,	29.536	60.2	29.242	60.1	.294		
24,	29.600	65.0	29.310	63.3	.290		
25,	29.535	60.0	29.263	60.0	.272		
26,	29.352	62.0	29.067	62.5	.285		
27,	29.449	63.5	29.150	63.6	.299		
28,	29.329	65.3	29.016	65.2	.313		
29,	29.474	64.0	29.165	64.0	.309		
30,	29.696	64.7	29.397	64.6	.299		
July 1,	29.572	66.8	29.279	65.5	.293	0.206	Mean, 0.221
2,	29.308	63.8	28.990	64.6	.318	0.238	
3,	29.306	63.2	28.994	63.7	.312	0.233	
4,	29.502	63.0	29.210	63.0	.292	0.214	
5,	29.649	64.0	29.357	64.2	.292	0.212	
6,	29.346	64.3	29.030	64.0	.316		

1830.	Barom. *a.*	Att. ther. *c.*	Symp. *b.*	Att. ther.	*a—b.*	*a—b* corrected for *c.*	
July 7,	29.163	64.0	28.840	64.1	.323		
8,	29.012	62.4	28.700	62.3	.312		
9,	29.090	63.6	28.776	64.3	.314		
10,	29.412	63.2	29.122	63.3	.290		
11,	29.411	64.0	29.106	64.2	.305		
12,	29.290	63.6	28.985	64.0	.305		
13,	29.687	63.2	29.391	63.5	.296		
14,	29.455	62.8	29.170	63.1	.285		
15,	29.356	65.2	29.040	65.1	.316		
16,	29.391	64.5	29.084	64.5	.305		
17,	29.315	61.3	29.022	64.8	.293		
18,	29.253	63.7	28.940	64.3	.313		
19,	29.534	64.5	29.239	64.5	.295		
20,	29.559	64.8	29.250	64.7	.309		
21,	29.748	66.1	29.429	65.8	.319		
22,	29.613	67.0	29.287	67.6	.326		
23,	29.589	68.0	29.242	67.9	.347		
24,	29.685	67.7	29.327	67.9	.358		
25,	29.784	70.0	29.428	69.5	.356		
26,	29.880	71.5	29.464	70.6	.416		
27,	30.050	72.5	29.634	72.1	.416		
28,	30.108	75.0	29.660	74.8	.448		
29,	29.893	70.7	29.506	70.6	.387		
30,	29.591	71.6	29.154	71.4	.437		
31,	29.633	69.7	29.240	69.3	.397		
Aug. 1,	29.325	67.7	28.950	67.6	.375	0.286	
2,	29.164	69.0	28.742	67.7	.422	0.330	
3,	29.488	67.2	29.130	67.0	.358	0.270	Mean, 0.276
4,	29.590	65.6	29.253	65.7	.337	0.253	
5,	29.495	66.5	29.167	66.0	.328	0.241	
7,	29.491	65.0	29.158	65.0	.333		
8,	29.479	65.4	29.151	65.7	.328		
20,	29.701	62.4	29.404	62.8	.297		
21,	29.687	63.0	29.391	63.4	.296		
22,	29.633	62.7	29.340	62.8	.293		
23,	29.462	63.5	29.145	63.9	.317		
24,	29.352	63.7	29.041	63.9	.311		
25,	29.226	62.7	28.906	63.0	.320		
26,	29.406	62.7	29.101	63.2	.305		
27,	29.314	63.5	29.008	64.0	.306		
28,	29.167	60.7	28.884	61.1	.283		
29,	29.567	64.0	29.277	64.0	.290		

In these tables an unequivocal and very considerable fall in the indication of the sympiesometer is evident. The difference of the index errors are by no means uniform, and, even in a few instances, have a negative instead of a positive sign. These, however, are not real exceptions, for the negative results are quite trifling, and very far below the amount of the *second* differences. The irregularities are obviously not from errors of observation, since they have a connection from day to day with periods of maxima and minima; and the attentive reader will probably have already observed the important influence of the contemporaneous circumstances recorded in the other columns of the table upon the column of differences.

The discussion of these more intricate effects we postpone to another part of this memoir; but no theory of these variations of second differences can prevent our concluding, *that the level of the sympiesometer was gradually sinking*. This phenomenon will not, I imagine, be observed in stationary instruments. I am disposed to attribute it to the gradual transmission of minute globules of air shaken into the oil in the cistern, by frequent carriage, upwards through the column which confines the hydrogen gas. It must, however, have been a gradual process, and was attributable to no carelessness in use. To my full belief, the sympiesometer, while in my possession, was never turned upside down even for a moment; and, in all my perambulations, the column of oil was never once separated.

§ 3. *On an Error of Variable Magnitude depending on external circumstances.*

This most important and most troublesome of the errors affecting the sympiesometer, arises from the want of strict accordance in the indications of the mercurial and gaseous bulbs at the same instant, as regards temperature. We have seen from the theory of the instrument that its whole accuracy depends on the postulate, that the effects of heat on the gaseous bulb shall be accurately eliminated by the precise correspondence of the indications of the mercurial thermometer. The utmost delicacy is here requisite. The error of a single tenth of a degree of Fahrenheit will create an error of nearly a fathom of altitude in the scale of heights, (at thirty inches of pressure;) and those who are accustomed to delicate thermometric experiments must be aware how often, even under favourable circumstances, that error must be increased four and five, nay, ten times. The very dissimilar sensibility of the bulbs, the one containing the rarest known substance, the other the densest of fluids, involves sufficient sources of error. But in the sympiesometer these bulbs have been placed at such a distance from one another, and under such different circumstances, that, unless under the most favourable conditions, the coincidence is but a happy compensation of errors. Before my first paper was published, I had theoretically examined the sensibility of the bulbs, and found the difference perfectly erroneous; it is, however, modified in practice by the inclosure of the gaseous bulb in the brass cylinder, and the free exposure of the mercurial one, which, if in some cases it equalizes errors, frequently gives rise to very troublesome ones. The errors from this source to which the sympiesometer is exposed, are therefore of two kinds, the one arising from the different temperature of two contiguous strata of air in which the two bulbs are placed; the other from rapid changes of temperature which affect one bulb more speedily than the other. The first of these, trivial as it may appear, is frequently of serious inconvenience, particularly in observations within doors, where the draught of air from a door or window may prevent the instrument from acting for half an hour after the cause of inequality has been removed. Even the distance of fifteen inches between the bulbs is often sufficient, in a perfectly still room, to impart a sensible difference of temperature to the two.

The unequal sensibility of the bulbs being, as I endeavoured in my former paper to demonstrate, the great defect of the instrument, particularly when used in the open air and applied to the measurment of heights, I resolved to institute experiments on the time required by each respectively, to acquire a new temperature in perfectly still air. The obvious mode of experiment was to bring the instrument into a medium differing in temperature by a considerable number of degrees from that in which it had already acquired an equilibrium, and then converting the gaseous column into a simple air thermometer, by eliminating the effects of pressure, compare the accessions of heat indicated by the two thermometers at different intervals of time. The conduct of the experiment, however, required much delicacy, and some contrivance; but I succeeded in repeating it several times with a degree of precision beyond my expectation. The most convenient time was in frosty weather, when the external air differed considerably in temperature from that of the room in which they were made. But the difficulty was, how to observe the indications of these delicate thermometers, without approaching to them the warmth of my body. For this object, I availed myself of the elegant principle first noticed by Gauss and applied by Captain Kater to his floating collimator, which enables us to view an object at a short distance with a telescope, by means of the intervention of a second object-glass, in the focus of which the object to be viewed is placed; and since, on passing through the detached object-glass, the rays of course emerge parallel to each other, in that state they enter the object-glass of the telescope. I employed a fine forty-two inch achromatic with a considerable power, and adjusted to the farther extremity of it a pasteboard tube, containing an object-glass of about four feet focus, at which distance beyond, the sympiesometer was hung for observation. By this means I was sitting at the eye end of the achromatic, about nine feet from the instrument, recording its motions with the same precision as if I had been within six inches of it.

These observations will not only prove valuable as a correct indication of the relative sensibility of the bulbs, but will form the most precise illustration of the laborious length of time required in still air to induce an equilibrium; so that where the instrument is wished to act on a sudden transposition to air of a different temperature, unless it be exposed to wind, it must often be wholly useless. The following series was made by exposing the instrument outside of a window on a calm day, when, by the final result, the external temperature appears to have been 36°.0. At the time of exposure the temperature shown by both bulbs was 53°.2. The pressure continued stationary.

TABLE III.—28th December 1829.

Times from the commencement.	Thermometers.		Excess above surrounding air.		
	Gas.	Merc.	Gas.	Merc.	Diff.
0′. 0″	53°.2	53°.2	17°.2	17°.2	0°.0
0 .30	52 .6	51 .3	16 .6	15 .3	—1 .3
1 . 0	51 .8	50 .1	15 .8	14 .1	—1 .5
1 .30	51 .3	49 .2	15 .3	13 .2	—2 .1
2 . 0	50 .3	48 .2	14 .3	12 .2	—2 .1
2 .30	49 .4	47 .4	13 .4	11 .4	—2 .0
3 . 0	48 .6	46 .9	12 .6	10 .9	—1 .7
3 .30	47 .4	46 .1	11 .4	10 .1	—1 .3
4 . 0	46 .8	45 .8	10 .8	9 .8	—1 .0
4 .30	46 .1	45 .3	10 .1	9 .3	—0 .8
5 . 0	45 .0	45 .1	9 .0	9 .1	+0 .1
5 .30	44 .1	44 .7	8 .1	8 .7	+0 .6
6 . 0	43 .5	44 .2	7 .5	8 .2	+0 .7
6 .30	43 .1	43 .8	7 .1	7 .8	+0 .7
7 . 0	42 .7	43 .4	6 .7	7 .4	+0 .7
7 .30	42 .4	42 .7	6 .4	6 .7	+0 .3
8 . 0	41 .4	42 .4	5 .4	6 .4	+1 .0
8 .30	41 .4	42 .1	5 .4	6 .1	+0 .7
9 . 0	40 .6	41 .9	4 .6	5 .9	+1 .3
9 .30	40 .2	41 .6	4 .2	5 .6	+1 .4
10 . 0	39 .7	41 .3	3 .7	5 .3	+1 .6
10 .30	39 .5	41 .1	3 .5	5 .1	+1 .6
11 . 0	39 .1	40 .9	3 .1	4 .9	+1 .8
11 .30	38 .7	40 .6	2 .7	4 .6	+1 .9
12 . 0	38 .5	40 .4	2 .5	4 .4	+1 .9
12 .30	38 .2	40 .2	2 .2	4 .2	+2 .0
13 . 0	38 .0	40 .0	2 .0	4 .0	+2 .0
13 .30	37 .8	39 .8	1 .8	3 .8	+2 .0
14 . 0	37 .6	39 .6	1 .6	3 .6	+2 .0
14 .30	37 .5	39 .4	1 .5	3 .4	+1 .9
15 . 0	37 .3	39 .3	1 .3	3 .3	+2 .0
15 .30	37 .2	39 .1	1 .2	3 .1	+1 .9
16 . 0	37 .1	39 .0	1 .1	3 .0	+1 .9
16 .30	37 .1	38 .8	1 .1	2 .8	+1 .7
17 . 0	36 .8	38 .5	0 .8	2 .5	+1 .7
17 .30	36 .6	38 .4	0 .6	2 .4	+1 .8
18 . 0	36 .6	38 .3	0 .6	2 .3	+1 .7
18 .30	36 .5	38 .2	0 .5	2 .2	+1 .7
19 . 0	36 .4	38 .1	0 .4	2 .1	+1 .7
19 .30	36 .4	38 .0	0 .4	2 .0	+1 .6
20 . 0	36 .4	38 .0	0 .4	2 .0	+1 .6

20 .30	36 .3	37 .9	0 .3	1 .9	+ 1 .6
21 . 0	36 .3	37 .8	0 .3	1 .8	+ 1 .5
21 .30	36 .3	37 .6	0 .3	1 .6	+ 1 .3
22 . 0	36 .3	37 .5	0 .3	1 .5	+ 1.2
22 .30	36 .2	37 .4	0 .2	1 .4	+ 1 .2
23 . 0	36 .2	37 .4	0 .2	1 .4	+ 1 .2
23 .30	36 .1	37 .2	0 .1	1 .2	+ 1 .1
24 . 0	36 .1	37 .2	0 .1	1 .2	+ 1 .1
24 .30	36 .0	37 .1	0 .0	1 .1	+ 1 .1
25 . 0	36 .0	37 .0	0 .0	1 .0	+ 1 .0
25 .30	36 .0	37 .0	0 .0	1 .0	+ 1 .0
26 . 0	36 .0	36 .9	0 .0	0 .9	+ 0 .9
26 .30	36 .0	36 .9	0 .0	0 .9	+ 0 .9
27 . 0	36 .0	36 .8	0 .0	0 .8	+ 0 .9
27 .30	36 .0	36 .8	0 .0	0 .8	+ 0 .9

In the preceding table there is a circumstance worthy of particular notice. I allude to the apparently greater sensibility of the mercurial bulb at the commencement of the observations, as indicated by the sign of the column of differences. We there observe a minimum at 2′, a change of sign, or coincidence, at 5′, a maximum at 13′, and a third coincidence would soon have taken place had the observations been extended beyond 28′. Now this arises from the observations not being made in still air, (for though calm, the instrument being outside of a window, was exposed to currents,) which, acting on the freely exposed mercurial bulb, did not reach so soon the gas, which is sheltered by a brass cylinder. It is to this peculiar mode of affection that some singular motions of the two thermometers are to be ascribed which perpetually occur in external observations, and occasionally producing adventitious coincidences the indications again diverge. In the two following series we have almost strictly the result of the action in still air being made in a room after exposure to the external cold. Had we it in view accurately to analyze the rate of heating, we should have to apply some minute corrections for the change of pressure during the experiment, and for the deviations from perfect equality of temperature of the bulbs at the commencement. But they are amply accurate for our present purpose.

TABLE IV.—28th December 1829.

Times from the commencement.	Thermometer.		Defect below surrounding air.		
	Gas.	Merc.	Gas.	Merc.	Diff.
0′. 0″	36°.0	36°.8	15°.4	14°.6	— 0°.8
0 .30	37 .2	37 .5	14 .2	13 .9	— 0 .3
1 . 0	37 .9	38 .0	13 .5	13 .4	— 0 .1
2 . 0	39 .1	38 .5	12 .3	12 .9	+ 0 .6
3 . 0	41 .4	39 .1	10 .0	12 .3	+ 2 .3
4 . 0	42 .4	40 .2	9 .0	11 .2	+ 2 .2
5 . 0	43 .6	40 .8	7 .8	10 .6	+ 2 .8
6 . 0	44 .5	41 .5	6 .9	9 .9	+ 3 .0
7 . 0	45 .1	42 .1	6 .3	9 .3	+ 3 .0
8 . 0	45 .8	42 .8	5 .6	8 .6	+ 3 .0
9 . 0	46 .6	43 .4	4 .8	8 .0	+ 3 .2
10 . 0	46 .9	44 .0	4 .5	7 .4	+ 2 .9
11 . 0	47 .5	44 .4	3 .9	7 .0	+ 3 .1
12 . 0	47 .8	44 .9	3 .6	6 .5	+ 2 .9
13 . 0	48 .0	45 .4	3 .4	6 .0	+ 2 .6
14 . 0	48 .2	45 .9	3 .2	5 .5	+ 2 .3
15 . 0	48 .6	46 .2	2 .8	5 .2	+ 2 .4
16 . 0	48 .9	46 .6	2 .5	4 .8	+ 2 .3
17 . 0	49 .2	46 .9	2 .2	4 .5	+ 2 .3
18 . 0	49 .4	47 .3	2 .0	4 .1	+ 2 .1
19 . 0	49 .6	47 .6	1 .8	3 .8	+ 2 .0
20 . 0	49 .7	47 .9	1 .7	3 .5	+ 1 .8
21 . 0	49 .8	48 .2	1 .6	3 .2	+ 1 .6
22 . 0	50 .0	48 .4	1 .4	3 .0	+ 1 .6
23 . 0	50 .2	48 .7	1 .2	2 .7	+ 1 .5
24 . 0	50 .3	48 .9	1 .1	2 .5	+ 1 .4
25 . 0	50 .4	49 .0	1 .0	2 .4	+ 1 .4
26 . 0	50 .5	49 .1	0 .9	2 .3	+ 1 .4
27 . 0	50 .6	49 .4	0 .8	2 .0	+ 1 .2
28 . 0	50 .7	49 .8	0 .7	1 .6	+ 0 .9
29 . 0	50 .8	49 .9	0 .6	1 .5	+ 0 .9
30 . 0	50 .8	50 .0	0 .6	1 .4	+ 0 .8
31 . 0	50 .9	50 .1	0 .5	1 .3	+ 0 .8
32 . 0	50 .9	50 .2	0 .5	1 .2	+ 0 .7
33 . 0	51 .0	50 .3	0 .4	1 .1	+ 0 .7
34 . 0	51 .1	50 .4	0 .3	1 .0	+ 0 .7
35 . 0	51 .2	50 .6	0 .2	0 .8	+ 0 6
36 . 0	51 .2	50 .7	0 .2	0 .7	+ 0 .5
37 . 0	51 .2	50 .8	0 .2	0 .6	+ 0 .4
38 . 0	51 .3	50 .8	0 .1	0 .6	+ 0 .5
39 . 0	51 .3	50 .9	0 .1	0 .5	+ 0 .4

40 . 0	51 .3	50 .9	0 .1	0 .5	+ 0 .4
41 . 0	51 .3	51 .0	0 .1	0 .4	+ 0 .3
42 . 0	51 .3	51 .0	0 .1	0 .4	+ 0 .3
43 . 0	51 .2	51 .1	0 .2	0 .3	+ 0 .2
44 . 0	51 .2	51 .1	0 .2	0 .3	+ 0 .2
45 . 0	51 .3	51 .2	0 .1	0 .2	+ 0 .1
46 . 0	51 .3	51 .2	0 .1	0 .2	+ 0 .1
47 . 0	51 .4	51 .2	0 .0	0 .2	+ 0 .2
48 . 0	51 .4	51 .2	0 .0	0 .2	+ 0 .2
49 . 0	51 .4	51 .3	0 .0	0 .1	+ 0 .1
50 . 0	51 .4	51 .3	0 .0	0 .1	+ 0 .1

In the preceding table the thermometers set out from a somewhat different temperature, which is the cause of the — sign in the column of differences, as is also the case in the following; but the moment of coincidence may easily be found, from which, if required, the times may be reckoned. In both it is nearly at the first minute.

TABLE V.—2d January 1830.

Times from the commencement.	Thermometer.		Defect below surrounding air.		
	Gas.	Merc.	Gas.	Merc.	Diff.
0′.0″	33°.0	35°.8	21°.2	18°.4	— 2°.8
1.0	37 .2	37 .8	17 .0	16 .4	— 0 .6
2.0	39 .6	38 .5	14 .6	15 .7	+ 1 .1
3.0	41 .6	39 .5	12 .6	14 .7	+ 2 .1
4.0	43 .2	40 .5	11 .0	13 .7	+ 2 .7
5.0	44 .4	41 .4	9 .8	12 .8	+ 3 .0
6.0	45 .6	42 .3	8 .6	11 .9	+ 3 .3
7.0	46 .4	43 .1	7 .8	11 .1	+ 3 .3
8.0	47 .0	43 .8	7 .2	10 .4	+ 3 .2
9.0	47 .8	44 .7	6 .4	9 .5	+ 3 .1
10.0	48 .3	45 .2	5 .9	9 .0	+ 3 .1
11.0	48 .9	46 .0	5 .3	8 .2	+ 2 .9
12.0	49 .3	46 .6	4 .9	7 .6	+ 2 .7
13.0	49 .7	47 .0	4 .5	7 .2	+ 2 .7
14.0	50 .0	47 .5	4 .2	6 .7	+ 2 .5
15.0	50 .3	48 .0	3 .9	6 .2	+ 2 .3
16.0	50 .6	48 .4	3 .6	5 .8	+ 2 .2
17.0	50 .9	48 .8	3 .3	5 .4	+ 2 .1
18.0	51 .0	49 .2	3 .2	5 .0	+ 1 .8
19.0	51 .3	49 .5	2 .9	4 .7	+ 1 .8
20.0	51 .4	49 .8	2 .8	4 .4	+ 1 .6

Times from the commencement.	Thermometer.		Defect below surrounding air.		
	Gas.	Merc.	Gas.	Merc.	Diff.
21′.0″	51°.6	50°.1	2°.6	4°.1	+ 1°.5
22.0	51 .7	50 .3	2 .5	3 .9	+ 1 .4
23.0	51 .8	50 .6	2 .4	3 .6	+ 1 .2
24.0	51 .9	50 .8	2 .3	3 .4	+ 1 .1
25.0	52 .0	51 .2	2 .2	3 .0	+ 0 .8
26.0	52 .2	51 .3	2 .0	2 .9	+ 0 .9
27.0	52 .3	51 .5	1 .9	2 .7	+ 0 .8
28.0	52 .4	51 .8	1 .8	2 .4	+ 0 .6
29.0	52 .5	51 .9	1 .7	2 .3	+ 0 .6
30.0	52 .7	52 .1	1 .5	2 .1	+ 0 .6
31.0	52 .8	52 .3	1 .4	1 .9	+ 0 .5
32.0	52 .9	52 .4	1 .3	1 .8	+ 0 .5
33.0	53 .0	52 .6	1 .2	1 .6	+ 0 .4
34.0	53 .1	52 .7	1 .1	1 .5	+ 0 .4
35.0	53 .2	52 .8	1 .0	1 .4	+ 0 .4
36.0	53 .2	53 .0	1 .0	1 .2	+ 0 .2
37.0	53 .3	53 .1	0 .9	1 .1	+ 0 .2
38.0	53 .4	53 .2	0 .8	1 .0	+ 0 .2
39.0	53 .5	53 .2	0 .7	1 .0	+ 0 .3
40.0	53 .6	53 .4	0 .6	0 .8	+ 0 .2
41.0	53 .6	53 .4	0 .6	0 .8	+ 0 .2
42.0	53 .7	53 .6	0 .5	0 .6	+ 0 .1
43.0	53 .7	53 .6	0 .5	0 .6	+ 0 .1
44.0	53 .8	53 .7	0 .4	0 .5	+ 0 .1
45.0	53 .8	53 .8	0 .4	0 .4	+ 0 .0
46.0	53 .9	53 .9	0 .3	0 .3	
47.0	53 .9	54 .0	0 .3	0 .2	
48.0	53 .9	54 .0	0 .3	0 .2	
49.0	54 .0	54 .0	0 .2	0 .2	
50.0	54 .0	54 .1	0 .2	0 .1	
51.0	54 .1	54 .1	0 .1	0 .1	
52.0	54 .1	54 .2	0 .1	0 .0	
53.0	54 .2	54 .2	0 .0	0 .0	

These tables may hereafter be referred to for numerical deductions; but, in the meantime, we may draw the following: 1. That a coincidence of temperature in the bulbs may be either accidental or permanent, as illustrated in the first of these tables. 2. That the accidental coincidence can never be calculated upon, and, therefore, in the present state of the sympiesometer, we must wait till both bulbs have acquired the temperature of the air. 3. That in still air, setting out from an excess or defect of 15°, about 50′ must be given to acquire the equilibrium, for 10° about 44′, for 5° about 34′. These last two coincidences strikingly agree in both the last tables.

This seems the proper place to give an additional and direct proof, (though I think that those who carefully read my last paper will hardly require one,) that the observations upon which I built my opinion of this being the main defect of the instrument, giving in a former number of this *Journal*, were not affected,—at least in their great and rapid variations, by the barometric difficulties of the place, arising from the confinement of a large mass of air in a deep ravine, of which the temperature and density might be liable to considerable variation. The following observations contain a comparison of the sympiesometer with a portable barometer with adjustable level, &c. and prove both the existence of the oscillations which I described, and, being made with every precaution to avoid the influence of radiation, that they are owing to the unequal sensibility of the bulbs, as, from the analysis of about 100 observations near the same spot, I endeavoured to demonstrate.

TABLE VI.—May 13, 1829.

Station,	Hour.	Barom.	Att. Ther.	Det. Ther.	Symp.	Att. Ther.
Colinton House,	12h.25	29.558	62.5	60.5	29.48	62.1
Water of Leith,	12 .40				29.47	63.2
	12 .45	29.724	70.		29.645	63.2
	12 .50	29.720	68.		29.69	62.4
	12 .55				29.715	62.7
	1 . 0			63.		
	1 . 5	29.724	66.	64.	29.715	64.0
	1 .10			63.	29.745	63.8
	1 .15			63.	29.725	63.2
	1 .20	29.716	65.	63.	29.695	63.2
	1 .25			64.	29.705	63.8
Colinton House,	1 .40	29.562	74.		29.415	63.8
	1 .45				29.49	67.8
	1 .50	29.550	71.		29.505	67.9
	1 .55				29.52	68.1
	2 . 0	29.554	70.		29.525	68.1
	2 5				29.535	68.3
	2 .25	29.553	69.		29.51	67.8

TABLE VII.—May 15, 1829.

Station.	Hour.	Barom.	Att. Ther.	Det. Ther.	Symp.	Att. Ther.
Colinton House,	2^h.35	29.647	62.	59.	29.58	61.6
Water of Leith,	3 . 0				29.58 *	60.1
	3 . 5	29.788	64.	57.	29.665	58.3
	3 .10				29.745	58.4
	3 .15			57.5	29.755	58.6
	3 .20	29.791	60.		29.77	58.2
	3 .25			55.7	29.77	57.6
	3 .30	29.788	58.	56.7	29.78	57.6
	3 .35			56.7	29.78	57.6
	3 .40			58.5	29.81 †	58.4
	3 .45	29.790	58.	58.	29.77	58.6
Colinton House,	4 . 5	29.660	59.	58.	29.475	60.8
	4 .15				29.56	62.5
	4 .30				29.59	63.2

The detached thermometer used at the Water of Leith was a delicate pocket one.

As in hot and calm weather the errors from unequal sensibility of the bulbs are greatest, so are also the errors of the first class which include the varied and complex effects of radiation and reflected heat. The dissimilar situation of the bulbs render a coincidence in bright sultry weather very difficult: the mercurial thermometer being much nearer the ground, and unprotected by any exterior casing, is often more affected by reflected heat than the more sensible gaseous column, so much so, that in extreme cases of reflection, I have sometimes closed the brass case of the instrument to place both bulbs more nearly in the same condition. The action of the sympiesometer when used abroad is therefore very dependent upon external circumstances, and the precautions of observation, and the number of times they require to be repeated, must of course vary according to these. Cloudy and windy weather is most favourable for promoting an equilibrium, which unfortunately is not the most conducive to the accuracy of the great conditions of barometric measurement. Under opposite circumstances, and particularly in confined situations, the accurate determination of the true indications of the instrument becomes both uncertain and harassing, and should it be thought a trifling objection that half-an-hour's assiduous ob-

* Cistern 3′ opened. † Gleam of sunshine.

servation is frequently requisite form even an approximation, I would remark, that, of all requisites for the considerable extension of approximate barometric measurements, and for the adoption of an instrument as a constant travelling companion, one of the very most important is promptness of action. When he is not alone, the inconvenience will be severely felt by the observer, and even if his motions are at his own command, it is impossible, in a journey of any extent, to make frequent halts, unless the duration of these be moderately short, and little liable to fluctuation. From absolute want of time, I have frequently been obliged to leave series of observations unfinished when the equilibrium was longer of being obtained than I anticipated.

It is proposed in the Second Part of this Memoir to enter upon the general applicability of manometric instruments to the measurement of heights, illustrated by the results of a great number of observations; but I think that the effect of accidental external circumstances, and the time required for bringing the sympiesometer into correct action, will be most satisfactorily shown by the details of obsservaions made at a variety of seasons for the determination of the height of a particular station. For this purpose, I shall select a pretty extensive series, made upon the difference of level of Colinton House, and the dam of the lower Bonally Reservoir, a station, which, though not so favourably placed as an insulated peak, is yet freely exposed, and directly commanded on few points of the horizon. This is not the place to enter into particulars respecting the mode of deducing the heights, and to trace their errors, but I shall premise that the only correction applied to the approximate height derived from the differences on the logarithmic or fathom scale of the instrument, was that for the temperature of the air, the expansion of which, at the supposed average state of moisture, was taken at .00250 for each degree of Fahrenheit above 32°; a number of remarkably easy application, and, as I shall afterwards show, not differing more from that more usually adopted, than the same quantity, as proposed by different philosophers of equal eminence. In the reductions of my measurements of heights, I have almost always made use of the variations of pressure during the intervals of observation at the lowest station, determined by a standard barometer, which gives these observations every advantage; for one of the greatest defects of the sympiesometer is the uncertainty attending its indications of minute horary changes, and the long time which, after returning to the lower station, (if within doors) we must wait for a corr esponding observation. The observations with the barometer on which, in conjunction with those of the sympiesometer, the small changes of pressure were determined, are not detailed, as irrelevant to the present object.

Experiment 1.—May 20th, 1829.
Fine. Wind E., brisk.

	Hour.	Symp. Inch.	Symp. Fath.	Att. Ther.	Det. Ther.
Colinton House,	12.h20′	29.815	= 169	62.8	59
Bonally Pond, cistern of symp. 5′ open,	4. 15	29.13	= 270	56.2	
	4. 20	29.085	= 276	53.9	$53\frac{1}{2}$
	4. 25	29.085	= 276	54.2	$53\frac{1}{2}$
	4. 30	29.085	= 276	54.2	$53\frac{1}{2}$
Colinton House cistern 10′ open,	5. 45	29.735	= 181	64.0	$53\frac{1}{2}$
	5. 55	29.80	= 171	63.0	
	6. 30	29.835	= 166	62.2	

Deduced height, 671 feet, or 1086 above the mean level of the sea.

Remark.—The uncommon excellence of this day's observations, the instrument having become quite stationary in about 10′, is owing to the combination of circumstances most favourable of any to the action of the sympiesometer, viz. a brisk wind blowing from a point nearly opposite the sun. The detached thermometer on the hill was a delicate pocket one by Cary.

Experiment 2.—May 26th, 1829.
Very fine day. Wind S.E. moderate.

Colinton House,	12h 0′	30.21	= 112	62.4	57.5

Bonally Pond, 5′ open,	1. 25	29. 46 = 221	53.5
	1 30	29. 44 = 224	52.8
	1. 35	29. 43 = 226	53.2
	1. 40	29.435 = 225	53.5
Colinton House,	6. 45	30.145 = 122	

Deduced height, 696 feet. Above the sea, 1111 feet.

EXPERIMENT 3.—May 30th, 1829.

Fine. Wind N. E. Variable force.

Colinton House,	10.^{h}55′	29. 89 = 158.5	65.4	64
Bonally Pond, 5′ open,	12. 10	29.255 = 251	60.1	
(calm)	12. 15	29.235 = 254	58.2	56
(breeze)	12. 20	29. 32 = 242	57.6	55
	12. 25	29. 22 = 256	55.6	54
	12. 30	29. 20 = 260	55.0	54.5
	12. 35	29. 19 = 261	55.8	55
	12. 40	29. 20 = 259	55.5	54
Colinton House,	3. 15	29. 84 = 166	63.8	62
	4. 0	29.835 = 167	63.8	

Deduced height, 634 feet. Above the sea, 1049 feet.

Remark.—Some difficulty, as to the variation of pressure in the interval, owing to different indications of the sympiesometer and barometer, probably partly occasioned this small result, compared with the last.

EXPERIMENT 4.—July 6th, 1829.

Wind W.; brisk.

Colinton House,	2.^{h} 0′	29.320 = 241	67.2	65
Bonally Pond, 5′ open,	3. 10	28.575 = 352.5	57.8	58
	3. 15	28. 58 = 352	57.6	58
Colinton House,	5. 40	29. 27 = 249	64.8	62

Deduced height, 698 feet. Above the sea, 1113 feet.

Remark.—The extraordinary facility of these observations was entirely owing to the brisk wind. If I recollect right, the day was also cloudy. The instrument, it would appear, had acquired very nearly the temperature of the air in the first five minutes.

EXPERIMENT 5.—December 31st, 1829.

Cloudy; wind E.; snow deep on the ground.

	Hour.	Symp. Inch.	Symp. Fath.	Att. Ther.	Det. Ther.
Colinton House,	2.h20′	30.30 =	99	57.1	35
Bonally Pond, 2′ open,	3. 17	29.50 =	214	30.7	
	3. 25	29.51 =	213	30.4	
Colinton House 40′ open	5. 20	30.28 =	102	54.4	34
	5. 45	30.28 =	102	54.9	

Deduced height, 679 feet. Above the sea, 1094 feet.

Remark.—Here we have the measurement under very different circumstances. A change of temperature from 58° to 30°, and deep snow on the ground. In noting this series, I have remarked, "I am convinced, that, however personally inconvenient, such weather is best for the measurement of heights, especially with the sympiesometer." Indeed, nothing can be so favourable for promoting an equilibrium of temperatnre as a covering of snow, combined with the small radiating effect of the sun at low altitudes. This, accordingly, is one of the most accurate, as well as shortest, series of this collection.

EXPERIMENT 6.—January 1st, 1830.

Colinton House,	2.h20′	30.275 =	102.5	55.2	32
Bonally Pond, 5′ open, (observation a little deranged,)	3. 45	29.52 =	212	25.6	
	3. 50	29.50 =	215	25.5	
Colinton House,	5. 20	30.17 =	117	52.5	
	7. 30	30.21 =	112	55.5	30

Deduced height, 642 feet. Above the sea, 1057 feet.

EXPERIMENT 7.—January 9th, 1830.

Colinton House,	2.h50′	29.53 =	210	57.6	42
Bonally Pond, 20′ open,	3. 55	28.89 =	306	35.6	
Colinton House,	5. 40	29.58 =	203	59.1	38

Deduced height, 618 feet. Above the sea, 1033 feet.

Remark.—This day's observation affords an example of one of the casualties attending the use of the sympiesometer. The observation at 2^h 50′ within the walls of a room was found to be deranged by the accident of the window being open, and, had not the barometer been employed for the estimation of the horary variation of pressure and computation of the height, a long period must have been wasted on permitting the instrument to attain an equilibrium in the room.

EXPERIMENT 8.—Jannary 23d, 1830.
Wind E.; brisk.

Colinton House,	2.h25′	29.635 = 196	51.5	35
Bonally Pond, 5′ open,	3. 40	28.90 = 304	30.4	
Colinton House,	5. 35	29.60 = 200	56.0	
	8.	29.58 = 202	57.5	

Deduced height, 639 feet. Above the sea, 1054 feet.

Remark.—The circumstances of little radiation and a brisk wind blowing opposite the sun at a temperature of 2° below the freezing point, rendered this series very readily observed.

EXPERIMENT 9.—February 12th, 1830.

Colinton House,	2.h 0′	29.49 = 217.5	59.0	46
Bonally Pond, 15′ open,	3. 30	28.78 = 322	38.8	
	3 35	28.78 = 322	38.9	
Colinton House,	6 . 15	29.47 = 220	61.1	

Deduced height, 645 feet. Above the sea, 1060 feet

EXPERIMENT 10.—March 13th, 1830.

Colinton House,	2.h10′	29.50 = 214	63.8	48
Bonally Pond, 10′ open,	3. 15	28.82 = 316	41.6	
Colinton House,	5. 30	29.46 = 221	59.4	
	7. 15	29.455 = 222	58.7	

Deduced height, 618 feet. Above the sea, 1033 feet.

EXPERIMENT 11.—April 17th, 1830.
Wind S. W. with violent gusts and stormy showers.

Colinton House,	12.h 5′	28.64 = 344	64.3	55
Bonally Pond, about 10′ open,	1. 15	28.06 = 433	47.2	
Colinton House,	4. 15	28.72 = 331	61.0	55

Deduced height, 581 feet. Above the sea, 996 feet.

Remark.—This observation gives a result differing more from the mean than any of the others of this series. This arises from the stormy state of the weather, so inimical to barometric observations of all kinds,—not from the defects of the instrument. It must be observed of this and some preceding experiments, that there is not more than one observation at the higher station. This arose from the accidental circumstance of my being able to leave the sympiesometer 10′ or even 15′ to take the temperature of the air, which was abundantly sufficient for the purpose in moderately windy weather, when the direct heat of the sun, and its reflection were null or trifling, as in cloudy weather during the winter season.

EXPERIMENT 12.—June 5th, 1830.

	Hour.	Symp. Inch.	=	Symp. Fath.	Att. Ther.	Det. Ther.
Colinton House,	10h 0′	29.375	=	233	62.6	59
Bonally Pond,						
10′ open.	4.15	28. 66	=	340	52.8	
	4.20	28. 66	=	340	52.8	
Colinton House,	8.30	29. 36	=	236	60.4	56 ?

Deduced height, 675 feet. Above the sea, 1090 feet.

EXPERIMENT 13.—June 26th, 1830.

Colinton House,	2h 45′	29.02	=	285.5	64.1	62
Bonally Pond,						
10′ open,	3.50	28.30	=	395.5	61.4	Sunshine.
	4. 0	28.34	=	390.5	62.0	Do.
	4. 5	28.30	=	395.5	61.4	Do.
	4.10	28.345	=	389	60.4	breeze&shade.
Colinton House,	8. 0	29.08	=	277	64.8	

Deduced height, 676 feet. Above the sea, 1091 feet.

Remark.—These observations afford an instructive example of a source of error to be guarded against, perhaps two days in three of observations in summer. Although carefully sheltered from direct sunshine, it was impossible to avoid a degree of reflection from the neighbouring grass: the consequence was, that, as we took the indications after an exposure of 10′ or of 20′, we should have differed 30 feet in altitude; and, by waiting 25′, and finding it again reduced to the indication at 10′, we might have fancied it correct, and thus be led into an error amounting to 39 feet, which could only be detected by a much longer series of observations, unless a breeze had happily occurred with passing clouds, which, after a stay of half an hour, assisted us in approximating to the truth. But a circumstance worthy of remark is this,—that in observations at 3h 50′, 4h 0′ and 4h 10′, the indication of the *air column* of the instrument *never changed.* The oscillations in the deduced pressure were caused solely by the varying indications of the mercurial thermometer. This was readily explained, for the instrument being itself in perfect shade, the gaseous bulb was, from its position, of course a foot and a-half farther from the reflecting grass than the mercurial one, and, besides, defended from radiation by its polished case of brass; and thus, the mercurial bulb becoming, contrary to its usual state, the more sensitive of the two, occasions errors in sultry weather which, on the present construction of the instrument, are quite inevitable.

Experiment 14.—July 24th, 1830.

Quite calm; very warm; close gray clouds generally obscuring the sun; wind generally west.

Colinton House,	2h 40′	29.23 = 239	68.3	70	
Bonally Pond,					
10′ open,	3.55	28.67 = 339	67.0		
	4.0	28.70 = 335	67.2		
	4.5	28.60 = 350	66.7		a little sun.
	4.8	28.60 = 350	66.8		
	4.11	28.63 = 346	66.8		
	4.14	28.58 = 353	66.6		clouds.
	4.17	28.55 = 355	66.9		
	4.21	28.60 = 350	66.6		
	4.24	28.62 = 346.5	66.6		
	4.27	28.70 = 334	65.0		wind changing to
	4.30	28.62 = 346.5	65.0		N. E. quite calm.
Colinton House,	5.45	29.32 = 241	68.2	69	

Deduced height, 678 feet. Above the sea, 1093 feet.

Remark.—This series is very instructive. The length of time required in observations, and the uncertain nature of the result at last is sufficiently obvious. Even after *eleven* observations made at one station, and continued for the space of three quarters of an hour, we find perpetual fluctuations, and the 10th and 11th give a difference of indication of no less than *seventy-five feet in height*. The observations were repeated oftener than usual, and with every precaution to prevent derangement by the approach of the body, in order to catch at even a temporary stability; and it is only by an analysis of the causes of fluctuation that we can approximate to the true indications. When we examine the motion of the gaseous and mercurial columns, we find precisely similar indications to those obtained from my experiments in 1825, detailed in my previous paper under analogous circumstances. The general tendency and the temperature was to fall, but this fall was interrupted by a succession of rises small in extent, owing, perhaps, in part to the succession of clouds and sunshine, but chiefly, I imagine, to the rise of heated strata of air from the lower country. The annexed table points out most satisfactorily, by the signs of the successive indications of the gaseous and mercurial columns, the nature of the action. By examining the relations of these two columns, we arrive at this important conclusion, that the motions of the mercurial thermometer always succeed by the interval of about one observation, the indications of the more sensitive gaseous bulb considered as a thermometer; it must be remembered that a rise in the pressure scale indicates a fall of temperature, and the reverse; therefore, opposite signs in the two columns indicate the same effect. Thus, a contraction of the gas at $4^h\ 0'$, indicated by +.03, is followed at 4.5 by a fall of the thermometer of —.5; the expansion of the gas —.10 is succeeded by an expansion of mercury of +.1; a zero in the differences of the one by a zero in those of the other, and so on. In the conclusion of the series, the sensibility of the bulbs seems to have been rendered more uniform

Hour.	Diff. Gas.	Diff. Merc.
$3^h.55'$	.00 in.	0°.0
4 . 0	+3	+0 .2
4 . 5	—10	—.5
4 . 8	0	+.1
4 .11	+3	.0
4 .14	—5	—.2
4 .17	—3	+.3
4 .21	+5	—.3
4 .24	+2	.0
4 .27	+8	—1.5
4 .30	—8	.0

by the change of wind mentioned in the observations, the uniformity of relation still continuing, but at shorter intervals than before. This experiment was performed under an exterior temperature, differing *more than forty degrees* from that in Experiment 6.

EXPERIMENT 15.—July 29th, 1830.
Wind E. Brisk. Very little sun.

Colinton House,	$1^h. 0'$	29. 55 = 207.5	74.4	68	
Bonally Pond,					
5′ open,	$2^h.10'$	29. 01 = 287.5	64.2		
	2 .13	29. 00 = 289	64.4		
	2 .16	28. 82 = 317	63.0		
	2 .19	28.865 = 309	63.3		
	2 .22	28. 90 = 303	62.4		Fog.
	2 .25	28. 96 = 295	61.6		
	2 .28	28. 96 = 294	61.6		
	2 .31	28. 97 = 292.5	62.8		Clearing.
Colinton House,					
50′ open,	4 . 0	29. 54 = 209	69.3	67	
	4 .30	29. 54 = 209	70.3		

Deduced height, 587 feet. Above the sea, 1002 feet.

Remark.—It were easy to enlarge on the peculiar indications of this experiment, but I must simply point out the real sources of the irregularities which it displays. Here was a brisk wind and no sun, the most favourable protections against errors of radiation. But the instrument was exposed to air in a high state of moisture, and containing strata of very variable temperature. The gradually approaching fog which had been skirting the neighbouring hills enveloped the instrument with rapid decrease of temperature at $2^h\ 22'$, and having attained its minimum, the thermometer began to rise in the last observation as the fog cleared off. The change of temperature during the decline was sensible every minute, and by the principles shown in the first part of this section, it was obvious that the indications must oscillate; I therefore seized the moment of minimum when the mercury was stationary for three minutes together, and a nearly corresponding steadiness occurring in the gaseous column, it afforded a proof of that being the real pressure, which, without this analysis, could not have been ascertained, and by waiting longer, the occurrence of an opposite series of oscillations as the temperature quickly rose, would have involved us in new perplexities.

I could have added striking examples of the influence of fogs on the sympiesometer in higher elevations, but they will be taken up in the Second Part of this Memoir, to which I must also refer all discussion of the fifteen values we have obtained for the height of the station before us, and a comparison with those obtained by geometrical methods.

Art. XI.—*On a new variety of Mineral Resin.* By James F. W. Johnston, M. A. &c. &c. Communicated by the Author.

While exploring the refuse heaps of an old lead mine called Settling Stones, about a mile above Newbrough Lodge, the seat of Nicholas Maughan, Esq. and six miles above Hexham, Northumberland, I met with several specimens of a substance having the following properties:—

Colour. Externally, red of various shades, black, and sometimes pale yellow, approaching to the colour of amber. Internally, red, or brownish-red, except in the yellow varieties, and by transmitted light of a brilliant deep red colour. It yields to the knife, but is hard, brittle, and has a bright glassy small conchoidal fracture. The fragments are transparent, and the fractured surfaces exhibit a pale greenish tinge, (an opalescence) which becomes more decided after the lapse of a few weeks; the transparency at the same time diminishing in a slight degree.

The specific gravity varies from 1.16 to 1.54 in the dark-red varieties.

In the flame of a candle it takes fire, burns afterwards of itself with considerable smoke, and an aromatic, slightly empyreumatic, odour, leaving a small coaly residuum.

On the sand bath, in a close tube, it gives off a small quantity of a transparent, colourless, and highly volatile naptha, having a peculiar odour, resembling that of some kinds of strong cheese. Heated to 400°, it does not melt, but assumes a bright black colour, though, when broken into fragments, it still transmits a rich red light. Over a spirit lamp it fuses, gives off a colourless naptha, a red empyreumatic oil, and leaves much charcoal.

It is insoluble in water, and is very slightly acted on by alcohol or ether. By hot concentrated nitric acid it is slowly, but entirely dissolved.

When rubbed it exhibits strong negative electricity.

Dr Brewster informs me, that, like amber, it has no crystalline structure.

This substance occurs along with brown spar (carbonate of iron,) and carbonate of lime, either in the form of little drops on the surface of the brown spar, where cavities occur in the vein, or in the midst of the massive brown spar, as if it formed part of the solid stone. In one specimen it rests upon carbonate of lime, containing crystals of Galena, and is covered with a mass of brown spar.

The brown spar in these cases forms thin layers, seldom exceeding an inch in thickness, either coating the surface of the blocks of trap which have been excavated, or alternating irregularly with thin portions of the same rock. The resinous matter has most probably an origin similar to that of common petroleum often found in various forms in lead veins; and the whin dike associated with or crossing the vein in this mine, appears to be connected with its formation. The position of the substance imbedded in stone, shows it to have remained in the same state for many ages;—it may be since the wall of trap began to cool. It is easy to see, that, on the eruption of such a dike of fluid matter, every combustible substance which came in its way would be decomposed, if susceptible of decomposition, and be driven into vapour; and that these vapours being confined where they had no outlet to the surface, must afterwards condense as the heat diminished, into oily and resinous substances of various kinds.

At Chapel Lime Quarry near Kirkcaldy, where the slaggy petroleum is found in some quantity, a product very similar to that above-described is occasionally found. The only specimen of it I have seen is in the possession of Mr Rose, mineral-dealer, and is in the form of small specks scattered here and there on crystallized carbonate of lime from that quarry. It is generally lighter coloured; but one small morsel is of the bright and beautiful red of that collected at Settling Stones.

The only mineral resin resembling the present, of which I have seen any description, is the mineral copal, or Highgate resin, found at Highgate in blue clay. The latter, however, melted by heat into a limpid fluid, a character which shows it to differ very much from that above-described.

The vegetable origin of amber seems now established beyond dispute. The collection of embalmed insects belonging to the University of Upsala, or the equally splendid private collection exhibited by Dr Berendt of Dantzick, at the late meeting in Hamburgh, appearing sufficient of themselves to convince the most sceptical. Yet it is not surprising that the occurrence of resinous substances like the foregoing, whose origin is incontrovertibly mineral, should be sufficient to lend plausibility to the opinion that amber is of mineral origin also.

PORTOBELLO, *7th December* 1830.

ART. XII.—*On Improved Methods of computing the Angles of Spherical Triangles when the sides are given.* By JAMES THOMSON, LL. D. Professor of Mathematics in Belfast College.

(To the Editor of the Edinburgh Journal of Science,)

SIR,

As even slight improvements in the solution of problems which are of frequent occurrence in practice, are of some importance, the following methods of computing the angles of plane and spherical triangles, when the sides are given, may interest some of your readers, they are contained in a small work of mine on Plane and Spherical Trigonometry, published a few months ago, and, so far as I know, they are new, with the exception of formulas (1) and (4.)

Let the angles be denoted by A, B, C, and the opposite sides by a, b, c; let also $s = \frac{1}{2}(a+b+c)$: then,

I. *In a plane triangle,*

$$\tan \tfrac{1}{2} A = \sqrt{\frac{(s-b)(s-c)}{s(s-a)}} \qquad - \qquad - \qquad (1.)$$

$$\tan \tfrac{1}{2} B = \frac{N}{s-b} \qquad - \qquad - \qquad - \qquad (2.)$$

$$\tan \tfrac{1}{2} C = \frac{N}{s-c} \qquad - \qquad - \qquad - \qquad (3.)$$

where $N = (s-a) \tan \frac{1}{2} A$.

II. *In a Spherical Triangle.*

$$\tan \tfrac{1}{2} A = \sqrt{\frac{\sin (s-b) \sin (s-c)}{\sin s \sin (s-a)}} \quad (4.)$$

$$\tan \tfrac{1}{2} B = \frac{N'}{\sin (s-b)} \quad (5.)$$

$$\tan \tfrac{1}{2} C = \frac{N'}{\sin (s-c)} \quad (6.)$$

where $N' = \sin (s-a) \tan \tfrac{1}{2} A$.

The first and fourth of these are given in all the works on trigonometry. To find the others we have in a plane triangle, according to (1.)

$$\tan \tfrac{1}{2} B = \sqrt{\frac{(s-a)(s-c)}{s(s-b)}}, \qquad \tan \tfrac{1}{2} C = \sqrt{\frac{(s-a)(s-b)}{s(s-c)}};$$

and in a spherical triangle, according to (4.)

$$\tan \tfrac{1}{2} B = \sqrt{\frac{\sin (s-a) \sin (s-c)}{\sin s \sin (s-b)}}, \quad \tan \tfrac{1}{2} C = \sqrt{\frac{\sin (s-a) \sin (s-b)}{\sin s \sin (s-c)}}.$$

Hence, by dividing successively the members of the first pair by those of (1,) and the members of the second pair by those of (4,) we obtain in the plane triangle,

$$\frac{\tan \frac{1}{2} B}{\tan \frac{1}{2} A} = \frac{s-a}{s-b}, \qquad \frac{\tan \frac{1}{2} C}{\tan \frac{1}{2} A} = \frac{s-a}{s-c}$$

and in the spherical triangle,

$$\frac{\tan \frac{1}{2} B}{\tan \frac{1}{2} A} = \frac{\sin (s-a)}{\sin (s-b)} \qquad \frac{\tan \frac{1}{2} C}{\tan \frac{1}{2} A} = \frac{\sin (s-a)}{\sin (s-c)};$$

from which, by multiplying by $\tan \frac{1}{2} A$, and denoting the common numerators $(s-a) \tan \frac{1}{2} A$, and $\sin (s-a) \tan \frac{1}{2} A$ by N and N′, we get the remaining formulas.

To exemplify these principles, let the sides a, b, c, of a plane triangle, be 679,537, and 429, to find the angles.

These data give $s = 822.5$, $s-a = 143.5$, $s-b = 285.5$, and $s-c = 393.5$; * and the rest of the work, by logarithms, stands as follows:

* In both plane and spherical triangles, the sum of the three remainders, $s-a$, $s-b$, $s-c$, is equal to the half sum s; and hence we have an easy mode of verifying the preparatory part of the computation.

s,	822.5	2.915136	Subt.
$s-a$,	143.5	2.156852	
$s-b$,	285.5	2.455606	
$s-c$,	393.5	2.594945	
		2)19.978563	
tan $\frac{1}{2}$ A 44° 17$\frac{1}{2}$′		9.989281	
A = 88° 35′.			

tan $\frac{1}{2}$ A - -	9.989281	Add.
log $(s-a)$ -	2.156852	
log N + 10 -	12.146133	
log $(s-b)$ -	2.455606	
tan $\frac{1}{2}$ B 26° 7$\frac{1}{2}$′ -	9.690527	
B = 52° 15′		
log N + 10 -	12.146133	
log $(s-c)$ -	2.594945	
tan $\frac{1}{2}$ C 19° 35′	9.551188	
C = 39° 10′		

In this operation only four numbers are required to be taken from the tables, those employed in the second column being all found in the first; and hence the great facility of the method is obvious. The angle C might be obtained from A and B. As it is found, however, with such ease as above, it is better thus to compute it, since the addition of the three angles affords a complete verification of the process.

As a second example, let the sides of a spherical triangle be 100°, 37° 18′, and 62° 46′.

Here we have $s = 100° 2'$, $s—a = 2'$, $s—b = 62° 44'$ and $s—c = 37° 16'$; and the rest of the work is as follows:

sin s	100° 2′	9.993307	subtract.
sin $(s—a)$	2	6.764756	
sin $(s—b)$	62 44	9.948845	
sin $(s—c)$	37 16	9.782132	
		22.972914	
tan $\frac{1}{2}$ A	88° 7′ 53″	11.486457	add
A =	176° 15′ 46″		
sin $(s—a)$		6.764756	
log N′		18.251213	subtract.
sin $(s—b)$		9.948845	
tan $\frac{1}{2}$ B	1° 8′ 57″	8.302368	
B =	2° 17′ 54″		
log N′		18.251213	subtract.
sin $(s—c)$		9.782132	
tan $\frac{1}{2}$ C	1° 41′ 13″	8.469081	
C =	3° 22′ 26″		

In this example, as in the foregoing, only four logarithms are to be taken from the tables for finding all the angles; and the arithmetical operations are of an easy kind. It is plain, that when the three angles are given, the sides may be found by a process resembling the foregoing, and equally easy.

JAMES THOMSON.

BELFAST, *Nov.* 26, 1830.

I beg to add also the following extract from "*An Introduction to the Differential and Integral Calculus*," which has been just published, relative to a subject of some interest in the history of science.

"394. Given the latitude of a place, and two circles parallel to the horizon; to find the declination of a body which, in its apparent diurnal motion, will pass from one of them to the other in the shortest time possible.*

"Let Z and P be the zenith and pole, and S and S′ the required points on the given parallels, having equal polar distances, P S and P S′. Now, since the time of describing the

* "'A single case of this problem, viz. to find the day of shortest twilight in a given latitude, employed, for several years, the two brothers. James and John Bernoulli, without success. By treating it algebraically, they were led to an equation of the fourth order, in which they were embarrassed to separate the useful roots from those which ought to be rejected; but, afterwards, by employing the synthetic method, they separately obtained answers very convenient for astronomical computation. In the year 1780, Fontaine attempted a solution by algebraic analysis. In this manner, he obtained an equation of the fourth order, which he required twenty quarto pages to reduce and explain.'—The foregoing extract is taken from a paper in the *Mathematical Companion* for 1805. Much information on the subject, with several solutions of the problem, will be found collected in the fourth volume of Leybourn's edition of the *Mathematical Questions proposed in the Ladies' Diary*. The method of solving this and similar problems, which is here given, was first pointed out by the author of this work, in the *Belfast Almanac* for 1828 and 1829. It has the advantage of extending the general method of determining maximums and minimums to a class of problems to which that theory was never applied successfully before, and of solving questions, once considered extremely difficult, in a more direct and, perhaps, more simple way, than by any other method that has yet appeared; thus affording a striking instance of the great power and excellence of the modern methods of investigation, when properly applied."

arc S S′ is a minimum, the angles S P S′ must also be a minimum. Hence (Sect. VII.),

$$\frac{d\,\mathrm{S\,P\,S'}}{d\,x}, \text{ or } \frac{d\,(\mathrm{P'}-\mathrm{P})}{d\,x}=0; \text{ whence } \frac{d\,\mathrm{P'}}{d\,x}=\frac{d\,\mathrm{P}}{d\,x}:$$

where P and P′ denote the angles Z P S and Z P S′, and x the polar distance P S or P S′. Now, putting the latitude $= l$, Z S $= a$, and Z S′ $= a'$, we have (formula 2, page 23)

$$\frac{d\,\mathrm{P}}{d\,x}=-\frac{\cot \mathrm{S}}{\sin x}, \quad \text{and } \frac{d\,\mathrm{P'}}{d\,x}=-\frac{\cot \mathrm{S'}}{\sin x};$$

and, therefore, by what we have just seen,

$$\frac{\cot \mathrm{S}}{\sin x}=\frac{\cot \mathrm{S'}}{\sin x}; \quad \text{whence, } \mathrm{S}=\mathrm{S'}.$$

Now, in the triangles P Z S and P Z S′ we have (Trig. No. 71)

$$\cos \mathrm{S}=\frac{\sin l-\cos a \cos x}{\sin a \sin x}, \quad \text{and } \cos \mathrm{S'}=\frac{\sin l-\cos a' \cos x}{\sin a' \sin x}.$$

Putting the second members of these equal to one another, multiplying by $\sin x$, $\sin a$, and $\sin a'$, we obtain, after transposition,

$$(\sin a' \cos a-\cos a' \sin a) \cos x=(\sin a'-\sin a) \sin l, \text{ or}$$

$$\sin (a'-a) \cos x=(\sin a'-\sin a) \sin l;$$

whence, by dividing by $\sin (a'-a)$, and by Trigonometry, No. 28,

$$\cos x=\frac{\cos \frac{1}{2}(a'+a)}{\cos \frac{1}{2}(a'-a)}. \sin l,$$

where $\cos x$ is the cosine of the polar distance, or the sine of the declination.

If $a=\frac{1}{2}\pi$, and $a'=\frac{1}{2}\pi+2\,b$, this becomes sin dec. $=-\tan b \sin l$. This formula solves the well-known problem in which it is required to determine the time at which the twilight is shortest in a given latitude, $2\,b$ denoting the sun's depression below the horizon, at the beginning of morning, or the end of evening twilight. If $2\,b$, as is generally supposed, be 18°, the result which we have obtained may be expressed by the following analogy, in which the negative sign shows that the latitude and the required declination are of contrary kinds :

Radius : sin lat. :: tan 9° : —sin dec.

" 394. 'Given the latitude of the place, and the positions of two hour circles with respect to the meridian; to determine the declination of that star whose change in altitude shall be the greatest possible in passing over the interval between those hour circles.'

"Here, in addition to the notation adopted in the last No. let Z S and Z S′ be represented by z and z'; and the angles P Z S and P Z S′, by Z and Z′. Then, since Z S′—Z S is a maximum, it might be shown, as in the last problem, and by formula 13, page 24, that

$$\frac{d\,z'}{d\,x} = \frac{d\,z}{d\,x}, \quad \frac{d\,z'}{d\,x} = \cos S', \text{ and } \frac{d\,z}{d\,x} = \cos S;$$

whence $\cos S' = \cos S$, and $S' = S$. Now we have

$$\cot S = \frac{\tan l \sin x - \cos P \cos x}{\sin P}, \text{ and } \cot S' = \frac{\tan l \sin x - \cos P' \cos x}{\sin P'}.$$

By putting these equal to each other, dividing by $\sin x$, multiplying by the denominators, and transposing, we get

$$(\sin P' \cos P - \cos P' \sin P) \cot x = (\sin P' - \sin P) \tan l, \text{ or}$$

$$\sin (P' - P) \cot x = (\sin P' - \sin P) \tan l.$$

Hence we obtain, by dividing by $\sin (P' - P)$, and by Trigonometry, No. 28,

$$\cot x, \text{ or } \tan \text{dec.} = \frac{\cos \frac{1}{2} (P' + P)}{\cos \frac{1}{2} (P' - P)} \tan l.$$

"This question is taken from *Gregory's Trigonometry*, page 243, where an erroneous answer, $\tan \text{dec.} = \frac{\sin \frac{1}{2} (P' - P)}{\sin \frac{1}{2} (P' + P)} . \tan l$, is given.

"Since $S = S'$, it would appear, by means of the formulas of the four sines that $Z = \pi - Z'$; whence it appears, that the azimuths of the body at the required positions are supplements of each other. It is also plain, that, if the angle S P S′ be bisected by the arc P Q, we shall have $\tan \text{dec.} = \frac{\cos Z P Q}{\cos S P Q} . \tan l$; that, at the equator, when $l = 0$, the declination must be nothing for every value of P and P′; and that, if $P = P'$, the formula will become $\tan \text{dec.} = \cos P \tan l$, an expression which will determine the declination of a star that, in crossing a given hour circle, will be increasing or diminishing its altitude more rapidly than any other star would in crossing the same hour circle. In this last case, it is evident, from the equation, $Z = \pi - Z$, that the star will cross the given hour circle and the prime vertical at the same time. Similar results for particular cases, besides that of the shortest twilight, might be derived, in a similar manner, from the solution of the problem in the last No. Other questions of this kind will be found in the next Section.

Art. XIII.—*Some general Remarks on Bodies having a like Composition, but unlike Properties.* By Professor Berzelius of Stockholm.*

By *Isomeric* (ισομερης) bodies, I understand those which, with a like chemical composition and atomic weight, possess unlike properties. There exists another class of bodies which, while they have the same composition per cent., have different weights. These are for the most part multiples of one another. Of this kind is carburreted hydrogen (Car. + 2 Hyd.) which, if the analyses are correct, forms, 1. Olefiant gas; 2. Another light gas, which condenses into an oil, with an atomic weight double of the former; 3. One or more crystalline bodies. These I do not include, since they must be better studied, and then probably distinguished by a new collective name.

Although we have for several years possessed well authenticated examples of isomeric bodies in the two different oxides of tin, composed of one atom metal and two atoms oxygen, as well as in the fulminic and cyanous acids, yet Clarke's paper on the difference between the common and the ignited phosphate of soda, his pyrophosphate, must be considered as having first led to a nearer study of these bodies. The *paratartaric* acid has presented itself at a seasonable time to throw farther light and certainty upon the matter.

The number of bodies which give isomeric combinations is probably great, though they have hitherto been little studied. I have several times observed that the ammonia subphosphate of magnesia, when first gently heated in a platinum crucible, to drive off the ammonia, and afterwards strongly ignited, showed the phenomena of ignition which I first ob-

* From a paper in the *Transactions of the Swedish Academy*, 1830.

served in some of the salts of the antimonic acid, and which has been since noticed in regard to zirconia, oxides of chromium and iron, carburet of iron, &c. In the phosphates, I could not reproduce this phenomenon at pleasure, and, therefore, I cannot give it as a property necessarily connected with their existence. It is enough for our present purpose that it occasionally takes place. It appears to indicate the *transition* from one isomeric modification to another, while the paraphosphate* which was put into the crucible is changed by ignition into the phosphate. It is probable, therefore, that all bodies which exhibit this phenomenon pass into another isomeric modification; although it does not follow from thence that this transition is always accompanied by the evolution of light, since we know that a chemical combination is often so accompanied, though in the greater number of cases it takes place without any of the phenomena of ignition. It is further probable that the speedy, yet permanent changes which certain bodies undergo by heating in liquids, during which, like the white of the egg, the colouring matter of the blood and fibrin, they pass from the soluble to the insoluble state, may be owing to a transition from one isomeric modification to another. On the other hand, the different dimorphous salts do not belong to this class of bodies, since their differences are entirely mechanical, and disappear by solution.

A very important, but as yet unanswerable question is, "Can the elements exist in two different states?" In one point of view this idea has no great degree of probability, and yet there are many facts which may be brought forward in support of it. For example, the different states of carbon in diamond and graphite, the differences in metallic platinum reduced, in the moist way from its salts by alcohol, or by igniting the double ammonium chloride; the differences in other metals, as, for ex-

* In another part of his paper, Berzelius proposes to distinguish the isomeric bodies by the particle παρα, denoting *change*, affixing it to that which has been modified and undergone the change where it can be ascertained. Hence, he names the pyrophosphoric simple phosphoric, as being the state of the newly prepared acid; the common phosphoric he names paraphosphoric, as being modified in some way by the agency of water. In the same way he calls the oxide of tin, thrown down by potash from the volatile chloride, the oxidum parastannicum.

ample, iron, according as it is reduced by hydrogen gas at a higher or a lower temperature; the dissimilar states of titanium and tantalum, when they are reduced by kalium, and afterwards freed from the latter by water, or when they are reduced by charcoal at a higher temperature, the unlike combustibility and solubility in fluoric acid of silicium before and after ignition, &c. &c.

Although, on the one hand, it must be granted that these differences may be accounted for by a dissimilar aggregation of the particles of bodies, yet, on the other hand, it must be remembered that the atoms of simple bodies may possibly, under different circumstances, be grouped together in more than one way for the formation of regular forms, and that groups united in different ways may produce different relations to light, and a different tendency to combination with other bodies.

The following bodies, in addition to the tartaric and paratartaric acids, have already been ascertained to belong to those which undergo isomeric modifications.

1. *Oxide* and *Chloride* of tin were the first bodies in which similarity of composition was ascertained with certainty to accompany dissimilar chemical properties. Of these differences I have given a circumstantial detail in my *System of Chemistry*. They came too unexpected to draw forth any remarks, and many have probably thought the statements founded on error.

In the titanic acid Heinrich Rose has found analogous isomeric modifications.

2. *Cyanous* and *Fulminic* acids form another well established example; and yet even this only gave birth to suspicions that there might be some errors yet undiscovered in the analysis which pointed them out as isomeric.

3. The *Phosphoric* acid gave birth to the idea of a like composition with unlike chemical properties. On this subject, Stromeyer has expressed himself the most decidedly. According to him, the difference lies not in the relation of the component parts, "but in the different ways in which they combine, and in the unlike condensation they have undergone." *

* See Stromeyer's paper in the preceding number of this *Journal*, p. 319.

With regard to the unlike condensation, it can be understood only of the phosphoric acid itself, and not of its component parts. On the other hand, Stromeyer has much darkened the question concerning these appearances by the experiments he has undertaken, since he draws from them the conclusion which very few will share with him, that these acids possess different capacities of saturation. These capacities he expresses by the quantity of oxide of silver which saturates 100 parts of the *ignited* and the *common* acids, (the phosphoric and paraphosphoric of Berzelius) and which, for the former, is 306.338, and for the latter, 504.412 parts. However, the capacity of saturation does not alter when the common phosphate of soda is changed by ignition into the other salt. I have, besides, in regard to the quantities of oxide of silver mentioned by Stromeyer, to remark, that they are incorrect not only in reference to the phosphoric acid when my atomic weight is taken, but also in regard to one another, so that they do not both correspond to the same atomic weight.

Again, as to the yellow phosphate of silver, I have already long ago * analyzed this compound, and found that 100 parts of phosphoric acid take up only 488 of oxide of silver. In regard to the atomic weight, Stromeyer's result is the mean of three experiments made in different ways, in which the quantity of oxide of silver differs half-a-per cent (from 83.183 to 83.712.) Errors of observation so large are now no longer admissible in chemical analyses, when these are so easily executed as in the present instance. I have therefore thought it unnecessary to confirm my old observations by any new analysis. The yellow phosphate of silver is $\dot{A}g^5\,\overset{\therefore\cdot\cdot}{P}{}^2$ (*a disesqui*-phosphate.)

The same objection lies against Stromeyer's analysis of what he calls the pyrophospate of silver, since he, from 100 parts of ignited phosphate of soda, by precipitation with nitrate of silver, obtained in one experiment 223.11, and in another 221.06 phosphate, (pyrophosphate, *) of silver. Here is again a difference of half-a-per cent. in results obtained in the same manner.

* *Afhandlingar i Fysik, Kemi och Mineralogi*, v. p. 400.

† In page 323 of our preceding number, 222.085 alone, the mean of these two results, is given.

Since I had no previous opportunity of analyzing this salt, I have lately investigated it, and found that the ignited phosphoric acid forms no less than three combinations with oxide of silver,—a *neutral*, a *bi*, and a *sesqui* salt. The last two are decomposed, though very slowly, by pure water, and, without considerable care, one may easily obtain a mixture of them with the neutral salt.

The *bi-phosphate* (bi-*pyro*phosphate of Stromeyer and others) is thrown down when a solution of ignited phosphoric acid in water is mixed with a solution of nitrate of silver. It may be washed with cold water till all the nitrate of silver is dissolved, without more than a very small portion of the salt being decomposed. At 100 C. (212 F.) it becomes soft and semifluid, and at a higher temperature it melts into a colourless and transparent fluid, which congeals on cooling into a brittle solid exactly resembling crystal glass. By an analysis of this salt I obtained 64.517 oxide of silver, and 35.483 phosphoric (pyrophosphoric) acid. Had the salt not been decomposed by the washing, I ought to have obtained 61.932 oxide of silver, and 38.068 phosphoric acid.

The *sesquiphosphate*, (sesquipyrophosphate,) is obtained by pouring boiling hot water on the newly precipitated biphosphate; by which means it is converted into a gummy unctuous turpentine like grey mass. This fused substance is sesquiphosphate, except in its inner parts, where, from the toughness of the substance preventing the water from reaching it, a little bi-phosphate remains. After treating for a short time with hot water, it is washed with cold, and is then much more difficult of fusion alone, than under warm water. The fused salt consists of 69.583 oxide of silver, and 30.417 phosphoric acid. If entirely free from *bi-phosphate* it should have given 70.933 base, and 29.067 acid. Lime, it is known, gives a similar turpentine-like clammy sesquiphosphate.

These salts were analyzed by solution in nitric acid, and precipitation of the silver in the state of chloride. I have not gone into the details, because it is impossible to obtain these salts perfectly pure, so that the results can only be regarded as an approximation.

Neutral phosphate, (pyrophosphate,) of silver is obtained by mixing a solution of pure ignited phosphate of soda, with a solution of pure, previously fused, nitrate of silver. The precipitate must be well washed, melted, when it gives an opaque glass resembling enamel, rubbed to powder and weighed in this state. Since Stromeyer in his experiments takes the double atom of chlorine at 4.5, instead of 4.4265, and a deviation from the true result must therefore arise from this cause, I considered it necessary so to devise my experiment as to remove this source of difference. I therefore decomposed the silver salt by heating with twice its weight of anhydrous carbonate of soda, in a platinum crucible previously glazed internally with carbonate of soda, that the silver might not come in contact with, and attach itself to the crucible. After heating gently for half an hour, it was brought nearly to a state of fusion. On cooling, the salt was dissolved in water, the metallic silver boiled out, and afterwards well washed on the filter with boiling water. 7.645 parts of the salt gave 5.435 parts of silver = 5.8571 oxide of silver, and therefore 100 parts consist of 76.351 oxide of silver, and 23.649 acid. As this is a little less than 76.49, the amount obtained by calculation, I treated the solution of phosphate, (pyrophosphate,) and carbonate of soda with muriatic acid, by which the fluid was rendered opalescent, showing it still contained a minute quantity of silver, though so small as to be incapable of accurate determination. The experiment, however, is sufficient to show that this silver salt has precisely the composition of a neutral phosphate of silver.*

When the acid liquid in which the sesquiphosphate is formed by heat is filtered and evaporated, there forms during the evaporation a crystalline enamel-like crust, which I have analyzed and found to be also the neutral phosphate. The residual liquid gave after evaporation a thick colourless syrup, consisting chiefly of phosphoric acid, which by *re*-solution in

* The conclusion from these experiments is, that Stromeyer has erred in considering the yellow phosphate to be a neutral salt, whereas it is a *bisesqui* salt, and, comparing the acid it contains with that present in the last of these described by Berzelius, which is actually a neutral salt, has deduced from this comparison the erroneous result, that the two phosphoric acids have different atomic weights or capacities of saturation. (See No. vi. p. 323.) *Tran.*

water left a gelatinous, but not yellow silver salt, which I have not yet analyzed.

I may here mention as a conjecture, that, although no isomeric combinations have yet been discovered of the arsenic acid, corresponding in their relations to oxide of silver with the ignited phosphoric acid, it would appear from the differences in its appearance, and its unlike solubility in water, that the arsenious acid is susceptible of two isomeric modifications.

4. Cyanogen, according to the experiments of Johnston, * may be obtained in two isomeric modifications, of which the one is gaseous cyanogen, and the other a solid black coaly looking mass, which remains on the decomposition of cyanide of mercury by distillation.

5. In organic nature, there appears to exist a great many isomeric bodies. The tartaric and paratartaric acids are the first accurately determined examples, but in a short time we are certain of finding more. Thus, for example, Prout has found that cystallized grape and diabetic sugars have precisely the same composition as milk sugar. Both contain water, the amount of which in grape sugar, is not ascertained. But if it be the same as in milk sugar, then it will follow that these bodies belong to the class I have called isomeric.

Art. XIV.—*On the Phenomena and Laws of Elliptic Polarization, as exhibited in the Action of Metals upon Light.* By David Brewster, LL. D. F. R. S. Lond. and Edin. †

From the first dawn of the science of polarization, the action of metals upon light has presented a troublesome anomaly. Malus at first announced that they produced no effect whatever; but by employing a different method of observation, I found that the light reflected by metallic surfaces was so far modified as to produce, when transmitted through thin crystallized plates, the complementary colours of polarized light. From a second series of experiments made previous to mine,

* *Edin. Jour. of Science*, N. S. July 1829, p. 119.

† From the *Phil. Trans.* 1830, p. 287. Read April 22, 1830.

Malus came to the conclusion, that the difference between transparent and metallic bodies consisted in this: that the former refract all the light which they polarize in one plane, and reflect all the light which they polarize in another; while metallic bodies reflect what they polarize in both planes.

Having discovered the property of transparent bodies to polarize light by successive reflexions at angles at which a single reflexion produced no perceptible, effect,* I resolved to apply this method of examination to metals; and on the 7th of February 1815, when I first made the experiment, I discovered the curious property possessed by silver and gold of dividing a polarized ray into complementary colours by successive reflexions. As this subject promised to open a wide field of inquiry, I prepared for the ardent prosecution of it with all the metallic bodies which could be procured; but the pressure of professional business prevented me for about a month from doing any thing very effectual.

On the 6th of March 1815, I received a letter from M. Biot, requesting some information on a matter of business; and in answering this letter on the same day, I communicated to him an account of the discovery above-mentioned.† Immediately after this I received the most perfect plates of silver, one pair polished by friction, and another by hammering; two pair of plates of gold, one of jewellers', and another of fine gold; with plates of steel, platinum, palladium, copper, brass, and speculum metal; and with their help I obtained the general result, that a single reflexion from a metallic surface produces the same effect upon polarized light as a certain thickness of a crystallized body, with many other results, which it is unnecessary here to indicate.

As soon as M. Biot had received notice of my discovery he seems to have devoted himself to the same inquiry; and with all the leisure of an Academician, and the splendid apparatus presented to him by the Institute, he obtained many of

* *Phil. Trans.* 1815, p. 142.

† It is related in the History of Optics, *Edinburgh Encyclopædia* vol. xv. p. 493, note, that I communicated this discovery to M. Biot on the day on which it was made;—this is a mistake, as it was done a month afterwards.

the results at which I had arrived, and others to which I have no claim; and on the 29th of March he transmitted to me, through Dr Wollaston, an open letter containing an abstract of his experiments, and expressing the hope that they would be of use to me in my researches.

Although this expression led me to believe that I should enjoy the privilege of publishing the first account of my own discovery, yet I took the precaution of having all my papers on the subject signed by the Treasurer of the Royal Society of Edinburgh, and I proceeded with new zeal in the further examination of the subject. I soon learned, however, from M. Biot, that he meant to treat the subject in his *Traite de Physique*; and though I remonstrated against this as a breach of courtesy, I had the mortification to see the discovery, to which I perhaps attached too much importance, published for the first time in a foreign work.

I trust the Society will excuse these details as a necessary apology for having so long delayed to fulfil the promise, more than once made in their *Transactions* to communicate to them an account of these experiments.* The reasons which I have assigned were subsequently strengthened by new inquiries which at first threw great doubts over the views which M. Biot and I had taken of the subject, and finally convinced me of the rashness of our generalizations. The study of M. Fresnel's fine discoveries respecting circular polarization enabled me to advance still further in the inquiry; and having more recently resumed the investigation, I trust I shall now be able to present to the Society a satisfactory analysis of the singular phenomena exhibited in the action of metals upon light.

* In a letter to Sir Joseph Banks, dated July 28th, 1815, I communicated an abstract of these and other experiments, with a request that he would permit the MS. to remain in his possession, as an evidence of my claims. Sir Joseph complied with this request: but nearly two years afterwards, happening to see the MS., he thought that it had been intended for publication, and laid it before the Royal Society without my knowledge. It was accordingly read on the 23d of January 1817, under the title of Abstract of Experiments on Light, and ordered to be printed. When the proof-sheet was sent me for correction, I requested the paper to be cancelled, as it was not intended for publication.

Sect. I.—*On the action of metals upon common light.*

When we analyze with a rhomb of calcareous spar a ray of common light, reflected at different angles from a metallic surface, there will be observed in one of the images a defalcation of light, as if a portion of the incident ray was polarized in the plane of reflexion. This effect will be still more distinctly seen if we examine the system of polarized rings formed round the axes of crystals by means of the light reflected from metals. If the light had suffered no modification by reflexion, or if the metal reflected in equal quantities the light polarized in opposite planes, the rings would not be visible at all; but it will be found that they are easily seen in the light reflected by all metals. They are most distinctly visible at an incidence of about 74°, at an average, and become fainter and fainter as the incidence exceeds or falls below that angle. They appear best defined in light reflected from galæna and metallic lead, and with least distinctness in light reflected from silver and gold, as shown in the following Table, in which the metals are arranged in the order in which they exhibit the rings most brightly, and consequently in the order in which they polarize the greatest quantity of light in the plane of reflexion:

Galæna,	Antimony,	Bismuth,	Grain tin,
Lead,	Steel,	Mercury,	Jewellers' gold,
Gray cobalt,	Zinc,	Copper,	Fine gold,
Arsenical cobalt,	Speculum metal,	Tin plate,	Common silver,
Iron pyrites,	Platinum,	Brass,	Pure silver,

If we now take two plates of each of these metals and examine the light which has undergone more than one reflexion, we shall find that the quantity of light which each polarizes in the plain of reflexion increases with each reflexion, and that in several of them the whole incident pencil is completely polarized.

When the luminous object is a wax-candle placed at the distance of ten feet, eight reflexions from a plate of steel at angles between 60° and 80° polarize the whole of the light, while at angles above 80° and below 60° a greater number of reflexions is required. With galæna, lead, cobalt, and antimony, a much smaller number of reflexions polarizes the whole pencil; whereas with pure and highly polished silver a very great number is necessary: the light reflected from the silver becomes redder and redder, indicating an increasing absorption or dispersion of the less refrangible rays.

By the use of common light it would be in vain to attempt to discover the law according to which the polarization of the incident pencil is effected in different metals; but by another mode of analysis we shall be led to the mathematical law for computing the exact proportion of the reflected pencil which is polarized at certain angles when the number of reflexions exceeds one.

Sect. II.—*On the action of metals upon polarized light.*

If a pencil of polarized light is received on a polished metallic surface placed so as to have a rotatory motion round the polarized ray, the reflected light will receive no modification, (excepting what arises from its property of apparently polarizing a portion of light in the plane of reflexion,) when the plane of incidence is inclined 0°, 90°, 180°, and 270° to the plane of primitive polarization; but in every other azimuth of the plane of incidence the reflected pencil will be found to have suffered a remarkable change, which gradually increases as the azimuth of that plane varies from 0° to 45°, from 90° to 135°, from 180° to 225°, and from 270° to 315°. At the azimuths of 45°, 135°, 225°, and 315°, the effect is a maximum, and it gradually diminishes from 45° to 90°, from 135° to 180°, from 225° to 270°, and from 315° to 360°.

In order to investigate the nature of this change, we shall suppose the plane of reflexion from the metal to be inclined — 45°, or to the left of the plane of primitive polarization. In this position let a plate of highly polished steel receive the polarized ray of ordinary intensity. At 89°, 88°, and 87° of incidence, almost no change is produced upon it by the action of the metal. We can easily see that the plane of polarization of the ray is turned from right to left, exactly as it would be by a transparent surface. In like manner, at all angles of incidence from 0° to about 40° no decided effect is produced, except the change in the plane of polarization. At angles less than 87° the change begins to appear, reaches its maximum at about 75°, and diminishes gradually to 40°. By means of the analyzing rhomb, it is easily seen that a great portion of the original pencil has had its plane of polarization changed from $+ 45°$ to $0°$, as the incidence diminishes from $75°$ to $0°$. If, indeed, we measure the rotation of the principal section of the rhomb when the extraordinary pencil is a minimum at different angles of incidence, we shall find it to correspond with $45° - \varphi$, φ being calculated from the formula $\tan \varphi = \frac{\cos (i + i')}{\cos (i - i')}$ in which $\frac{\sin i}{\sin i'} = 3.732$, the index of refraction for steel. The value of φ will be found to be nearly the same at $87°$ and $40°$, which shows why at these two angles the change under our consideration is just beginning to appear with light of ordinary intensity.

The physical effect of the metallic surface being a maximum at $75°$, we shall now examine the character of the pencil reflected at that angle.

1. The pencil thus reflected is not polarized light, because it does not vanish during the revolution of the analyzing rhomb.

2. It is not common light, because when we reflect it a second time at $75°$ from another steel surface, it is restored to light polarized in one plane.

In order to discover its nature, let it be transmitted along the axis of calcareous spar. The system of rings is changed almost exactly in the same manner as if a thin film of a crystallized body which polarizes the pale blue of the first order had crossed the system. If we substitute for the calcareous spar films of sulphate of lime, which give different tints, we shall find that these tints are increased according as the metallic action coincides with, or opposes that of the crystal.

On the authority of this experiment I was led to believe that metals acted upon light like crystallized plates; and when I found that the colours were not only better developed, but more pure after successive reflexions, it was a natural, though a rash, generalization, to conclude as I did, and as M. Biot did after me, that each successive reflexion corresponded to an additional thickness of the crystallized film.

In order to show the incorrectness of this deduction, let a ray polarized $+ 45°$ be reflected twice from steel an angles of

75°. In this case the effect of the second reflexion should be to double the tint produced by the first, if the tints are those of crystallized plates. The result, however, is, that the whole of the light is polarized in one plane, in place of consisting of two pencils polarized in opposite planes. M. Biot got over this embarrassment by regarding the tint produced by two reflexions as the white of the first order, which, in consequence of its complementary tint being black, is the only one where the light is all polarized in one plane: but had he examined the light reflected four times, six times, or eight times at 75°, he would have still found it all polarized in one plane, a result entirely incompatible with the supposition of the tints rising with the number of reflexions. That the tint is not the white of the first order may be more easily proved by making it pass along the axes of the calcareous spar; for we shall find that in place of producing an increment of tint, the effect of the second reflexion has been to destroy entirely the effect of the first, and to restore the ray to common polarized light. All this will appear by the perfection of the system of rings seen through the spar. If we examine in a similar manner the light which has undergone any number of reflexions between the plates, we shall easily ascertain that the effect never exceeds that of a quarter of a tint in Newton's scale.

Having thus ascertained that light polarized + 45°, and reflected at the maximum polarizing angle of metals, is neither common light nor polarized light, nor light constituted like that which passes through thin crystallized plates, I conceived the idea of its resembling circularly polarized light—that remarkable species of light which comports itself as if it revolved with a circular motion during its transmission through particular media.

According to Fresnel's beautiful discovery, a ray of light polarized + 45° is circularly polarized when it has suffered two total reflexions from glass at an angle of $54\frac{1}{2}$°; and when such a ray is made to suffer other two reflections at the same angle, it is restored to the state of light polarized — 45° to the plane of reflexion, whatever be the azimuth of the second plane of reflexion in relation to the first. In like manner I shall proceed to show that a ray of light polarized + 45°, and reflected once at the maximum polarizing angle from metals and certain metalic ores, has an analogous polarization, viz. a polarization hitherto unrecognized, and intermediate between circular and rectilineal polarization.

Let the ray polarized + 45° be reflected at 75° from steel, and let a second plate of steel be made to turn round the ray thus reflected. At the azimuths of 45°, 135°, 225°, and 315°, with the plane of primitive polarization, that is, when the planes of the two reflexions are either coincident or rectangular, the first reflected ray will be restored to polarized light at an incidence of 75°. At azimuths of 0° and 180° the restoration will be effected at an incidence of 80°, while at azimuths of 90° and 270° it will take place at an incidence of 70°, and at intermediate azimuths it will take place at intermediate incidences. Hence the ray of light reflected from steel, though it has the general properties of a circularly polarized ray, differs from it in this remarkable particular, that it requires different angles of incidence in different azimuths to restore the polarized light.

In circular polarization, as we have seen, the ray has the same properties in all its sides; and the angles of reflexion at which it is restored to polarized light in different azimuths are all equal, like the radii of a circle described round the ray. Hence, without any theoretical reference, the term circular polarization is from this and other facts experimentally appropriate. In like manner, without referring to the theoretical existence of elliptic vibrations produced by the interference of two rectilineal vibrations of unequal amplitudes, we may give to the new phenomena the name of elliptic polarization, because the angles of reflexion at which this kind of light is restored to polarized light may be represented by the variable radius of an ellipse.

In circular polarization the restored ray has its plane of polarization always inclined — 45° to the plane of the second system of reflections. In elliptic polarization the difference is remarkable. The inclination of the plane of the restored pencil is likewise —, but always less than 45°, as will appear from the following Table, which contains the greater number of metallic bodies:

Names of Metals.	Angles of Restoration. °	′	Names of Metals.	Angles of Restoration. °	′
Total reflexions	45	0	Bismuth -	21	0
Pure silver	39	48	Speculum metal	21	0
Common silver	36	0	Zinc -	19	10
Fine gold	35	0	Steel -	17	0
Jewellers' gold	33	0	Iron pyrites	14	0
Grain tin	33	0	Antimony	16	15
Brass -	32	0	Arsenical cobalt	13	0
Tin plate	31	0	Cobalt -	12	30
Copper -	29	0	Lead -	11	0
Mercury -	26	0	Galæna -	2	0
Platina -	22	0	Specular iron	0	0

The bodies in this Table are obviously in the inverse order according to which they polarize most light in the plane of reflexion.

I have inserted at the top of the Table the inclination of the restored pencil in total reflexions, which is 45°; and at the bottom, that of specular iron, which is 0°; in order to show the transition from elliptic polarization to circular polarization on the one hand, and to rectilineal polarization on the other.

In these experiments the primitive ray was polarized + 45° to the plane of reflection; but when this angle diminishes, the plane of the restored ray approaches to the plane of reflexion, and ultimately coincides with it at 0°; and when this angle increases, the plane of the restored ray recedes from the plane of reflexion, and the two planes form an angle of 180° when the other angle becomes 90°

The following experiments were made with plates of pure silver, in which the inclination φ was 39° 48′, when the inclination x of the plane of polarization was 45°.

Inclination x of the Plane of primitive Polarization to Plane of Reflexion.	Observed Inclination of the restored ray to the Plane of reflexion or φ.	Inclination φ calculated by the Formula.
+ 90°	— 90° 0′	— 90° 0′
85	84 36	84 0
75	74 10	72 10
65	63 51	60 46

55°		52° 18′	49° 57′
45	$\theta =$	39 48	39 48
35		32 23	30 28
25		23 10	21 14
15		13 16	12 35
5		4 40	4 10
0		0 0	0 0

Calling θ the inclination or value of φ at 45°, we may represent these observations by the formula, $\tan\varphi = \tan\theta \tan x$, and the actual change of the plane of polarization, or R, will be $R = x + \varphi$.

When φ is given, $\tan x = \frac{\tan\varphi}{\tan\theta}$, and when $\varphi = 45°$, and consequently $\tan\varphi = 1$, we have, $\cot x = \tan\theta$, and $x = 90° - \theta$.

Since light polarized + 45° is elliptically polarized by one reflexion from steel at 75°, and is restored to light polarized − 17° by a second reflexion at 75°, it is clear that a third reflexion at 75° will again polarize it elliptically, while a fourth reflexion at 75° will again restore it to light polarized $+\varphi$, φ being a quantity less than 17°, and given by the preceding formula. The same effects will be reproduced with different numbers of reflexions, as in the following Table.

No. of Reflexions from Steel at 75° of Incidence.	State of the Light Reflected.	Inclination of the Plane of Polarization. Observed.	Calculated.
1	Elliptically polarized	° ′	° ′
2	Restored to light polarized	−17 0	−17
3	Elliptically polarized		
4	Restored to light polarized	+ 5 10	+ 5 22
5	Elliptically polarized		
6	Restored to light polarized	− 2 0	− 1 38
7	Elliptically polarized		
8	Restored to light polarized	0 0	+ 0 30
9	Elliptically polarized		
10	Restored to light polarized	0 0	− 0 9
11	Elliptically polarized		
12	Restored to light polarized	0 0	+ 0 3

Hence it follows, that at every odd number of reflexions at the maximum polarizing angle the light is elliptically polarized, and at every even number it is restored to a single plane of polarization. In circular polarization the inclination φ of this plane is always $\mp 45°$, even after fifty reflexions, as I have ascertained by direct experiment; but in elliptical polarization the inclination diminishes at every restoration; and in the case of steel it is reduced to near 0° after eight reflexions, when the light is all polarized in the plane of reflexion; that is, the elliptic polarization gradually diminishes and terminates in rectilineal polarization.

The value of φ, as given in the preceding Table, and consequently the number of reflexions when it approaches to 0°, may be deduced from the formula,

$$\tan \varphi = \tan \theta \,.\, \tan x.$$

After the first reflexion $x = +45°$, and φ, or the inclination of the plane of the ray as restored by the second reflexion, is $= -17°$, as given by experiment. Hence the light which suffers the third reflexion, and is thereby elliptically polarized, is not, as originally, polarized $+45°$, but only $-17°$; and consequently, when it is restored after the fourth reflexion, the value of φ must be such as corresponds to an equality in the values of x and θ, both of them being $= 17°$. Hence the formula becomes,

$$\tan \varphi = \tan^2 x, \text{ or } \tan \varphi = \tan^n x;$$

n being the number of pairs of reflexions, or half the number of reflexions which the restored ray has undergone. In this way the last column of the preceding Table has been calculated. The same formula represents also, as it should do, the phenomena at the limits of elliptic polarization. In the case of circular polarization, where the plane of polarization of the restored ray is 45°, we have,

$$x = 45°, \tan x = 1, \text{ and } \tan \varphi = \tan^n x = 1, \text{ or } \varphi = 45°$$

after any number of reflexions however great. In like manner, in rectilineal polarization, where $x = 0°$, we have $\varphi = 0°$, that is, the ray is polarized in the plane of reflexion.

The above formula is suited to any series of reflexions at any angle when the value of φ for the first term of the series is known. The value of φ for two reflexions, the first term of the principal series, can be determined only by experiment, and has been given in a former table for several metals; but we may determine from it the value of φ for the first term of any other series, provided it is an even number, in the following manner. Making $x =$ the inclination for two reflexions at the maximum polarizing angle, and φ the value of x at any number of reflexions $2n$, we shall have,

$$\tan \varphi = \frac{\tan x + \tan^n x}{2}; \qquad \text{(A)}$$

where $\tan^n x$ is the value of φ at the maximum polarizing angle for $2n$ reflexions; but as no odd number can occur in the principal series, the preceding rule will not apply to such numbers.

The following Table shows the coincidence between the formula and experiment.

SILVER.

Number of Reflexions.	Values of n.	Angle of Incidence.	Inclination of Plane of Polarization. Observed.	Calculated.
		° ′	° ′	° ′
2	1	73 0	39 48	39 48
4	2	82 30	37 45	37 22
6	3	85 6	35 0	35 22
		STEEL.		
2	1	75 0	17 0	17 0
4	2	83 30	11 30	11 17
6	3	85 45	9 30	9 30

When the number of reflexions which begin the series is odd or fractional, we must determine, by the preceding formula, the value of φ for the even number immediately above it: and calling ν the number of odd or fractional reflexions, and N the number of even reflexions immediately above ν, φ the inclination for N reflexions as given by the formula (A), and φ the inclination required, we shall have,

$$\tan \varphi' = \tan x - (\nu - 2) \left(\frac{\tan x - \tan \hat{}}{N - 2}\right). \qquad \text{(B)}$$

The truth of this formula will appear from the following table:

Silver.

Number of Reflexions.	Angles of Incidence.	Inclination of the Plane of Polarization. Observed.	Calculated.
	° ′	° ′	° ′
3	79 40	38 28	38 33
5	77 13	33 10	33 36
5	84 5	26 0	26 24

Steel.

3	77 37	13 15	14 11
5	84 38	10 30	10 23

The same results will be obtained at the angles of equal phase below the maximum polarizing angle.

This last rule is suited to even as well to odd numbers of reflexions, but it does not give precisely the same results for even numbers as the formula (A). The difference, however, is far within the limits of the errors of observation. The inclination, for example, at 4 reflexions, is by formula (A) 37° 22′ for silver, whereas by formula (B) it is 37° 34′ the difference being only 12 minutes.

In circular polarization, therefore, the plane of polarization of the restored light continues by successive reflexions to oscillate on each side of the plane of reflexion with a never-varying amplitude from + 45° to — 45°; while in elliptical polarization, the same plane oscillates with an amplitude continually diminishing till it is brought to nothing in the plane of reflexion.

In steel, as we have seen, the polarization is highly elliptical, and the amplitude of the oscillations of the plane of restoration is quickly brought to zero; but in silver, where the polarization approaches nearly to circular, the oscillations diminish very slowly in amplitude, as the following table shows.

No of Reflex. from Silver at 73° of Incidence.	State of the Reflected Light.	Inclination of the Plane of Polarization, or φ. Observed.	Calculated.
1	Elliptically polarized,	° ′	° ′
2	Restored to light polarized,	—38 15	—38 15
3	Elliptically polarized,		
4	Restored to light polarized,	+31 15	+31 52
5	Elliptically polarized,		

6	Restored to light polarized,	$-26^\circ\ 0'$	$-26^\circ\ 6'$
8	Restored to light polarized,		$+21\ \ 7$
10	Restored to light polarized,		$-16\ 56$
12	Restored to light polarized,		$+13\ 30$
18	Restored to light polarized,		$-\ 6\ 42$
36	Restored to light polarized,		$+\ 0\ 47$

Owing to the high dispersive power of silver, I found it difficult to carry the comparison any further with white light, as the colours closed in upon the points of evanescence, and rendered it impossible to determine with any precision the inclination of the plane of polarization.

The preceding results afford the clearest explanation of the phenomena which steel and silver exhibit in the reflection of common light. As common light is similar to two equal pencils polarized $+45^\circ$ and -45°, and as steel brings two such pencils into a state of parallelism with the plane of reflexion, common light must therefore be wholly polarized in the plane of reflexion after eight reflexions. In like manner we see why the same effect is not produced by silver, because, after eight reflexions, the two planes of the pencils are inclined 42°, or $2 \times 21^\circ\ 7$, so as to form a partially polarized pencil.

The same results also furnish us with a method of computing the proportion of polarized light in any pencil of common light, reflected from metals at angles at which the restoration of the elliptical polarized pencil is effected. In order to determine this proportion for steel after two reflexions at 75°, we must consider that a pencil polarized $+45^\circ$ is restored by these two reflexions to light polarized -17°, and consequently a pencil polarized -45° to light polarized $+17$. Hence a beam of common light will consist, after two reflexions, of two pencils $+17^\circ$ and -17° of equal intensity, and consequently in the same state of partial polarization, as if common light had been reflected either at an angle of 45° or 68° from a surface of glass. Consequently in the formula*

$Q = 1 - 2 \sin^2 \varphi$, we have $\varphi = 17^\circ$ and $Q = 0.829$.

Hitherto we have considered elliptical polarization as pro-

* See my paper "On the Law of the Partial Polarization of Light by Reflexion," *supra*, p. 76.

duced only at the maximum polarizing angle. It may be produced, however, by a sufficient number of reflexions at any given angle either above or below the maximum polarizing angle, as appears from the following table, in which the reflexions are made from two parallel plates of steel.

No of Reflexions from Steel at which Elliptic Polariz. is produced.	No of Reflexions at which the pencil is restored to a single plane.	Angles of Incidence. Calculated.	Observed.
		° ′	° ′
3, 9, 15, &c.	6, 12, 18, &c.	85 45	86 0
$2\frac{1}{2}$, $7\frac{1}{2}$, $12\frac{1}{2}$, &c.	5, 10, 15, &c.	84 38	84 0
2, 6, 10, &c.	4, 8, 12, &c.	83 30	82 20
$1\frac{1}{2}$, $4\frac{1}{2}$, $7\frac{1}{2}$, &c.	3, 6, 9, &c.	79 39	79 0
1, 3, 5, &c.	2, 4, 6, &c.	75 0	75 0
$1\frac{1}{2}$, $4\frac{1}{2}$, $7\frac{1}{2}$, &c.	3, 6, 9, &c.	68 53	67 40
2, 6, 10, &c.	4, 8, 12, &c.	60 2	60 20
$2\frac{1}{2}$, $7\frac{1}{2}$, $12\frac{1}{2}$, &c.	5, 10, 15, &c.	56 5	56 25
3, 9, 15, &c.	6, 12, 18, &c.	51 24	52 20

The numbers given in the third column are calculated by the following method. The relation of the preceding phenomena to the angle of maximum polarization is obvious; and if we consider the nature of the formula, $\tan \varphi = \frac{\cos (i + i')}{\cos (i - i')}$, we shall see that the angles at which the rectilineal polarization of the primitive pencil is destroyed have a reference to the rotation which the reflecting surface produces in the plane of polarization. The angles indeed in the third column, at which similar effects are produced above and below 75°, are those at which φ has equal values. This is a very important relation, and enables us to determine the phase P of the two inequal portions of oppositely polarized light, by the interference of which the elliptic polarization is produced. It may be expressed by $P = 2\,R$.

But $R = 45° - \varphi$,

Hence $P = 90° - 2\,\varphi$,

$$\tan \varphi = \frac{\cos (i + i')}{\cos (i - i)}.$$

In this manner we obtain the following results.

No Reflex. for Elliptic Polariz.	Angle of Incidence on Silver.	Angle of Incidence on Steel.	Inclination of plane, or φ.	Rotation of plane, or R.	Phase, or P.
	° ′	° ′	° ′	° ′	° °
3	85 6	85 45	30 0	15 0	30 = $\frac{1}{3}$ of 90
$2\frac{1}{2}$	83 49	84 38	26 15	18 45	$37\frac{1}{2}$ = $\frac{5}{12}$ of 90
2	82 30	83 30	22 30	22 30	45 = $\frac{1}{2}$ of 90
$1\frac{1}{2}$	78 8	79 39	11 15	33 45	$67\frac{1}{2}$ = $\frac{3}{4}$ of 90
1	73 0	75 0	0 0	45 0	90 = $\frac{1}{1}$ of 90
$1\frac{1}{2}$	66 25	68 53	11 15	33 45	$67\frac{1}{2}$ = $\frac{3}{4}$ of 90
2	57 16	60 2	22 20	22 30	45 = $\frac{1}{2}$ of 90
$2\frac{1}{2}$	53 17	56 5	26 15	18 45	$37\frac{1}{2}$ = $\frac{5}{12}$ of 90
3	48 38	51 24	30 0	15 0	30 = $\frac{1}{3}$ of 90

In the results of the two preceding Tables, where the number of reflexions is an integer, it is easily understood how an elliptically polarized ray begins to retrace its course, and recover its state of polarization in a single plane, by the same number of reflexions by which it lost it: but it is interesting to observe, when the number of reflexions is $1\frac{1}{2}$, $2\frac{1}{2}$, $3\frac{1}{2}$, that the ray must have acquired its elliptic polarization in the middle of the second and third reflexion; that is, when it had reached its greatest depth within the metallic surface. It then begins to resume its state of polarization in a single plane, and recovers it at the end of 3, 5, and 7 reflexions. This stationary point at which the retrograde effect commences, may be made to have its position at any depth beneath the surface, by changing the angles of some of the reflexions, or by combining plates of metal of different polarizing powers.

The same curious property is exhibited in total reflexions, as I have found that the circular polarization can be produced by $2\frac{1}{2}$, $3\frac{1}{2}$, &c. reflexions.

Hitherto we have chiefly examined the phenomena when the reflexions are performed either all above or all below the polarizing angle. We shall now proceed to the case when one reflexion is made on one side, and one on the other side of the maximum polarizing angle.

When a ray polarized + 45° has been reflected once from steel at an angle of 85° or of 54°, it has acquired partially the state of elliptic polarization, and to such a degree that three reflexions more at the same angle will complete the effect. But if the ray partially polarized elliptically by one reflexion at 85° suffers a second reflexion at 54°, it does not acquire more elliptic polarization, but it retraces its course, and recovers its state of single polarization. The same phenomenon occurs at the following angles.

Angles of partial Elliptic polarization.	Values of φ	Angles at which it recovers its Polarization.	Values of φ
1 Reflex. at $87\frac{1}{2}$°	36° 5′	1 Reflex. at 41°	36° 11′
85	27 28	54	20 0
80	12 12	68	12 36
77	5 24	72	5 59
75	0 0	75	0 0

It is obvious, by comparing these angles with those in the preceding Table, that they correspond, and are those at which equal phases or rotations are produced.

The effect of two reflexions, at angles of equal phase, upon the inclination I of the plane of polarization is shown in the following Table.

	Inclination I of the Plane of Polarization. Observed.	Calculated.
1 Reflex. at 90° and 1 at 0°	45°	45° 0′
$87\frac{1}{2}$ 41	—	30 0
85 54	26	26 5
80 68	19	20 8
77 72	18	17 2
75 75	17	17 0

The last column of the table is calculated by the formula

$$I = \tan\varphi\,(45° - i') + i',$$

i being 17°, or the inclination after two reflexions at the maximum polarizing angle.

In the preceding inquiry we have considered only the phenomena when the consecutive reflexions are performed in coincident planes. The investigation becomes more troublesome, and the results more interesting when the plane of the second reflexion is presented in every different azimuth to the ray that is either wholly or partially elliptically polarized by the first reflexion.

Let a pencil be elliptically polarized by one reflexion from steel at 75°, and let the azimuths be reckoned from the plane of this reflexion. We have already seen that a second reflexion at 75° in azim. 0° and 180° restores the pencil to a single plane of polarization; but if we turn the plane of the second reflexion into axim. 45° or 225°, we shall find that the angle of restoration is no longer 75°, but 78°. At azim. 90° and 270° it is again 75°, and in azim. 135° and 315° it is only 68°, having varied from 68° to 78°.

The following table shows the observed and calculated angles of restoration in different azimuths.

Azimuths from Plane of first Reflexion.		Angles of Restoration from Steel.	Complement of Angles of Restoration or Elliptical Radii. Observed.	Calculated.
0° and	180°	75°	15°	14.9°
22½	202½	77	13	12.7
45	225	78	12	12
67½	247½	77¾	12¼	12.7
90	270	75	15	14.9
112½	292½	70	20	19
135	315	68	22	22
157½	337½	70	20	19
180	360	75	15	14.9

The radii in the two last columns are obviously those of a curve approaching to an ellipse whose major and minor axes are situated, the one 45° to the right, and the other 45° to the left of the plane of the first reflexion. The major semiaxis is 22° and the minor 12°. Hence calling x the variable radius of the ellipse, a the greater, and b the lesser semiaxis, and θ the azimuth, reckoned from the lesser axis, in which the radius x is wanted, we shall have

$$x = \frac{a\,b}{\sqrt{a^2 \cos^2 \theta + b^2 \sin^2 \theta}}.$$

When $\theta = 45°, 135°$, &c. $\sin^2 \theta \cos^2 \theta = \frac{1}{2}$ and

$$x = \frac{a\,b}{\sqrt{\frac{1}{2}\,a^2 + \frac{1}{2}\,b^2}}$$

By calculating the values of x corresponding to the azimuths in the table, we obtain the numbers in the last column, which are so near the observed numbers as to leave no doubt that an ellipse represents the observations.

If we perform the same experiments with a plate of silver at 73°, we shall observe, with surprise, that the angle of restoration is the same in all azimuths, that is, that the ellipse has merged into the circle. There is a slight deviation indeed, just sufficient to show that the circle is slightly oval, but I could not measure the amount of it.

This result arises from the elliptical polarization of silver being very nearly circular. If we call β the angle of restoration after two reflexions, the ratio of a to b, the major and minor axes of the ellipse, may be thus expressed:

$$a : b = \sin 2\,\beta : \text{rad.}$$

In steel, where $\beta = 17°$ and $2\,\beta = 34°$, we have $a : b = 0.559 : 1 = 10 : 21\frac{1}{2}$, differing very little from $10 : 22$ the actual ratio.

In silver, where $\beta = 39°\ 48'$, $a : b = 0.9835 : 1 = 17 : 17\frac{1}{4}$.

In circular polarization, where $\beta = 45°$, $a : b = 1 : 1$, which gives a circle.

In rectilineal polarization, where $\beta = 0$, $a : b = 0 : 1$, which gives a straight line.

It now becomes an interesting subject of inquiry to ascertain the form and position of the ellipse, when the angle of incidence on the first plate exceeds or falls below the maximum polarizing angle.

The following experiments were made with silver at angles of incidence of 80° and 68°, the maximum polarizing angle being 73°.

Silver.—Angle of Incidence on First Plate 80°.

Azimuth to Right.	Complement of Angle of Restoration by 2d Plate.	Azimuth to Left.	Complement of Angle of Restoration by 2d Plate.
0°	28° 2′	0°	28° 2′
$11\frac{1}{4}$	26 35	$11\frac{1}{4}$	24 40
$22\frac{1}{2}$	25 20	$22\frac{1}{2}$	21 0
$33\frac{3}{4}$	21 13	$33\frac{3}{4}$	16 40
45	18 20	45	14 35
$56\frac{1}{4}$	14 20	$56\frac{1}{4}$	11 10
$67\frac{1}{2}$	11 32	$67\frac{1}{2}$	10 0
$78\frac{3}{4}$	10 15	$78\frac{3}{4}$	10 0
90	10 0	90	10 0

SILVER.—Angle of Incidence on First Plate 68°.

0°	13°	0°	13°
$11\frac{1}{4}$	14	$11\frac{1}{4}$	13
$22\frac{1}{2}$	$15\frac{1}{2}$	$22\frac{1}{2}$	$13\frac{1}{2}$
$33\frac{3}{4}$	16	$33\frac{3}{4}$	14
45	17	45	$14\frac{1}{2}$
$56\frac{1}{4}$	19	$56\frac{1}{4}$	$15\frac{1}{2}$
$67\frac{1}{2}$	20	$67\frac{1}{2}$	$16\frac{1}{2}$
$78\frac{3}{4}$	20	$78\frac{3}{4}$	18
90	20	90	20

In the first of these sets of experiments, the semiaxes of the ellipse are as 10° to 28°, and its major axis is in azim. 0° and 180° or in the plane of the first reflexion.

In the second series the ratio of the semiaxes is as 13° to 20°, and the major axis is in azim. 90° and 270°, or perpendicular to the plane of the first reflexion; but in both series there is a want of symmetry in the curve to the right of azim. 0° where it bulges out, showing that in both series the greater axis is a little to the right of azim. 0°.

Hence it appears, that in silver, whose elliptic polarization is nearly circular, the ellipse which regulates the angles of restoration has its greater axis in the plane of the first reflexion for all angles greater than 73°, the maximum polarizing angle; and from a circle it increases in ellipticity till at the limit of 90° the lesser semiaxis is 0°, and the greater 90°, and it becomes a straight line. For angles above 73° the ellipse has its greater axis perpendicular to the plane of reflexion, and gradually increases in ellipticity from the circle till at the limit of 0° its lesser semiaxis is 0°, and its greater 90°, when it becomes a straight line.

The peculiar character of elliptic polarization shows itself in another manner, and with peculiar interest, in the variable position of the ellipses which regulate the angles of restoration upon steel.

We have already seen that the curve which is circular in silver at the maximum polarizing angle, is in steel an ellipse whose semiaxes are as 12° to 22°, the greater axes being inclined 45° to the right of azim. 0°.

The following table will show how the effect varies at angles of incidences above and below the polarizing angle

Steel.—Angle of Incidence 80°.

Azimuth to Right.	Complement of Angle of Restoration by Second Plate.	Azimuth to Left.	Complement of Angle of Restoration by Second Plate.
0 °	23°	0 °	23°
11¼	25	11¼	20
22½	26	22½	16⅓
33¾	24	33¾	13
45	20½	45	11½
56¼	18	56¼	10
67½	15⅓	67½	9½
78¾	11	78¾	9¾
90	10	90	10

Steel.—Angle of Incidence 68°.

Azimuth to Right.	Complement of Angle of Restoration by Second Plate.	Azimuth to Left.	Complement of Angle of Restoration by Second Plate.
0 °	11°	0 °	11°
11¼	24	11¼	10
22½	24½	22½	9
33¾	25½	33¾	9¾
45	26⅓	45	11
56¼	25⅓	56¼	15
67½	20	67½	18
78¾	21	78¾	20
90	22	90	22

By comparing these results with those obtained from steel at 75°, and with the observations already made on the passage of the ellipse into a straight line, the following results may be deduced.

Angle of Incid. on first Steel Plate.	Ratio of Semiaxes of the Ellipse.	Character of the Ellipse.	Position of the greater Axis of the Ellipse.
0°	0 ° : 90°	Straight line	Azim. 90° and 270°.
68	9 : 26	Ellipse	—— betw. 45° and 56° to R.
75	1 : 22	Ellipse	—— - - 45° to R.
80	9½ : 26	Ellipse	—— - - 22½° to R.
90	90	Straight line	—— - - 0

Hence it is obvious that the major axis of the ellipse is 45° $\pm \varphi$ R to the right of 0° of azimuth, φ being computed from the formula

$$\tan \varphi = \frac{\cos (i + i')}{\cos (i - i')}$$

There is a deviation at the incidence of 68° and 80° of some amount, but still it is scarcely without the limits of the errors of observations when common light is used. In strong lights the coincidence will doubtless be more perfect.

The best method of determining the position of the major axis, is to place the second plate at such an angle to the ray received from the first, that it may exceed by two or three degrees the angle of restoration in azim. 0°. Hence if we turn the second plate round the ray into all azimuths from 0° to 90° in the right hand quadrant where the greater axis lies, it must come into two azimuths where the restoration takes place at the same incidence. The complements of these two angles of incidence will be equal radii of the ellipse, and consequently the azimuth which bisects the two azimuths in question, will be that of the major axis of the ellipse. By increasing the angle of incidence on the second plate, other two azimuths containing equal radii of the ellipse will in like manner be found; and we might, if necessary, at least obtain an angle of incidence where the two radii coincided with the greater axis.

The position of the ellipse being thus given, we may determine it for all angles of incidence. Calling x the angle of incidence on the first plate, then we shall have four points in the ellipse as follows. The radii in azim. 90° and 270° are always $90° - x$, and the radius in azim. 0° and 180° is the complement of the angle of incidence at which φ in the last equation has the same value as at the angle x. Hence the form of the ellipse is also given.

In these experiments the polarization of the primitive ray has always been + 45°. When this plane varies its position, that of the restored ray also changes, as we have already shown; but it remains to be seen what change takes place in the angles of restoration. At all angles of incidence, a variation in the plane of primitive polarization does not alter the angles of restoration or the corresponding radii of the ellipse in azim. 0°, 90°, 180° and 270°; but at all intermediate azimuths of the second plate the angles of restoration diminish while the primitive plane varies from 45° to 0°, and increase when it varies from 45° to 90°. The following experiments show the progress of the change when the azimuths of the second reflexion are + 45° and — 45°,

STEEL.

Inclination of Plane of prim. Polarization.	Azimuth of Second Reflexion + 45° to Right. Observed.	Calculated.	Azimuth of Second Reflexion — 55° to Left. Observed.	Calculated.
0°	0°	0° 0′	1°	0° 0′
5	2	2 4	2	1 0
10	4	3 46	$3\frac{1}{2}$	2 3
15	6	5 43	$4\frac{1}{2}$	3 7
20	$8\frac{1}{2}$	7 50	6	4 14
25	11	9 54	6	5 26
30	13	12 11	7	6 43
35	15	14 40	8	8 6
40	18	17 25	$9\frac{1}{2}$	9 41
45	$20\frac{1}{2}$	20 30	$11\frac{1}{2}$	11 30

These observations are represented by the formula, $\tan \theta = \tan a \cdot \tan x$; a being the angle of restoration when θ, the inclination of the plane of primitive polarization, is 45°.

I have not given the values of θ from 45° to 90°, because it is difficult to ascertain even in strong lights when the evanescence commences. At 90° the action of the first plate is 0, so that at this limit the angle of restoration is the angle at which the elliptic polarization is no longer visible, from the smallness of the angle of incidence, an angle which varies with the intensity of the light employed

Hitherto we have attended only to the phenomena produced by two similar metals. When the metals are dissimilar, the one silver and the other steel, I found that at the mean maximum polarizing angle of 74°, the inclination of the plane of the restored ray was 28° 30′. But $28° 24' = \frac{39° 48' + 17°}{2}$, so that the inclination is an arithmetical mean between that of silver and that of steel. By four reflexions at 74° the inclination was reduced to 14°, while by four reflexions at about 83° and 58°

the inclination was $21\frac{1}{2}°$, nearly equal to $\frac{28°30'+14°}{2}$, according to the formula in page 297. By thus combining dissimilar metals we may produce elliptic polarization of all degrees of intensity intermediate between those produced by similar metals.

As the circular polarization of total reflexion is the limiting case of elliptical polarization, it becomes important to establish by experiment their intimate connection and almost perfect similarity. Upon combining metallic and total reflexions this was at once evident; and I found in general that circular polarization of any intensity, as produced by either one or more reflexions from glass, may always be restored to rectilineal polarization by one or more metallic reflexions, provided the latter are all made at angles less than the maximum polarizing angle, and that the two classes of reflexions are performed in coincident planes.

As this takes place throughout the whole range of total reflexion from 41° to 90°, it follows that total differs from metallic reflexion in its having two opposite kinds of circular polarization, like the two opposite kinds of elliptical polarization which take place on each side of the maximum polarizing angle of metals. But notwithstanding this, the circular like the elliptic polarization has a maximum at about 50°, declining rapidly to zero at 41°, and on the other side slowly to zero at 90° of incidence.

When one reflexion from steel was combined with two total reflexions from glass at $54\frac{1}{2}°$, the inclination of the plane of the restored ray was $30\frac{1}{2}°$, an arithmetical mean between 45° that of total reflexion, and 17° that of steel, for $\frac{45° + 17°}{2} = 31°$. With silver the inclination was $42\frac{1}{2}°$, and $\frac{45° + 39°\ 48'}{2} = 42°\ 24'$.

If we make the metallic reflector receive the circularly polarized ray in every azimuth, we shall find that in azimuth 90° the circular polarization is compensated by a metallic reflexion above 80°. As the azimuth diminishes to 0°, this angle of compensation diminishes also, passes through 75° in the case of steel, and diminishes to a number depending on the angle of incidence at which the total reflexion is made. We are thus enabled to study the phenomena of circular polarization by the aid of metals, and to obtain results at which it would be exceedingly difficult, if not impossible, to arrive by any other method. This subject, however, presents too wide a field to be treated thus incidentally.

SECT. III.—*On the complementary colours produced by successive reflexions from the polished surfaces of metals.*

I have already given a general account of the phenomena of colour produced by successive reflexions; and I have shown that the tints thus produced are by no means the same as those of crystallized plates, as they do not rise in the scale by successive reflexions.

In my early experiments on total and metallic reflexions, I regarded the two classes of phenomena as exactly the same, mutatis mutandis; and in communicating these results to Dr Young, I pointed out their coincidence with his theoretical views. Dr Young noticed these experiments in the following manner.*

"Dr Brewster has also shown that the total reflexion of light within a denser medium, and the brilliant reflexion at the surfaces of some of the metals, are capable of exhibiting some of the appearances of colour as if the light concerned were divided into two portions, the one partially reflected in the first instance, the other beginning to be refracted, and caused to return by the continued operation of the same power. The original interval appears to be extremely minute, but is capable of being increased by a repetition of similar reflexions as well as obliquity of incidence."

In a letter which I received from this eminent philosopher, dated March 25th, 1816, he thus modifies an objection which he had previously made to my opinion, that the phenomena were owing to the interference of the light which had entered the surface with that which had suffered partial reflexion.

"The light which you suppose to have entered a little way into a reflecting surface, in the case of total reflexion, is singularly circumstanced with regard to the objection I mentioned in my last letter. I did not like the idea of supposing a

* Art. CHROMATICS, *Supp. Encyc. Brit.* p. 157.

surface of any kind to contain a finite space; but, in fact, if your theory should be confirmed, this objection might be greatly diminished by the consideration, that the thickness of the surface would still be like an infinitesimal of a different order from the interval corresponding to its apparent effect, being the versed sine of a curve of which that small interval is the arc, and possibly in a circle of curvature not very minute."

In continuing my experiments on this subject, I found that the colours of total reflexion did not rise in the scale by successive reflexions; and as they modified the tints of crystallized bodies by adding to, or subtracting from, them a given portion of a tint, I announced in the end of 1816, in the *Journal of the Royal Institution*, that I had discovered "a new species of moveable polarization, in which the complementary tints never rise above the white (the blueish white,) of the first order, by the successive application of the polarizing influence."* I determined, experimentally, the angles at which this tint was successively produced and destroyed, and thus discovered some of the leading properties of total reflexion, before, I believe, M. Fresnel had made any experiments on the subject. It was he, however, who ascertained that this new species of polarization was circular polarization; and it is impossible to speak too highly of the ingenuity and talent which he exhibited in that difficult inquiry.

This view of the phenomena of total reflexion unsettled the opinions which I had entertained respecting the action of metals, and I was thus led to revise and extend the unpublished experiments which I had made on the subject.

In order to ascertain the effect of a single metallic surface, I took a crystallized plate of glass whose central tint was the bluish white of the first order, and positive like sulphate of lime. This tint varied from a quarter of a tint in value down to zero. The primitive ray was polarized + 45°, and the plate of steel was horizontal. This ray was received at an incidence near 90°, and the principal section of the analysing prism was in the plane + 45°, while the length of the plate of glass was fixed perpendicular to the plane + 45°, or to the principal section of the prism, so as to move along with it.

* *Journ. Roy. Inst.* vol. iii. p. 213.

At an incidence of 88° the metallic action destroyed the action of the equivalent crystallized plate when the section of the analysing prism was turned from + 45° to + 38°.

At an incidence of $83\frac{1}{2}$° the same effect was produced when the same section was turned into the plane + $22\frac{1}{2}$.

And at an angle of 75°, viz. the maximum polarizing angle, the compensation took place when the axis of the crystal had moved round 45°.

In like manner, at an angle of 60° the compensation took place when the axis of the crystal was turned round 45° + $22\frac{1}{2}$°, or — $22\frac{1}{2}$°; and,

At an angle of incidence of 40° the compensation was effected when the axis of the crystal had turned round 45° + 37°, or into the plane — 37°. The same results are obtained when the light falls on the metal before it passes through the crystal.

Hence it follows, that at the maximum polarizing angle the effect of the equivalent crystal placed in azimuth 45° to the plane of primitive polarization, is compensated by the action of the metallic surface, while at greater angles of incidence the compensation is effected in azimuths less than 45°; and, at less angles of incidence, in azimuths greater than 45.

When the reflexion from the metal is made in a plane perpendicular to the meridian, the opposite effect is produced.

The angles at which the compensation takes place in the preceding experiments are obviously such, that calling R the angle of rotation of the axis of the crystal, it has always to i the angle of incidence the same relation as in the formula,

$$\tan\,(45° - R) = \frac{\cos\,(i + i')}{\cos\,(i - i')}.$$

Hence we are led to the important conclusion, that the pencil which enters the metal follows the changes of polarization of the partially reflected pencil, which is regulated by the same law as in transparent bodies.

It now became interesting to examine the effect produced by the joint action of the metal, and an equivalent crystal, in changing the plane of polarization of the restored ray. The following are the results with different metals at the maximum polarizing angle.

Metals.	Position of the Plane of Polarization.	Rotation effected.
Silver (pure)	+ 42°	3°
Copper	+ 36½	8½
Mercury	+ 35	10
Platina	+ 34	11
Speculum metal	+ 32	13
Steel	+ 30½	14½
Lead	+ 26	19
Galæna	+ 17½	27½

These metals follow the same order in their action upon the plane of polarization that they hold in the table in page 144, though in reference to the rotation actually produced in both cases the order is inverted.

The preceding table points out in a very instructive manner the difference between the action of a metallic surface and an equivalent crystallized film. When two metallic surfaces act together, the plane of polarization of the restored ray is invariably thrown beyond the plane of reflexion; whereas in the combination of a crystallized film with a metallic surface, the same plane never reaches the plane of reflexion, the plane having always a negative position in the former case, and a positive one in the latter. Thus in two reflexions from silver at 73°, the primitive ray polarized + 45° has its plane of polarization changed into — 39° 48′, whereas in the combination of one reflexion from silver with the crystallized film, the plane is changed only into + 42°

In order to determine the law of the metallic action at different incidences and with different numbers of reflexions, I interposed between the eye and the metal, which was silver, a plate of calcareous spar, which exhibited its uniaxal system of rings.

The influence of the metal in modifying the rings was a maximum at 73°, exactly as if they had been crossed by a positive crystalline film which polarized a quarter of a tint, or the pale blueish white of the first order, and whose axes was situated in a plane + 45°, or that which bisects the planes of the two pencils oppositely polarized by the metal. The influence of the metal, or the tint which it polarizes, diminishes gradually from 73° to 90°, where it vanishes, and consequently where the rings recover their symmetry and their tints. At this limit the position of the axis of the equivalent film is a line still bisecting the planes of the two oppositely polarized pencils. At incidences from 73° to 0° the opposite effect takes place, the rings recovering their symmetry at 0°, and the position of the axis of the equivalent film being now vertical, and bisecting the planes of the two oppositely polarized pencils.

At all intermediate angles of incidence the axis has intermediate positions; and calling A the inclination of the axis to the plane of reflexion, we shall have $A = \varphi + 45°$,

φ being positive or + from 90° to 73°, and negative or — from 73° to 0°.

The intensity of the metallic tint, so to speak, or of the positive equivalent plate T, will be

$$T = \tfrac{1}{4}\frac{P}{90} = \frac{2R}{360} = \left(\frac{45° - \varphi}{180}\right).$$

Hence we see the error of the proposition hitherto maintained, that an increase of incidence, reckoning from the perpendicular, produces the same effect as an increase of thickness in thin crystallized plates.

When the rings are combined with two reflexions at 73° in silver, or 75° in steel, they do not suffer the slightest change, the principal section of the prism being placed in the plane — 39° 48′ with silver, and —17° with steel. By two reflexions, however, between 73° or 75° and 90°, an effect is produced on the rings which increases gradually in silver from 73° to 82° 30′, and diminishes from 82° 30′ to 90°. At 82° 30′ the effect is the same as after a single reflexion at 73°; for since four reflexions at 82° 30′ restore the elliptically polarized ray, two reflexions at the same angle must have produced complete elliptical polarization. At angles between 82° 30′ and 90° the pencil is only partially polarized elliptically; whereas from 82° 30′ to 73° the light has been more than elliptically polarized, the restoration of it having been begun during the second reflexion. Hence, in order to determine the phase for any angle between 82° 30′ and 90°, we must take the sum of the phases for each reflexion, or 2 P; whereas between 82° 30′

and 73° we must take the excess of the sum of the two phases above 90° or 90° — 2 P. In both cases the pencil has suffered a partial elliptic polarization;—in the former, from the sum of the actions of the two reflexions, and in the latter, from their unbalanced actions. The very same effects take place between 73° and 57° 16′, the other maximum, as between 73 and $82\frac{1}{2}$°; and between 57° 16′ and 90°, as between $82\frac{1}{2}$ and 90°.

In the case of three reflexions there are two points or nodes of restoration, viz. 78° 8′ and 66° 35′, the maximum being at 85° 6′, 73°, and 48° 38′, at each of which points the phase is 90°. At 73° the second reflexion restores the ray elliptically polarized by the first reflexion, and the third reflexion again produces elliptic polarization. At 85° 6′ and 48° 38′, six reflexions produce a restoration of the pencil, and consequently three reflexions must have polarized the pencil elliptically with a phase of 90°. From 85° 6′ to 90°, and from 48° 38′ to 0°, the pencil has been only partially elliptically polarized, and the phase at any angle between these will be 3 P. At any angle between 48° 38′ and 85° 6′, the phase will be $\overline{2 \times 90} - 3$ P.

In general, calling n the number of reflexions, the phase between 90° and the nearest maximum, and between 0° and the nearest maximum, will be n P, while at all other angles of incidence it will be $(\overline{n - 1} \times 90) - n$ P.

(To be concluded in next Number.)

ART. XV.—*Account of a remarkable Water-spout accompanied with a Luminous Meteor.* By M. GROSMANN.

AFTER a drought which lasted here for many weeks, we were refreshed by an agreeable rain which fell on the 16th of June. It continued at intervals on the 17th and 18th. From the 20th to the 24th, the thermometer had risen by a constant N. E wind to 19—25 (Réaumur.) Though on the evening of the 24th the barometer being as high as 27 inches 9.1 lines a slight storm of rain greatly cooled the air, it had again becomevery hot on 25th, both before and after a shower had fallen at eleven o'clock in the morning. The ground was therefore, so to speak, on fire. The barometer fell to 27 inches, 7.8 lines About two o'clock in the afternoon, one mile above Treves to the E. N. E. of Ruwer and of Pfalzel, at about 20° above the horizon, a phenomenon took place which struck with astonishment, and for half an hour caused a deal of uneasiness in, a number of men who were occupied out of doors.

The sky after the rain was over was still cloudy, when all at once, from the middle of a black cloud which rose from the E. N. E., a luminous mass began to move in a contrary direction, and to tear itself away violently. The cloud soon took the shape of a chimney towards the top, from which escaped a whitish gray smoke, mixed here and there with jets of flame, and raising itself by several apertures with much force, as several witnesses testified, as if it had been driven with celerity by many blasts.

The meteor had arrived above the Vines of Disburg, and opposite Ruwer, when at some distance more to the S. upon the right bank of the Moselle, quite in contact with the ground, a new meteor as it seemed to be to many individuals, appeared in a terrific manner. It dispersed some masses of coals which were heaped round a tree, overthrew a man who was working at a lime-kiln, and threw itself across the Moselle with formidable noise, as if a great many stones were crashing against each other The water rose in a lofty column.

Rolling along on the ground with the same noise, this last meteor went to the Moselle crossing the fields of Pfalzel, leaving evident traces of its route in zigzag lines across the fields of corn and pulse. Parts of the pulse was entirely destroyed, another part was laid and damaged, and the rest raised and carried far away by the wind.

Several women, near whom the meteor passed, fainted, others fled screaming and hid themselves; all the fields seemed on fire. Two labourers who were mounted on a tree observed the meteor in its whole progress. Another had the courage to follow it, and this was easily done at an ordinary foot pace.

But in one of its zigzags which it described the meteor suddenly surrounded him. He felt sometimes drawn forward, sometimes violently lifted up. He lay down, resting himself firmly on the ground with his tools, but he was still thrown over. The whirlwind then left him and continued its course. He does not remember any particular effect either upon his organs of taste or of smell, but there was a deafening noise. He affirms that it had two currents, one of which raised itself obliquely, carrying with it straws and other light bodies; the other took an opposite direction. The route which the meteor had traced in crossing the fields, was, according to different accounts, from 10 to 18 paces wide by 2500 long. Its form was nearly conical; its colour was sometimes whitish grey or yellow, sometimes dull brown, but more frequently the colour of fire. The first meteor was in the air above this one, nearly parallel in advance towards the N. It presented during about eighteen minutes, a great mass of greyish white, which seemed often to vomit the red smoke of flame; and which, when seen at the distance of about half a mile, was in the form of a serpent 140 feet long, the head of which was towards the N. N. E.; and the tail in the opposite direction.

In eight or ten minutes, the tail had suffered a change in descending. At the instant when it was near touching the head, the whole phenomenon disappeared; and at the same time also the inferior meteor, without, as we are assured by an eye witness, the slightest explosion. But a smell of sulphur, very offensive, distributed itself over the whole country. Almost immediately after, a storm burst upon the woods situated to the N. N. W. of the place where the meteor appeared, and it was attended by a hail shower, the stones of which were of immense magnitude.

The sun did not appear all this while, and there was no wind.

The large meteor was seen at Gutweiler, Cassel, and other places, and also at Treves. It appears to have descended from the heights of *Hochwald*—*Schweigger's Jahrbuch der Chimie, &c.*

TREVES, *June 30th* 1830.

ART. XVI.—*An account of a remarkable accident which occurred in a mine of Bovey Coal, in consequence of the compression of the Air.* By the Inspector, Dr PROFESSOR NÖGGERATH.*

In order to understand correctly the following account of the accident which occurred at the pit of Turnick in the territory of Cologne, it is necessary to premise a few words concerning the subterraneous position of the coal pits, and the manner of working them which for some time has prevailed in this region. The coal business is of great importance in this district, and several hundred workmen are employed in it.

Some pretty high ridges of hills, which arise in the basaltic Godesberg, extend more than half a league from Bonn on the Rhine, to the region of Bergheim on the road from Cologne towards Aachen, where they end in a plain, constituting the main locality of the ternary formation of Bovey coal. This coal is commonly covered with layers of clay not very compact; and over the clay to the surface of the ground, is a deposit of coarse sand and rubbish, washed from the hills. Where this covering is thick and strong, in order to obtain the coal to advantage, the mining is performed by a process, or rather a particular kind of excavation, which in our region is termed *tumelbau.* The parts of the tumelbau are shafts, passages, and arched cavities. The whole mine is commonly sunk to one base, namely, just above the level where water would naturally stand. But when, by its natural situation, or by artificial draining, the mine is carried very deep, and the stratum of coal is thick and extensive, a second tumelbau is made under the other, after the former has become exhausted.

In the mine there are two apertures, one of them for the passage to and from it, and the other, called the wind shaft, for ventilation. These two shafts at the bottom are united by a passage, connecting them at right angles. Upon this passage the mining is begun by forming a *tummel*, which is arched above like a bee-hive, and its bottom is continued on the level of the passage between the two shafts. A tummel is commonly from three to six fathoms in diameter, and from two to five in height. The coal

* *Jahrbuch der Chem. und Phys.*

itself serves to support the highest part of the tummel, because, by the pressure of its sides it settles of itself to a certain extent, the excavation still preserving its arched form. When the tummel is carried so high as to reach the top of the layer of coal, or the level of the tummel is itself reached by a second mine that has been sunk under it, by degrees it commonly breaks and becomes filled, by which means, the walls or parts subject to the pressure, are again brought together and consolidated. By the filling up of the tummel, many funnel-shaped apertures on the surface are formed. A second or third tummel can at any time be formed upon a passage by removing the surrounding coal, or by making a new excavation; and in the present instance, upon the right and left of the main excavation, and at the shafts, new passages had been formed, and new tummels had appeared, so that the mine was continually enlarging, and receding farther backwards. Finally, near the wind shaft, and likewise by the main tummel, pillars were erected to make the works as strong as possible. A new tummel had been sunk below the passage between the two shafts, and the shafts themselves were continued down to the level of its base, and a passage formed for connecting them. The old tummel did not fill again as is common after a new one is formed below, because from one-third to one-fifth of the superincumbent coal had been left for support, in consequence of the extreme caution of the workmen. But it was difficult to secure it altogether, as the coal was very light in some places, and the timber was small as a matter of economy, in consequence of the moderate profits of the mine.

The accident occurred in the tummelbau of Botterbroicher, Therchengrube, near Turnick, Feb. 7, 1826. There were in the mine, 1st, John Weber, contractor, who was killed; 2d, Martin Pohl, coal cutter, who had his sight slightly injured, besides a superficial bruise of the thigh; 3d, Jacob Brewer, coal porter, who had a shoulder dislocated, and a severe contusion of the left thigh. Without the mine, though near a shaft, were in waiting two windlass turners, John Bieck and Hilger Zimmermann.

At 7 o'clock, A. M. as the workmen were going into the mine where they had worked the day before, a pressure was perceived, and in apprehension of the falling in of the tummel, they took the precaution of employing themselves upon the coal near its entrance. About 9 o'clock, the pressure had sensibly increased, and some large pieces of the clayey stratum of the arch fell in. Weber the contractor heard the crash, and ran to those who were in the pit. He had the tummel searched externally and internally, and thought it not proper for the workmen to go immediately to their business, but that they should first eat their breakfast, and if in the meantime, there should be another break, they might quit their work. Upon this, Weber set out to brace the pit, the workmen remaining seated at their breakfast in the wind shaft, near the passage to the tummel. Weber had been gone but a few moments, and had not probably reached the middle of the ascent of the shaft, when the tummel suddenly fell in, and the rush of the air was so terrible that Pohl, who was five fathoms within the wind-shaft, and Brewer, who was in another passage, were thrown down. Pohl recovered himself as he was lying upon the wind-shaft; and upon regaining his senses, looked after his companion, whom he found senseless in the main passage. After Brewer had recovered so as to come to his reason, they found Weber the contractor near them, apparently lifeless. Upon calling, Bieck came to their assistance. Brewer was drawn out by means of a rope, and the body of Weber was brought out in the same manner. Pohl was able to climb out, without assistance.

Bieck and Zimmermann state, that after Weber had left them, Bieck called and inquired whether they should take their breakfast within the shaft? After receiving an answer, they went about ten paces from the mouth of the shaft to the hut, to eat their breakfast. In less than a quarter of an hour an alarming noise was heard. The brick roof of the hut, that was built over the entrance of the mine, was blown into atoms, and an entire ladder was thrown out of the shaft, which fell upon the hut of the other shaft, that still remained standing. The hats of those who were within the mine were found at the distance of twenty paces without from the mouth of the shaft, as also two iron hooks with which the ladder had been fastened, were picked up near the shaft, one of them broken short, the other torn out of the timber and twisted. Upon examining the shaft, it was found, that by the powerful dislodgement of the ladder, the timber of the shaft had been much damaged.

This is a very strange and singular accident of its kind, which could arise only from the violent compression of the air in the cavity of the tummel, at the moment when it was filled by the simultaneous fall of the materials of the arch. The great strength of the strata, combined with the circumstances, that there was, directly over the pit, a very firm clayey roof, was the cause of there being given to the tummel (though it was very ill judged,) uncommonly large dimensions; for by the report of the coal-cutters, it was twelve fathoms in diameter, and four and a half in height. To increase the force of the rush of air, another circumstance greatly contributed. In lately fitting up the mine, two apertures only were retained, the wind-shaft and the passage for conveyance. As the tummel fell, the whole mass of air was forced out of one passage only, the pressure being directed to this, because during the cold weather the wind-shaft had been stopped at its mouth.

When we consider the amazing force with which the ladder was carried out of the shaft, and the other circumstances testified by the workmen, it is most probable that Weber, who was on the ladder as the tummel fell in, and wore a long linen frock, was lifted up by it so high, that the distance of the fall caused his death.

The medical examination of the corpse of Weber, discovered numerous fractures. The fourth, fifth, and sixth ribs of the left side, and the heads of others were broken, and driven into the cavity of the chest. The pericardium on the left side from above downwards was ruptured, as was also the right cavity of the breast. The left lobe of the lungs exhibited several lacerations, and was crushed into a confused mass.

Art. XVII.—ANALYSIS OF SCIENTIFIC BOOKS AND MEMOIRS.

Principles of Geology, being an attempt to explain the former changes of the Earth's Surface by reference to causes now in operation. By Charles Lyell, Esq. F. R. S. Foreign Secretary to the Geological Society. In two volumes. Vol. I. London, 1830. Pp. xv. and 511.—Continued from Vol. III. p. 349.

Before proceeding to quote from the remaining portion of Mr Lyell's volume, we propose to dwell for a moment on his theory of the changes of temperature in the course of the great geological period, which we had just noticed in last number. He proves, as may easily be done, the influence of land, especially high land, in modifying the mean temperature of the globe, according as it is in higher or lower latitudes than the isothermal line representing that mean temperature. He then proposes the elevation of land in the arctic regions, since the earliest epochs of geological records as a fit explanation of the admitted refrigeration of the crust of the earth; but in applying this theory to observed facts, his inductions appear not so legitimate, nor is a sufficiently precise picture drawn of the testimony of the strata. Our first objection is this; while Mr Lyell admits the efficiency of intertropical land in ameliorating the climate, * as well as of arctic land, in deteriorating it, in giving full weight to every indication he can obtain of the actual effect of the latter, he sinks in total neglect, operations, perhaps analogous in nature, though precisely the reverse in effect which have occurred in the former. "Our information," he says, "is at present limited to latitudes north of the tropic of Cancer, and we can only hope, therefore, to point out that the condition of the earth, so far as relates to our temperate and arctic zones, was such, as the theory before offered, would have led us to anticipate." † It will not do to say, that, in accordance with the "uniformity" system, we are to suppose the ratio of land and water on the globe a constant quantity; that would be still more illogical than the supposed conjecture of the New Zealander, which Mr Lyell ridicules, that there is no more land in the northern than the southern hemisphere; besides the chances are, that the exchange of land would have taken place from the antarctic to the arctic regions, and not from the equator. While, therefore, Mr Lyell is boldly drawing his conclusions from appearances in higher latitudes, his argument is wholly inept, unless he can at the same time prove that changes similar to those he describes were not going on in intertropical regions, for were they doing so, as by all analogy we may suppose, the two results being opposed, their effect would be null or trifling. It therefore appears to us, that, by pointing out to the reader what alone served his theory, and by making him neglect under the plea of want

* P. 117. † P. 126.

of information, what was equally essential to its validity, the author has commenced his argument with a total flaw in the premises.

But farther, the assertion is far too vague, that a "glance at the best geological maps now constructed of the various countries in the northern hemisphere, whether in North America or Europe, will satisfy the inquirer that the greater part of the land has been raised from the deep, either between the period of the deposition of the chalk and that of the strata termed tertiary, or at subsequent periods, during which the various tertiary groups were formed in succession. For, as the secondary rocks from the lias to the chalk inclusive, are, with a few unimportant exceptions, marine, it follows, that every district now occupied by them has been converted into land since they originated." P. 134, 135.

Now in this argument, Mr Lyell seems to pass over the important fact, that it is only land elevated in a higher latitude, than the parallel representing the mean temperature, (not of the *surface* of the globe but) of the quadrantal arc of the meridian, which could by his own theory deteriorate the climate. It is therefore most of all in the extreme north of the continents and in the polar regions that we must look for those modern rocks which Mr Lyell considers the test of recent elevation. Under this point of view, the fact, we believe, is calculated to produce different results. We do not pretend to anything like accurate knowledge of the formations of the higher latitudes, but it is strikingly remarkable that in the voyages of Parry and other modern navigators, rocks later than the coal formation are almost wholly awanting, except in some parts of West Greenland. This may indeed be owing to the disappearance by natural causes of extended continents now broken into islands, as some have conjectured, which have retained only their more consolidated formations. But were this admitted, the supposition would militate, as is obvious, in another way against our author's supposition.

In conclusion of this subject, we would at least confidently say, that Mr Lyell was bound to have entered more into the detail of facts upon which so important a principle as he has proposed could alone be established. It is by no means wanting in ingenuity and plausibility, but we think it must strike every one as being rather an induction from the hypothesis of "absolute uniformity," which it is calculated to support, than as flowing directly from the facts which are intended to form its basis.

Chapter ninth contains Mr Lyell's views on the nature of the evidence afforded by fossil remains, considered as establishing a progressive developement of perfection in the organic structure of the objects of physiology, which have existed contemporaneously with a terrestrial surface immediately below the strata in which they occur. Mr Lyell denies this opinion. His arguments are too briefly and inconclusively stated; his love of magnifying exceptions rather than arguing from general laws is here again exemplified. Without hazarding an opinion on the very interesting question agitated, a question entirely of facts, we will only say that Mr Lyell's view of it appears incomplete and unsatisfactory, and that, at all events, it ought certainly to have been postponed to another portion of the work.

Chapters tenth to seventeenth inclusive, contain an elaborate and interesting detail of the proofs of the great existing energy of water, as a modifying, disintegrating, and transporting agent. As our author has in general confined himself to modern and well authenticated events, the subject becomes very interesting, and the details mark much care and industry in their collection. The transporting effects of running water, of springs, of the ocean, the formation of marine and lacustrine deltas and bars are successively considered. We shall make some extracts from this part of the work, and commence with a notice of the great flood at Tivoli in 1826, of which no account has appeared in this Journal.

"*Flood at Tivoli*, 1826.—We shall conclude with one more example, derived from a land of classic recollections, the ancient Tibur, and which, like all the other inundations to which we have alluded, occurred within the present century. The younger Pliny, it will be remembered, describes a flood on the Anio, which destroyed woods, rocks, and houses, with the most sumptuous villas and works of art. For four or five centuries consecutively, this headlong stream, as Horace truly called it, has often remained within its bounds, and then, after such long intervals of rest, at different periods inundated its banks again, and widened its channel. The last of these catastrophes happened 15th Nov. 1826, after heavy rains, such as produced the floods before alluded to in Scotland. The waters appear also to have been impeded by an artificial dike, by which they were separated into two parts, a short distance above Tivoli. They broke through this dike, and, leaving the left trench dry, precipitated themselves with their whole weight, on the right side. Here they undermined, in the course of a few hours, a high cliff, and widened the river's channel about fifteen paces. On this height stood the church of St. Lucia, and about thirty-six houses of the town of Tivoli, which were all carried away, presenting, as they sank into the roaring flood, a terrific scene of destruction to the spectators on the opposite bank. As the foundations were gradually removed, each building, some of them edifices of considerable height, was first traversed with numerous rents, which soon widened into large fissures, until at length the roofs fell in with a crash, and then the walls sank into the river, and were hurled down the cataract below.

"The destroying agency of the flood came within two hundred yards of the precipice on which the beautiful temple of Vesta stands; but fortunately this precious relic of antiquity was spared, while the wreck of modern structures was hurled down the abyss. Vesta, it will be remembered, in the heathen mythology, personified the stability of the earth; and when the Samian astronomer, Aristarchus, first taught that the earth revolved on its axis, and round the sun, he was publicly accused of impiety, 'for moving the everlasting Vesta from her place.' Playfair observed, that when Hutton ascribed instability to the earth's surface, and represented the continents which we inhabit as the theatre of incessant change and movement, his antagonists, who regarded them as unalterable, assailed him, in a similar manner, with accusations founded on religious prejudices. We might appeal to the excavating power of the Anio as corroborative of one of the most controverted parts of the Huttonian theory; and if the days of omens had not gone by, the geologists who now worship Vesta might regard the late catastrophe as portentous. We may, at least, recommend the modern votaries of the goddess to lose no time in making a pilgrimage to her shrine, for the next flood may not respect the temple." Pp. 196, 197.

We have already remarked, that in the formation of deltas, Mr Lyell sees the embryos of new continents, and considers the strata of calcareous and siliceous matter, deposited by the rivers which form them, as real rock formations similar to those we now find constituting the whole crust of the globe. Here, as usual, however, Mr Lyell quotes only the examples favourable to his hypothesis, overthrows the doctrine of universal formations with a stroke of his pen, and puts in the back ground all the difficult rocks to which his theory could never apply. The indurated formations of the delta of the Rhone are an interesting illustration of his views.

" That a great proportion, at least, of the new deposit in the delta of the Rhone consists of *rock*, and not of loose incoherent matter, is perfectly ascertained. In the museum at Montpellier is a cannon taken up from the sea near the mouth of the river, imbedded in a crystalline calcareous rock. Large masses, also, are continually taken up of an arenaceous rock, cemented by calcareous matter, including multitudes of broken shells of recent species. The observations recently made on this subject corroborate the former statement of Marsilli, that the earthy deposits of the coast of Languedoc form a stony substance, for which reason he ascribed a certain bituminous, saline, and glutinous nature, to the substances brought down with sand by the Rhone. If the number of mineral springs charged with carbonate of lime which fall into the Rhone and its feeders in different parts of France be considered, we shall feel no surprise at the lapidification of the newly-deposited sediment in this delta. It should be remembered, that the fresh-water introduced by rivers, being lighter than the water of the sea, floats over the latter, and remains upon the surface for a considerable distance. Consequently, it is exposed to as much evaporation as the waters of a lake; and the area over which the river-water is spread, at the junction of great rivers and the sea, may well be compared, in point of extent, to that of considerable lakes. Now, it is well known, that so great is the quantity of water carried off by evaporation in some lakes, that it is nearly equal to the water flowing in; and in some inland seas, as the Caspian, it is quite equal. We may, therefore, well suppose that, in cases where a strong current does not interfere, the greater portion not only of the matter held mechanically in suspension, but of that also which is in chemical solution, must be precipitated within the limits of the delta. When these finer ingredients are extremely small in quantity, they may only suffice to supply crustaceous animals, corals, and marine plants, with the earthy particles necessary for their secretions; but whenever it is in excess (as generally happens if the basin of a river lie partly in a district of active or extinct volcanos), then will solid deposits be formed, and the shells will at once be included in a rocky mass." Pp. 234, 235.

He afterwards says, " that the matter carried by rivers into seas and lakes is not thrown in confused and promiscuous heaps, but is spread out far and wide along the bottom, is well ascertained; and that it must for the most part be divided into distinct strata, may in part be inferred where it cannot be proved by observation. The horizontal arrangement of the strata, when laid open to the depth of twenty or thirty feet in the delta of the Ganges and in that of the Mississippi, is alluded to by many writers; and the same disposition is well known to obtain in all modern deposits of lakes and estuaries. Natural divisions are often occasioned by the interval of time which separates annually the deposition of matter during the periodical rains, or melting of the snow upon the mountains. The deposit of each year acquires some degree of consistency before that of the succeeding year is superimposed. A variety of circumstances also give rise annually to slight variations in colour, fineness of the particles, and other characters. Alternations of strata distinct in texture, mineral ingredients, or organic contents, are produced by numerous causes. Thus, for example, at one period of the year, drift wood may be carried down, and at another mud, as was before stated to be the case in the delta of the Mississippi; or at one time when the volume and velocity of the stream are greatest, pebbles and sand may be spread over a certain area, over which, when the waters are low, fine matter or chemical precipitates are formed. During inundations the current of fresh-water often repels the sea for many miles; but when the river is low, salt-water again occupies the same space. When two deltas are converging, the intermediate space is often, for reasons before explained, alternately the receptacle of different sediment derived from the converging streams. The one is, perhaps, charged with calcareous, the other with argillaceous matter; or one may sweep down sand and pebbles, the other impalpable mud. These differences may be repeated with considerable regularity, until a thickness of hundreds of feet of alternating beds is accumulated." Pp. 253, 254.

The details of the destroying agency of the ocean, particularly on the east coast of England, are very curious, and in chapter fifteenth, are some interesting particulars accompanied with several good wood cuts of rocks in Shetland, extracted from Dr Hibbert's elaborate work. But we have not room for farther extracts from this part of the volume.

Chapters eighteenth to twenty-second contain an interesting and popular account of the effects of modern volcanos, though we do not observe much of profound or original remark. The first of these chapters discusses the general distribution of volcanic vents, and points out their remarkable continuity and connexion with the scenes of earthquakes. We have to make a short extract on the distinction of ancient and modern eruptions.

" We must also be careful to distinguish between lines of extinct and active volcanos, even where they appear to run in the same direction, for ancient and modern systems may cross and interfere with each other. Already, indeed, we have proof that this is the case; so that it is not by geographical position, but by reference to the species of organic beings alone, whether aquatic or terrestrial, whose remains occur in beds interstratified with lavas, that we can clearly distinguish the relative age of volcanos of which no eruptions are recorded. Had Southern Italy been known to civilized nations for as short a period as America, we should have had no record of eruptions in Ischia; yet we might have assured ourselves that the lavas of that isle had flowed since the Mediterranean was inhabited by the species of testacea now living in the Neapolitan seas. With this assurance it would not have been rash to include the numerous vents of that isle in the modern volcanic group of Campania. On similar grounds we may class, without much hesitation, the submarine lavas of the Val di Noto in Sicily, in the modern circle of subterranean commotion, of which Etna and Calabria form a part. But the lavas of the Euganean hills and the Vicentin, although not wholly beyond the range of earthquakes in Northern Italy, must not be confounded with any existing volcanic system; for when they flowed, the seas were inhabitated with animals entirely distinct from those now known to live, whether in the Mediterranean or other parts of the globe. But we cannot enter into a full development of our views on these subjects in the present volume, as they would carry us into the consideration of changes on the earth's surface far anterior to the times of history, to which our present examination is exclusively confined."—P. 235.

Chapters eighteenth and nineteenth contain an interesting synopsis of the volcanic phenomena of the bay of Naples; we shall not enter into these details, but, chiefly as a specimen of the author's style, we shall give his concluding paragraphs on the Buried Cities, eloquently showing the errors into which future geologists might be led, similar to those which Mr Lyell considers to warp the views of many of the present day.

"Yet favoured as this region has been by Nature from time immemorial, the signs of the changes imprinted on it during the period that it has served as the habitation of man, may appear in after-ages to indicate a series of unparalleled disasters. Let us suppose that at some future time the Mediterranean should form a gulph of the great ocean, and that the tidal current should encroach on the shores of Campania, as it now advances upon the eastern coast of England: the geologist will then behold the towns already buried, and many more which will inevitably be entombed hereafter, laid open in the the steep cliffs, where he will discover streets superimposed above each other, with thick intervening strata of tuff or lava—some unscathed by fire, like those of Herculaneum and Pompeii, others half melted down like these of Torre del Greco, or shattered and thrown about in strange confusion like Tripergola. Among the ruins will be seen skeletons of men, and impressions of the human form stamped in solid rocks of tuff. Nor will the signs of earthquakes be wanting. The pavement of part of the Domitian Way, and the Temple of Nymphs, submerged at high tide, will be uncovered at low water, the columns remaining erect and uninjured; while other temples which had once sunk down, like that of Serapis, will be found to have been upraised again by subsequent movements. If they who study these phenomena, and speculate on their causes, assume that there are periods when the laws of Nature differed from those established in their own time, they will scarcely hesitate to refer the wonderful monuments in question to those primeval ages. When they consider the numerous proofs of reiterated catastrophes to which the region was subject, they may perhaps commiserate the unhappy fate of beings condemned to inhabit a planet during its nascent and chaotic state, and feel grateful that their favoured race escaped such scenes of anarchy and misrule.

"Yet what was the real condition of Campania during those years of dire convulsion? 'A climate where heaven's breath smells sweet and wooingly—a vigorous and luxuriant nature unparalleled in its productions—a coast which was once the fairy land of poets, and the favourite retreat of great men. Even the tyrants of the creation loved this alluring region, spared it, adorned it, lived in it, died in it.'* The inhabitants, indeed, have enjoyed no immunity from the calamities which are the lot of mankind; but the principal evils which they have suffered must be attributed to moral, not to physical causes—to disastrous events over which man might have exercised a control, rather than to the inevitable catastrophes which result from subterranean agency. When Spartacus encamped his army of ten thousand gladiators in the old extinct crater of Vesuvius, the volcano was more justly a subject of terror to Campania, than it has ever been since the rekindling of its fires."—Pp. 359, 360.

From the succeeding chapter upon Etna, we cannot resist quoting the following singular details.

"A remarkable discovery has lately been made on Etna of a great mass of ice, preserved for many years, perhaps for centuries, from melting, by the singular event of a current of red hot lava having flowed over it. The following are the facts in attestation of a phenomenon which must at first sight appear of so paradoxical a character. The extraordinary heat experienced in the south of Europe during the summer and autumn of 1828, caused the supplies of snow and ice, which had been preserved in the spring of that year for the use of Catania and the adjoining parts of Sicily and the island of Malta, to fail entirely. Considerable distress was felt for the want of a commodity regarded in these countries as one of the necessaries of life rather than an article of luxury, and on the abundance of which in some large cities the salubrity of the water and the general health of the community is said in some degree to depend. The magistrates of Catania applied to Signor M. Gemmellaro, in the hope that his local knowledge of Etna might enable him to point out some crevice or natural grotto on the mountain, where drift snow was still preserved. Nor were they disappointed; for he had long suspected that a small mass of perennial ice at the foot of the highest cone was part of a large and continuous glacier covered by a lava current. Having procured a large body of workmen, he quarried into this ice, and proved the superposition of the lava for several hundred yards, so as completely to satisfy himself that nothing

* *Forsyth's Italy*, vol. ii.

but the subsequent flowing of the lava over the ice could account for the position of the glacier. Unfortunately for the geologist, the ice was so extremely hard, and the excavation so expensive, that there is no probability of the operations being renewed. On the 1st of December 1828, I visited this spot, which is on the south-east side of the cone, and not far above the Casa Inglese, but the fresh snow had already nearly filled up the new opening, so that it had only the appearance of the mouth of a grotto. I do not, however, question the accuracy of the conclusion of Signior Gemmellaro, who being well acquainted with all the appearances of drift snow in the fissures and cavities of Etna, had recognized, even before the late excavations, the peculiarity of the position of the ice in this locality. We may suppose, that, at the commencement of the eruption, a deep mass of drift snow had been covered by volcanic sand showered down upon it before the descent of the lava. A dense stratum of this fine dust mixed with scoriæ is well known to be an excellent non-conductor of heat, and may thus have preserved the snow from complete fusion when the burning flood poured over it. The shepherds in the higher regions of Etna are accustomed to provide an annual store of snow to supply their flocks with water in the summer months, by simply strewing over the snow in the spring a layer of volcanic sand a few inches thick, which effectually prevents the sun from penetrating. When lava had once consolidated over a glacier at the height of ten thousand feet above the level of the sea, we may readily conceive that the ice would endure as long as the snows of Mont Blanc, unless melted by volcanic heat from below. When I visited the great crater in the beginning of winter, (December 1, 1828,) I found the crevices in the interior encrusted with thick ice, and in some cases hot vapours were streaming out between masses of ice and the rugged and steep walls of the crater. After the discovery of Signior Gemmellaro, it would not be surprising to find, in the cones of the Icelandic volcanos, repeated alternations of lava streams and glaciers." Pp. 369-371.

In chapter twenty-second Mr Lyell endeavours to confute the doctrine of craters of elevation, founded by Von Buch and supported by Humboldt. Without entering into the merits of the question, we would suggest that Mr Lyell has perhaps hardly paid sufficient deference, not to the great names, but to great powers and vast experience of these two profound naturalists.

The four concluding chapters of the volume are devoted to some very interesting details respecting earthquakes and their effects, which our author considers of high importance, and adequate to most of the effects of sudden violence and gradual elevation manifested in the strata of the globe. His arguments for the greater magnitude of past effects are founded upon reiterated action similar to that of aqueous agents. He cannot make a mountain 2000 feet high at once, but he does it by 200 shocks at ten feet each. Now this mode of reasoning is less tenable than in the slow and constant degradation by water, because earthquakes are not among the ordinary and calculable forces employed by nature, and to proceed upon the assumption of earthquakes returning at the same spot, with the same species of action (elevating or depressing,) for hundreds of times, seems allowing more to her past than could be said for her present uniformity; beside there are, we suspect, many phenomena of the strata which could only be accounted for by single exertions of nature greater far than any now on record. In his twenty-fifth chapter Mr Lyell has given, in connection with the theory of the elevation and depression of land, an account of the remarkable changes of apparent level in the Bay of Baja, and the testimony of the Temple of Serapis. The facts which bear on this interesting subject are luminously brought together, and the theory deduced is quite identical with that given in a former number of this *Journal*, to which Mr Lyell has liberally referred. A contemporary review of Mr Lyell's work, considers that he has brought forward "an overwhelming mass of evidence in proof of the fact, that this part of the Campanian coast was lowered at least twenty feet some time between the third and the sixteenth century, and re-elevated about as much again at the epoch of the eruption which produced the Monte Nuovo. *The circumstances which demonstrate this are so clearly legible*, that it would never perhaps have been disputed, but for the natural repugnance to admit so remarkable a local coincidence of elevation and depression to nearly the same extent, as well as the strong prejudices existing in regard to the immobility of the land, by which we have probably been blinded to the force of many similar facts."

In the conclusion of his volume, Mr Lyell discusses the relative amount of subsidence and elevation caused by earthquakes, and conceives, upon the grounds of his theory, that the former predominated; since otherwise the accumulations which upon any hypothesis must have been immense from the productions of volcanos, and the transportation of materials to the surface of the globe by springs or other causes, must have sensibly increased the radius of the globe, and an equalizing effect must therefore be admitted by local subsidence, or (as we rather suspect) by some equiparate cause. Now, as we consider this assertion of Mr Lyell one of the best founded in the volume, we must take this oportuity of rescuing it from the very unceremonious and unwarrantable condemnation which it has received in the review just quoted. "This is a problem" it is there said, "which we have no data for solving. Mr Lyell assumes without argument, that the dimensions of the globe are invariable, and then concludes for an excess of subsidence over elevation, in order to compensate the continual production of fresh matter from the interior of the globe in shape of lava, and the deposits of mineralized springs. *But as we consider the assumption unwarrantable, the inference is of course equally so.*" Here there is an entire oversight of the remarkable fact demonstrated first, we believe, by Laplace, that since the time of Hipparchus the duration of the solar day has not varied *one hundredth part of a centesimal second*,* a quantity which corresponds to an increase of the terrestrial radius almost infinitesimal, and which may be shown not to amount to many feet. This extraordinary deduction, demonstrable on the most undeniable principles, points to a wonderful compensation of opposite errors.

We close Mr Lyell's book, as we commenced it, with feelings of real gratification, with a high opinion of his industry and talents of combination and classification, and with a conviction of his possessing original powers, which we would wish to see more drawn upon in the succeeding volume, or, as we hope and expect, *volumes* of his work.

* Laplace, *Systeme du Monde*, 5me, Ed. tome ii. p. 87.

Art. XVII.—SCIENTIFIC INTELLIGCE

I. NATURAL PHILOSOPHY.

METEOROLOGY.

1. *Account of the Georgia Meteor and Ærolite.*—Having recently received from Dr Boykin, specimens of the meteoric stone which fell in Forsyth, in Georgia, in May 1829, we are induced to republish an extract from an original statement of the facts, as it appeared in the newspapers at the time.

"Between three and four o'clock, on the 8th instant, on that day, a small black cloud appeared south from Forsyth, from which two distinct explosions were heard, following in immediate succession, succeeded by a tremendous rumbling or whizzing noise, passing through the air, which lasted from the best account, from two to four minutes.

"This extraordinary noise was, on the same evening, accounted for by Mr Sparks and Captain Postian, who happened to be near some negroes working in a field, one mile south of this place, who discovered a large stone descending through the air, weighing, as was afterwards ascertained, thirty-six pounds.

"The stone was, in the course of the evening, or very early the next morning, recovered from the spot where it fell. It had penetrated the earth two feet and a half. The outside wore the appearance as if it had been in a furnace: it was covered about the thickness of a common knife blade, with a black substance somewhat like lava that had been melted. On breaking the stone, it had a strong sulphureous smell, and exhibited a metallic substance resembling silver.

"The stone, however, when broken, had a white appearance on the inside, with veins. By the application of steel, it would produce fire.

"The facts as related, can be supported by many individuals who heard the explosion and rumbling noise, and saw the stone.—Elias Beall."

The following notice, forwarded to the Editor by Dr Boykin, of Georgia, under date of June 2, 1830, corresponds substantially with the above.

"No one can tell from what direction the meteor came.—The first thing noticed was the report, like that of a large piece of ordnance; some say the principal explosion was succeeded by a number of lesser ones in quick succession, similar to the explosions of a cracker; one has told me the secondary noise was only a reverberation. Very soon after the explosion, some black people heard a whizzing noise, and on looking saw a faint '*smoke*' descend to the ground; at which time they heard the noise produced by the fall of a stone; they ran to the spot, for they saw where it fell, and discovered the hole it had made in the ground, being more than two feet in a hard clay soil: the negroes and others who went early to the spot, say they perceived a sulphureous smell. The stone weighed thirty-six pounds: it fell at a small angle with the horizon."

Having received the specimens, just as this number of the Journal is about being finished, I can only add the following notice: The colour of the interior of the stone is a light ash-gray, and very uniform, except that it is sprinkled throughout with thousands of brilliant points of metallic iron, having very near the colour and lustre of polished silver. The iron is rarely in points larger than a small pin's head, but the points are so numerous that nearly the whole of the powder of the stone is taken up by the magnet, even when it is in fine dust, and by a magnifier the little points of iron can even then be seen standing out from the magnet. It greatly resembles the Tennessee meteoric.

It has the usual black crust on certain parts, and this, though resembling a semi-fused substance, exhibits bright metallic points when a file is drawn across it. A similar black crust is seen pervading the stone in some places through its interior, and forming where it is seen a cross fracture, black lines, or veins. The stone is full of semi-fused black points and ridges similar to the crust, and its entire mass seems half vitrified in points, so as to resemble an imperfect glass. The specific gravity, as ascertained by Mr Shepard, is 3.37.—*American Journal,* No. 38, pp. 388, 389.

2. *Notice of the circumstances attending the fall of the Tennessee Meteorites,* May 9, 1827.—On Wednesday the 9th inst. about 4 o'clock P. M. the day being as clear as usual, my son and servants were planting corn in the field, they heard a report similar to that of a cannon, which was continued in the air resembling the firing of cannon or muskets by platoons, and the beating of drums as in a battle. Some small clouds with a trail of black smoke, made a terrific appearance, and from them, without doubt, came a number of stones with a loud whizzing noise, which struck the earth with a sound like that of a ponderous body. One of these stones my son heard fall about fifty yards from where he was. In its descent to the ground it struck a paupau tree of the size of a small hand spike, and tore it to pieces as lightning would have done; guided by the tree, he immediately found the spot, and there he found the stone about eight or ten inches under the ground; this stone weighed five pounds and a quarter. Mr James Dugge was also present. They stated that the stone was cold but had the scent of sulphur.

On the same day, and about the same time, my son-in-law, Mr Peter Ketsing, was in a field with his labourers, about one mile distant, when a stone fell which weighed eleven pounds and a-half. This took place near him, his wife, and three other women. A number of respectable men were present when it was found and taken up; it was twelve inches under ground. I have seen one that fell at Mr David Garret's, and part of one that fell at Mr John Bones', I have also heard of one more that has been found. These stones are perfectly similar, glazed with a thin black crust, and bear the marks of having been through a body of fire and black smoke. Many gentlemen who have been excited within a few days to come to my house to see them, say they never saw such before.

The editor of the paper says the noise was heard ten or twelve miles or more.

I have nothing to add, says Professor Silliman, to the descriptions of this stone already published, except that the innumerable metallic points which are visible through the light gray (almost white) surface of the mass are nearly as brilliant as silver, although they have obviously been rounded by heat. They are attended by an immense number of brilliant black vitreous globules, which have every appearance of perfect fusion, and the entire mass has that harsh acrid feel which belongs to lavas and trachytic rocks.

The black crust has evidently been in a state of at least pasty fusion; its roughnesses are rounded, and on drawing a file over any of its prominent points, bright metallic iron is immediately uncovered.

There is no account of a fire-ball attending these meteorites, but as it was full day light and probably sunshine, we cannot conclude that there was no fire-ball. It is most probable that there was one.—*American Journal*, No. 38, pp. 378, 379.

II. CHEMISTRY.

3. *Composition of Mellitic Acid.*—Messrs Liebig and Wöhler have analyzed the mellate of silver, and found that when burned with peroxide of copper, it gives off a gas which is absorbed entirely by caustic potash. From their results they deduce the following composition :

		Theory.	Experiment.
4 atoms Carbon,	24	50	50.21
3 do. Oxygen,	24	50	49.79
Atom of mellitic acid, =	48	100	100

The atomic weight ascertained by experiment is 49.5, which comes very near the composition above stated.

4. *Succinic Acid.*—The same chemists have made a new analysis of this acid also. The composition obtained, as above, for mellitic acid, is the same as that obtained by Berzelius for the composition of succinic acid, except that the latter contains also two atoms of hydrogen. By subliming the succinic acid in chlorine, and by passing a current of chlorine through a solution of the acid in water, Liebig and Wöhler attempted to abstract the hydrogen from the succinic, and to convert it into mellitic acid, but without success. The acid they obtained had 50 for its atomic weight, and consisted of 2 hyd. + 3 ox. + 4 car., which indicate pure succinic acid.

5. *Paratartaric Acid.*—Berzelius has given this name to the tartaric acid of the Vosges, long ago observed by John and Gay-Lussac, to differ from common tartaric acid. By the analysis of Berzelius, it proves to have not only the same atomic weight, but also the same atomic constitution, and to contain the same per centage of the several constituents. He calls it therefore *Para*-tartaric, to denote its difference from, and yet its intimate connection with, the common tartaric acid. The crystallized *Para*-tartaric differs from the tartaric, in being of a different form, in requiring five times its weight of water for solution, while tartaric acid dissolves in half its weight, and in containing two atoms of water, one of which is driven off

by a gentle heat. The *Bi*-paratartrate of soda is more soluble than the tartrate, but the same salts of potash are alike difficult of solution, and both form two double salts, with oxide of antimony. One of the double tartrates can be obtained only in the form of a gummy mass. The corresponding paratartrate crystallizes in small needles, becoming opaque and milk-white in the air, from loss of water. But the salts of lime are the most interesting. Both contain four atoms of water, and therefore both have the same per centage of all the constituents. The paratartrate, however, is the less soluble, and from this property is derived the best mode of distinguishing or separating the two acids. If a portion of the tartrate and paratartrate of lime be dissolved, each in a separate vessel, (or in the same,) in muriatic acid slightly diluted, and caustic ammonia added to saturation, the *Paratartrate* speedily falls as a semi-crystalline white opaque precipitate; the *tartrate*, on the other hand, is not thrown down, till the liquid is much concentrated, when, after some time, crystals in square octahedrons begin to be deposited on the sides of the glass.

6. *Salicine.*—This new substance, obtained from the bark of the willow, occurs in prismatic crystals, and is very bitter. 100 parts of water, at the temperature of 19° 5 cent, dissolve 5.6 parts of salicine. Its solubility increases with the temperature, and boiling water will dissolve it in any proportion. It is soluble in alcohol, but ether and the essential oils do not dissolve any of it.

Concentrated sulphuric acid poured upon the salicine gives it a fine red colour, like that of bichromate of potash. It melts four degrees above the heat of boiling water, an increase of heat gives it a fine citron yellow colour, and renders its fracture like that of a resin.

It is composed, according to MM. Pelouze, Jules, and Gay-Lussac, of

Carbon,	55.49	2 Proportions.
Hydrogen,	8.18	2
Oxygen,	36.33	1
	100.00	

Its composition may be represented by two volumes of olefiant gas, and one of oxygen.—*Ann. de Chim.* June 1830, p. 220.

7. *Carburet of Sulphur not decomposed by Electric forces.*—In this *Journal*, No. iii. New Series, p. 183, we have mentioned M. Becquerel's remarkable experiment on the decomposition of carburet of sulphur by small electric forces. According to M. Wohler, the black deposition on the sides of the tube is not carbon, but merely the sulphuret of copper produced from the sulphur in the sulphuret of carbon.—Poggendorf's *Annalen*, tom. 18, p. 482.

III. GENERAL SCIENCE.

8. *Mortality among Leeches during storms.* (*Fer. Bull.*)—That atmospheric changes have a remarkable influence upon leeches, is a well established fact. In 1825, M. Derheims, of St Omer, ascribes the almost sudden death of them, at the approach of, or during storms, to the coagulation

of the blood of these creatures, caused by the impression of the atmospheric electricity. This opinion, which at that time was the result of theory, he confirmed in the month of March last, by direct experiment.—*Ann. des Sciences d'Observation.*

9. *Prizes.*—At the Anniversary Meeting of the Royal Society of London on the 30th November, the President announced that the Council had adjudged the Royal Medal to Dr Brewster, for his recent communications on Light; and the Copley Medal to M. Balard of Montpellier, for his discovery of Brome.

Art. XVIII.—LIST OF PATENTS GRANTED IN SCOTLAND SINCE SEPTEMBER 16, 1830.

28. September 16. For an Independent Safety Boat of Novel Construction. To William Dobree, county of Middlesex.

29. September 16. For certain Improvements in Distillation and Evaporation. To William Shand, county of Kincardine.

30. September 16. For certain Additions to the Engines commonly called Locomotive Engines. To Charles Blacker Vignoles, London, and John Ericsson, county of Middlesex.

31. September 16. For an Apparatus calculated to prevent or render less frequent the explosion of Boilers in generating Steam. To Joseph Cochaux, London.

32. September 17. For certain Improvements in Machines or Machinery for Cutting Timber into Veneers or other useful forms. To Alexander Craig, Mid-Lothian.

33. September 17. For certain Improvements in the Process of Making and Purifying Sugars. To Marmaduke Robinson, Junior, Westminster.

34. September 22. For an Improved Fid. To Henry George Pearce, Richard Gardner, and Joseph Gardner, Liverpool.

35. September 22. For certain Improvements in the construction of Wheels for Carriages to be used on Railways. To William Losh, county of Northumberland.

36. October 16. For an Improvement in the Manufacture of Painting-Brushes and other Brushes applicable to various purposes. To Timothy Mason, Middlesex.

37. October 16. For an Improvement in the Preparing or Making of certain Sugars. To William Augustus Archbold, Middlesex.

38. October 21. For certain Improvements in the Apparatus or Machinery used in the Processes of Brewing and Distilling. To Æneas Coffey, Dublin.

39. October 21. For an Improved method of Lighting Places with Gas. To Michael Donovan, Dublin.

40. November 11. For an Economical Apparatus or Machine to be applied in the process of Baking for the purpose of Saving Materials. To Robert Hicks, Middlesex.

41. November 23. For certain Machinery, and the Application thereof to Steam Engines for the purpose of Propelling and Drawing Carriages on Turnpike Roads and other Roads and Railways. To JOHN HEATON, WILLIAM HEATON, GEORGE HEATON, and REUBEN HEATON, county of Warwick.

42. November 23. For certain Improvements in Printing Machines. To AUGUSTUS APPLEGATH, county of Kent.

43. November 23. For certain Improvements in Making or Preparing Saddle Lining, Saddle Cloth, and Girths for keeping Saddles in their place on Horses or other Animals of burden. To SAMUEL CLARKE, county of Devon.

44. November 23. For Improvements in Evaporating Fluids applicable to various purposes. To JOSEPH GIBBS, county of Kent.

45. November 23. For certain Improvements in Machinery or Apparatus for Printing Calicoes and other Fabrics. To MATHEW BUSH, Dumbarton.

46. November 23. For certain Improvements on Locomotive and other Carriages or Machines applicable to Rail and other Roads, which Improvements or part or parts thereof are also applicable to Moving Bodies on Water, and Working other Machinery. To THOMAS BRAMLEY, county of Surrey.

47. November 30. For certain Improvements on Machines or Apparatus for Measuring Land and other Purposes. To JAMES CHESTERMAN, county of York.

ART. XIX.—*Summary of Meteorological Observations made at Kendal in September, October, and November* 1830. By Mr SAMUEL MARSHALL. Communicated by the Author.

State of the Barometer, Thermometer, &c. in Kendal for September 1830.

Barometer.	Inches.
Maximum on the 1st,	30.07
Minimum on the 23d,	28.98
Mean height,	29.51
Thermometer.	
Maximum on the 4th,	61°
Minimum on the 30th,	39°
Mean height,	52.01°

Quantity of rain, 8.027 inches.
Number of rainy days, 22.
Prevalent winds, south-west.

In the summary of the weather for this month, the almost constant rain we have had must be noticed, as out of the 30 days there has been rain on 22 of them. About the time of the equinox, violent gales of wind prevailed for many days both before and after the 23d. The barometer has kept about a mean between 29 and 30 inches, during the greater part of the month. The mean temperature of the month has been very nearly equal to that of June, and yet the weather generally has been considered cold and chilly. The aurora borealis has been seen during the month, and it has always been followed by rain in about twenty-four hours after its appearance. The wind has been in the west and south-west 21 days.

October.

Barometer.	**Inches.**
Maximum on the 10th, - - -	30.38
Minimum on the 29th, - - -	29.32
Mean height, - - - -	29.98
Thermometer.	
Maximum on the 8th, - - -	60°
Minimum on the 17th, - - -	30.5°
Mean height, - - -	48.37°

Quantity of rain, 4.695 inches.
Number of rainy days, 16.
Prevalent wind, west.

Since the 18th we have had almost continued rain; before that time we had about a fortnight of dry weather. On the whole, the early part of the month was very fine, and the barometer during the whole of the month has been high. The thermometer has indicated frost only on four nights, and in the day time we have had none. No snow has yet been observed on the hills. On the evening of the 5th a luminous arch of light crossed the heavens, and lasted at least an hour, from 8 to 9 o'clock. No streamers were observed, probably on account of the moonlight.

November.

Barometer.	**Inches.**
Maximum on the 24th, - - -	30.24
Minimum on the 7th, - - -	28.74
Mean height, - - - -	29.55
Thermometer.	
Maximum on the 4th, - - -	55.5°
Minimum on the 19th, 24th, and 25th, - -	31°
Mean height, - - - - -	43.10°

Quantity of rain, 10.023 inches.
Number of rainy days, 24.
Prevalent wind, south-west.

The barometer has been mostly low during this month, and as yet we have had very little frost; indeed none except on the three nights abovementioned. The aurora borealis was seen on the evening of the 3d. Until the 18th we had not one day on which rain did not fall, mostly in large quantities, and during the greater part of the day. From the 18th of last month to the 18th of this, there were but two days when rain was not measured. The difference in temperature between the days and nights has been very trifling, and generally but a few degrees. The total quantity of rain measured this year is 55.948 inches. Snow was seen on the hills for the first time this season, on the 15th.

ART. XX.—REGISTER OF THE BAROMETER, THERMOMETER, AND RAIN-GAGE, *kept at Canaan Cottage.* By ALEX. ADIE, Esq. F. R. S. Edin.

THE Observations contained in the following Register were made at Canaan Cottage, the residence of Mr Adie, by means of very nice instruments, constructed by himself. Canaan Cottage is situated about $1\frac{1}{2}$ mile to the south of Edinburgh Castle, about 3 miles from the sea at Leith, and about $\frac{1}{4}$ of a mile N. of the west end of Blackford Hill. The ridge of Braid Hills is about 1 mile to the south, and the Pentland Hills about 4 miles to the west of south. The height of the instruments is 300 feet above high water-mark at Leith. The morning and evening observations were made about 10 A.M. and 10 P.M.

SEPTEMBER 1830.

D. of Week.	Day of Month.	Thermometer. Morn.	Thermometer. Even.	Thermometer. Mean.	Register Therm. Min.	Register Therm. Max.	Register Therm. Mean.	Barometer. Morn.	Barometer. Even.	Rain.
W.	1	59	55	57	52	62	57	29.94	29.92	.07
T.	2	53	54	53.5	56	62	59	29.72	29.48	.04
F.	3	56	51	53.5	48	57	52.5	29.51	29.72	.06
S.	4	53	50	51.5	45	58	51.5	29.74	29.48	.13
S.	5	56	52	54	46	59	52.5	29.25	29.10	.02
M.	6	55	53	54	48	58	53	29.25	29.54	
T.	7	54	53	53.5	50	58	54	29.70	29.78	
W.	8	56	52	52	43	62	52.5	29.82	29.66	.07
T.	9	53	51	50	48	60	54	29.41	29.42	.05
F.	10	53	48	50.5	41	60	50.5	29.48	29.45	
S.	11	49	48	48.5	45	60	52.5	29.43	29.12	.31
S.	12	57	48	52.5	44	59	51.5	29.04	29.19	
M.	13	52	48	50	42	61	51.5	29.26	29.11	.16
T.	14	59	50	54.5	44	56	50	29.07	29.05	.06
W.	15	54	47	50.5	44	60	52	29.17	29.21	.90
T.	16	42	51	46.5	48	56	52	29.92	29.23	.01
F.	17	48	49	48.5	55	61	58	29.56	29.54	.01
S.	18	54	47	50.5	40	60	50	29.40	29.45	
S.	19	54	52	53	44	59	51.5	29.25	28.90	.04
M.	20	54	45	49.5	45	57	51	28.85	29.00	.15
T.	21	51	46	48.5	40	52	46	28.96	28.92	1.27
W.	22	55	48	51.5	43	58	50.5	29.28	29.21	
T.	23	58	48	53	43	60	51.5	29.80	29.00	.10
F.	24	52	50	51	43	55	49	28.96	28.80	.10
S.	25	56	45	50.5	46	59	52.5	29.26	28.70	.10
S.	26	55	55	55	42	58	50	29.95	29.86	
M.	27	60	56	58	52	64	58	29.78	29.86	
T.	28	56	46	51	48	59	53.5	29.92	29.86	
W.	29	54	44	49	43	57	50	29.90	29.88	
T.	30	55	53	51	36	58	47	29 70	29.67	
	Sum,	1623	1195	1559	1364	1765	1561.5	884.08	880.91	3.65
	Mean.	54.1	49.83	51.97	45.47	58.85	52.15	29.469	29.363	

OCTOBER 1830.

D. of Week.	D. of Mon.	Thermometer. Morn.	Thermometer. Even.	Thermometer. Mean.	Register Therm. Min.	Register Therm. Max.	Register Therm. Mean.	Barometer. Morn.	Barometer. Even.	Rain.
F.	1	57	42	49.5	41	60	50.5	29.65	29.82	
S.	2	52	52	52	35	57	46	29.78	29.58	
S.	3	58	55	56.5	48	58	53	29.63	29.43	
M.	4	55	48	51.5	45	58	51.5	29.83	30.08	
T.	5	55	46	50.5	45	59	52	30.20	30.19	
W.	6	55	53	54	45	57	51	30.08	30.05	
T.	7	55	52	53.5	47	58	52.5	30.09	30.13	
F.	8	56	49	52.5	48	59	53.5	30.15	30.25	
S.	9	58	50	54	45	61	53	30.32	30.35	
S.	10	52	48	50	46	54	50	30.35	30.30	
M.	11	52	44	48	45	54	49.5	30.23	30.20	
T.	12	48	47	47.5	40	51	45.5	30.25	30.21	
W.	13	52	49	50.5	45	53	49	30.25	30.17	
T.	14	52	38	55	38	55	46.5	30.10	30.07	
F.	15	46	41	43.5	34	54	44	30.06	30.05	
S.	16	48	36	42	33	54	43.5	30.10	30.15	
S.	17	47	37	42	30	54	42	30.16	30.11	
M.	18	48	43	45.5	32	51	41.5	30.00	29.86	
T.	19	51	57	54	39	57	48	29.66	29.68	
W.	20	59	59	59	49	61	55	29.50	29.50	
T.	21	56	58	57	47	60	53.5	29.66	29.70	.07
F.	22	52	47	49.5	47	53	50	29.95	29.97	
S.	23	54	50	52	43	58	50.5	30.09	30.14	
S.	24	52	54	53	48	54	51	30.05	29.80	.07
M.	25	52	42	47	42	58	50	29.58	29.68	
T.	26	42	40	41	38	47	42.5	29.95	30.00	.02
W.	27	48	54	51	37	54	45.5	29.95	29.52	
T.	28	52	42	47	42	56	49	29.36	29.17	
F.	29	48	35	41.5	41	50	45.5	29.20	29.60	
S.	30	43	48	45.5	31	48	39.5	29.77	29.50	
S.	31	54	53	43.5	43	56	49.5	29.57	29.52	
		1609	1469	1539	1291	1717	1501	927.32	926.87	.16
		51.9	47.39	49.64	41.64	55.39	48.52	29.913	29.905	

NOVEMBER 1830.

D. of Week.	D. of Mon.	Thermometer. Morn.	Thermometer. Even.	Thermometer. Mean.	Register Therm. Min.	Register Therm. Max.	Register Therm. Mean.	Barometer. Morn.	Barometer. Even.	Rain.
M.	1	53	53	53	51	56	53.5	29.50	29.71	.02
T.	2	55	55	55	48	58	53	29.72	29.52	
W.	3	57	57	57	45	58	51.5	29.32	29.32	.34
T.	4	46	44	45	44	51	47.5	29.29	29.29	.03
F.	5	47	42	44.5	38	50	44	29.40	29.22	.31
S.	6	42	42	42	32	47	39.5	29.12	28.53	.68
S.	7	44	35	39.5	32	47	39.5	28.75	29.13	
M.	8	39	43	40	30	44	37	29.37	29.33	
T.	9	48	48	48	42	51	46.5	29.26	29.22	
W.	10	51	41	46	42	52	47	28.89	28.88	.24
T.	11	44	41	42.5	36	47	41.5	28.89	29.20	
F.	12	47	48	47.5	37	49	43	29.53	29.51	
S.	13	48	48	48	43	49	46	29.20	29.10	
S.	14	43	40	41.5	38	43	40.5	29.40	29.10	.12
M.	15	42	49	45.5	36	47	41.5	29.03	28.76	.33
T.	16	48	42	45	38	48	43	28.65	28.82	
W.	17	42	40	41	36	45	40.5	29.20	29.58	
T.	18	39	36	37.5	31	46	38.5	29.74	29.81	
F.	19	44	46	45	30	47	38.5	29.76	29.56	
S.	20	46	45	45.5	43	49	46	29.23	24.35	
S.	21	48	45	46.5	43	50	46.5	29.33	29.00	.40
M.	22	43	40	41.5	36	46	41	29.33	29.60	
T.	23	43	36	39.5	35	45	40	29.92	30.20	
W.	24	40	40	40	28	43	35.5	30.13	30.08	
T.	25	42	36	39	38	44	41	30.07	30.10	
F.	26	41	38	39.5	30	42	36	30.07	29.78	
S.	27	40	38	39	36	42	39	29.65	29.56	
S.	28	42	41	41.5	36	42	39	29.53	29.55	.10
M.	29	42	40	41	39	42	40.5	29.90	29.85	.05
T.	30	40	39	39.5	38	42	40	29.90	29.85	.51
		1346	1288	1317	1131	1422	1276.5	883.08	882.51	3.13
		44.87	42.93	43.9	37.7	47.4	42.55	29.436	29.417	

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ART. I.—*Meeting of the Cultivators of Natural Science and Medicine at Hamburgh, in September* 1830. By JAMES F. W. JOHNSTON, M. A. &c. &c. Communicated by the Author.

Während man in der ältern zeit die naturforschung als eine angenehme aber nutzlose Beschäftigung und als ein harmloses Spielzeug müssiger Köpfe ansah, hat man sich in der neuesten zeit immer mehr von ihrem grossen Einfluss auf den Cultur-Zustand und das Wohl der Völker überzeugt:—und so sehen wir die Lenker der Völker bemüht grossartige Anstalten zu ihrer Beförderung und Erweiterung zu errichten.

Whereas, in former times, men regarded the inquisition of nature as a pleasant but useless employment, and as a harmless pastime for idle heads, they have, of late years, become every day more and more convinced of its influence upon the civilization and welfare of nations, and the leaders of the people are everywhere bestirring themselves for the erection of establishments to promote its advancement and extension.

Tiedeman's Address to the Meeting at Heidelberg in 1829.

MANY illustrations of the fact stated in this sentence of Tiedeman's oration are to be found in our own country, in which the change of public opinion in regard to scientific pursuits has been nearly as great as in any other; but no single illustration of it to be met with in any country is more strikingly instructive, than what is contained in the history of the Society of German Scientific Men. Commencing at its outset with the trifling concourse of some twenty lovers of natural science, it has every year augmented and grown,—despite of the openly avowed hostility of some governments, and the secret espionage of others,—till, in the short space of nine years, it has attained to the character of a great national congregation, of which the most distinguished naturalists of the age are proud to be members, and which kings vie with each other in honouring. At first a few cities only were open to them, and the dread of political associations shut many gates against them;—now their task is to choose among many rival claimants, each of which would gladly entertain them. They are borne along now by the tide of public opinion, directing at once and directed by it; and the honour formerly bestowed willingly on individuals, from a knowledge and appreciation, in some degree, of their labours, is now bestowed with equal cheerfulness, and with great increase, on the whole body of indefatigable men, of which individual philosophers are but members.

The Society of German Naturalists owes its origin to Professor Oken of Munich. This indefatigable and free-minded man was formerly Professor of Natural History in the University of Jena in Weimar, to which chair he was appointed in 1807. He was already favourably known for five or six volumes in natural science, especially zoology; and, amidst his professional labours, he found leisure for adding every year one or two to their number. Among these was *A System of Natural Science*, *A Treatise on Light and Heat*, and *A System of Natural History*. In 1817 he commenced at Jena a monthly journal of Literature and Science in a quarto form, of which he still continues to be the editor. But the times were critical, or men in power, at least, thought them so. The principles of the Holy Alliance demanded a strict surveillance of the periodical literature; and it was dangerous for small states to give countenance to liberal men, or to permit political treatises to be published in their dominions. Oken cared little for men in power. He acted independently, and admitted into his Journal some articles of a political nature, which gave high offence. The consequence was the intimation, "either you must discontinue the *Isis*, or give up your chair."—"I told them," said Oken, "I cared nothing for their chair, and I would go on with the *Isis* in spite of them." Of course, he lost his chair; and, though he was allowed to remain, the *Isis* was forbidden to be published in Weimar. The publication therefore was transferred to Leipzig, while Oken continued to reside at Jena. In 1827 he obtained a chair from the King of Bavaria in the university of Munich, where he is now professor of physiology.

Oken is a little man, probably near fifty years of age, of dark, yet sanguine complexion, and features whose habitual, if not natural, expression, is severity and determination. His dark eye and compressed lips have a forbidding and distance-keeping expression, for one can read upon them our own national motto, "Nemo me impune lacessit." I do not know how far his power of saying severe things corresponds with his apparent inclination; but, if the one equal in amount only half of the other, I should be very sorry indeed to come under his lash.

In conversation Oken is nevertheless pleasant and communicative; and I shall not soon forget the buzz and general sensation,—the turning of eyes, and moving of feet, in the rooms of the Apollo Saal, on the occasion of our first public soirée, when the words, "Oken is come!" were passed along the assembly. His friends crowded first to greet him, after which foreigners and other strangers were severally introduced; and one could easily forgive the slight air of patronage with which he, though the last comer, *welcomed* them to the meeting, when we considered how goodly an assemblage his efforts had brought together.

It was in the *Isis*, and while still at Jena, that Oken proposed the plan of a great yearly meeting of the cultivators of natural science and medicine, from all parts of the German Fatherland. It was a noble idea, and nobly has it at length been brought about. But in 1821 Oken was still a tainted man,—the remembrance of his political sins was still fresh,—and his proceedings were consequently regarded with suspicion. Societies of all sorts were dreaded by the German governments, and they feared some hidden and dangerous design under the guise of a concourse of philosophers. But open influence could not be exerted against that which as yet had no being; and, in 1822, the first meeting took place at Leipzig, Dr Schwägrichen, Professor of Botany, in the chair. But what a contrast did this first meeting present to those of the last three or four years. At Leipzig there were about twenty came together from the city, and these were joined by about a dozen strangers. It was, however, a beginning. In 1823 they met in Halle in greater force, the celebrated botanist Sprengel being president, and Schweigger, well known for his Journal, which he has edited so indefatigably for twenty years, being secretary. Wurtzburg, famed as a medical school, was the seat of the third assemblage, D'Outrepont and Schönlein, of the medical faculty of that University, holding office. They now began to muster strong, both in numbers and in talent; and here the meeting first obtained a consistency and fixed establishment. Frankfort received them hospitably in 1825; and the scientific men, and the authorities of the city, united in showing attention to the strangers. This place had the honour of first adding public respect to the private entertainments got up by the inhabitants to gratify their visitors; and, among these private entertainments, that of Banker Bathman deserves especial commemoration. If not the first, it was the greatest yet paid to the entire body since their assembling commenced. Thus Frankfort, though the seat of no university, has a claim to much merit as a patron of scientific men. At Dresden, in the following year, preparations were also made for their reception; and the learned men connected with that seat of the fine arts, exerted themselves to make their visit a pleasant one. Seiler, the Director of the Surgical Academy, was president, and Carus the Anatomist was secretary. The sixth meeting in 1827 was held at Munich, the seat of a flourishing University, opened only the preceding year under the favouring auspices of Louis Maximilian of Bavaria. This city also deserves well of the Society, and the attentions of the King were such as it had not hitherto experienced. Besides general attention to the comfort and accommodation of the whole body, particular attentions were paid to the individual members; and each person, during the period of his stay, had an invitation to dine at least once in the palace. They began now to reckon their number by hundreds; and the amount and variety of subjects brought forward at their public meetings having increased beyond expectation, it was found necessary to break themselves up into sections, of which the botanists, an amiable and enthusiastic race of men, first set the example. Thus time was gained ; men of like tastes and pursuits brought more frequently and more closely together ; and every one spared the infliction of dissertations and discussions upon the thousand and one subjects in which he felt no earthly interest ; for, though all cultivators of natural science rejoice in the advancement, and admire those who successfully cultivate any one department, yet each one has his own favourite branch or branches, beyond which he has little anxiety to roam, and unconnected with which discussions, however learned, are often only tiresome. It was a judicious plan, then, to make the separation into sections, and thus to permit the shell and fly men to discuss the mysteries of their several ologies, without scandalizing the more *grave* and *weighty* pursuits of medicine and oryctognosy. This practice, begun at Munich, assumed a more extended and definite form at Berlin, and was finally arranged and consolidated at Heidelberg.

Berlin gave a powerful impulse to the rising distinction of the Deutscher Naturforscher Versammlung. Every thing was done by the Prussian government, and under the immediate superintendence of the distinguished president, Baron Humboldt, for the convenience and accommodation of the strangers ; and arrangements were there first entered into by which the comfort of those from a distance was materially increased. Purses, even, as well as persons were attended to ; and as living in hotels was considered too expensive for many, who, nevertheless, in a strange place, would be unable to provide themselves with private lodgings, several hundreds of the latter were secured, chiefly in situations which gave convenient access to the places of meeting ; and thus, the most complete strangers found themselves at once economically and conveniently situated. Fetes and excursions also were got up, and concerts given, which the royal family, and even the king himself, graced with his presence ; while poetry and music lent their aid to welcome and eulogize the votaries of science. The celebrated Humboldt presided, and Lichtenstein, the well known zoologist and South African traveller, held the office of secretary. The number of strangers who came from various parts of Germany and the northern countries, was 269 and the total amount of members enrolled was 464. In this numerous assemblage of learned men, England had but one representative. Yet one man may be worth a host, and the science of England suffered no derogation in the person of "Charles Babbage, London." This meeting at Berlin was by far the most splendid that has yet taken place, not from the number of strangers who attended it, for in that respect it was nearly equalled by the late meeting at Hamburgh, but from the circumstance of its being held in the capital of a powerful kingdom, where the government had shown a disposition to honour, and pay attention to scientific men,—in Berlin, the seat of the first university in Germany, where the professors of every science are among the most eminent men, and the collections in every department of natural history of the most splendid description to be found in the whole empire. It remains to see what Vienna can do, in the ensuing September, to rival the more northern metropolis.

The beautiful and romantic city of Heidelberg was the seat of the following anniversary: Tiedeman and Gmelin, whose names have so long illustrated the university of Heidelberg, and whose labours have so much increased and diffused the knowledge of the anatomical and chemical sciences, held on this occasion the two official situations. At this meeting the number of strangers amounted only to 193, and the total amount of members to 273, but it proved, nevertheless, a very interesting and satisfactory assemblage. Besides the zeal for science, there are always many circumstances which will influence the resort of naturalists to any one place of meeting. The most important of these are the distance and the facility of access. To obviate the former in some measure, and to bring the meeting occasionally, at least, near the homes of all, it has become the custom to select in alternate years a city in the north and south of Germany, as the place where the assembly shall be held. But while this regulation secures the attendance of the men of the south, for example, it excludes almost, *ab ipsa re*, all those of the north from any participation in the proceedings. There are few who have leisure for extended journies of this kind every year, and still fewer whom circumstances will permit to undertake them. It does not follow, therefore, from a diminution of numbers at any one anniversary, that any diminution in the zeal of philosophic men, or of their estimation of the benefits to be derived from frequent general intercourse, has taken place, but simply that the facilities of attaining these benefits has been less. All circumstances considered, therefore, the meeting at Heidelberg, though less numerous, by far, than that of the Prussian capital, was at once creditable to Germany,—creditable to the men of science in which it abounds,—and highly creditable to the city in which it was held.

Among the autographs appended to the account of this meeting, drawn up by Tiedeman and Gmelin, I find "Robert Brown, botanist, London,"—a man of whom Agardh said to me, "I believe him to be the greatest botanist of this or any other country;" and "Andrew Duncan, Materia Medica, Edinburgh," for whom many earnest and kind inquiries were made of me at the subsequent meeting in Hamburgh.

It has become now a matter of debate among the cities of Germany, which shall have the honour of receiving the society at their anniversary. To have the smallest chance, the city desirous of the honour, must either be represented by a deputation of members attending the meeting, or must otherwise express to the society through its president,—its desires, its claims, and the efforts it will make for general accommodation. An application of this kind from Prof. Oersted in Copenhagen, gave rise to a discussion of considerable importance. The society is entitled a *German* society, and by the spirit of the statutes, its meetings can be held only in the cities of Germany. It was, therefore, proposed by Dobereiner, of Jena, and Muncke, of Heidelberg, that the terms of the statutes should be altered, leaving to all future meetings the power of nominating cities beyond the boundaries of Germany, as places of assembly. This motion was opposed by Lichtenstein, who argued very judiciously that it would be time enough to make such an alteration in the statutes when the greater part of the German cities had been visited, and they found themselves at a loss where to go. The laws, therefore, were allowed to remain unaltered, and after some rivalship between Hamburgh and Gotha, the former was fixed upon as the seat of the ninth anniversary. It seems indeed an unreasonable demand on the part of the Copenhagen men of science, and one which would greatly enhance the evils arising from distance, and want of access above alluded to, to ask a transfer of the place of meeting from Germany to the Danish islands. It is the king of Denmark who is anxious for a visit of so many learned men, in the hope that it may give an impulse to the science and education of his own dominions, which he is sincerely desirous to foster and encourage by every means in his power. But could the Danes and Swedes forget their mutual hostility,—and what have science and scientific men to do with national animosity,—it were better to institute a "*Scandinavian* Society," in imitation of the German, and such as we hope to see before long in the British islands. This society would embrace Sweden, Norway, Finland, Denmark proper,—and the Duchies might also be included; and in this wide field, which has produced so many of the greatest men in science, there are ample materials for the formation of a scientific anniversary of the most splendid description. Were Berzelius, Oersted, and Pfaff to unite their efforts, the matter would be accomplished at once. There are in each country able and promising men to second them, and the governments of Denmark and Sweden would not be slow in rendering them every necessary assistance. The meetings would not be so numerous as those in Germany now prove to be, but from that very circumstance they would derive an additional interest, and be doubly beneficial. I consider it one of the greatest objections to the German meetings, that they have now become so numerous as to defeat the great object for which they were instituted,—to enable men of science to cultivate an acquaintance with one another. It was only necessary, for example, to attend the late assembly at Hamburgh, to see how impossible it was for such acquaintances to be formed to any extent. The pleasantest days I spent there were at the commencement, when I had leisure to learn to know well a few men. When the crowd came, every body accosted every body, and no particular person could be met with at any one time or place. You saw faces, and were introduced to people for whom you cared nothing, and when you had at last laid your hands upon a man whose conversation you could enjoy, straightway another person insinuated himself between you, or sat down beside you, or came up with some foreign subject in his mouth,—and so farewell to your quiet instructive tête-à-tête. It is all very well for a day or two, to run from flower to flower in this way,—dropping here a little word, and there a little word, but it is extremely unsatisfactory in the end; and so one finds it when he sits down at night, or after the lapse of a week so spent, to sum up what he has learned,—wherein he has been improved, or with what he has been enlightened,—he discovers that he has only been amusing himself,—and that he would have shown equal wisdom had he saved himself the toil and trouble of his long journey, and spent his evenings instead, in the theatre or the ball-room. A great deal might be done by maintaining better order, and by more judicious arrangements than were put in force at Hamburgh, but more or less of the evil I have adverted to will always attend meetings equally numerous.

The reports of these meetings hitherto published by the official directors, by Humboldt and Lichtenstein of the meeting at Berlin, and by Tiedeman and Gmelin of that at Heidelberg, are only meagre outlines of the proceedings. They give the president's opening speech in full, or mention that such and such things were done on certain days, and record only the titles of the papers read on the different branches of science. They are nothing but mere formularies—useful indeed in showing how and in what order the *affair* was conducted, but almost entirely uninteresting in a scientific point of view. The best accounts published, have been those of *Oken* in the *Isis*. These contain not only the order of the proceedings, but all the important papers read,—an outline of the subjects discussed, at least in some of the sections,—notices by the editor of the persons or things most worthy of remark in the place where the assembly had met; and critical remarks upon things said or done, or suggestions for saying or doing them better on future occasions. One objection, however, lies against all these reports and statements; they are excessively slow in making their appearance; so that, even in Germany, many months elapse before any thing better than flying or ephemeral reports of the proceedings reach those whom circumstances had prevented from assisting at them. It is not my intention to follow the example of Oken, unless it be in occasional strictures upon the proceedings. Few individuals can obtain materials for a complete report; but I shall group together such a number of desultory remarks regarding the proceedings, the persons assembled, and the places they represented, as will, I hope, amuse and interest my readers, while they at the same time give a general idea of the way in which these meetings go off among our German neighbours.

By the regulations of the Society, the first public meeting takes place on the 18th of September. Members generally arrive before that time, however, and private meetings, both individually and in parties, take place several days earlier. One person drops in after another so slowly and gradually, that if you are on the spot when they begin to assemble, you have the opportunity of renewing old or of making several new and interesting acquaintances. The points of a man's character which are to attach him to us are not always discernible at a glance, and it requires time and opportunity for other men also in whose memory we should like to hold a place, to become sufficiently acquainted with us, and with our claims upon their regard. I do not consider it enough to have seen and conversed with a great man; I wish, if possible, also that he should remember to have seen and spoken with me. The latter wish implies a little more vanity perhaps than the former, but both spring, I conceive, from principles equally virtuous and equally laudable. The one implies an admiration of that virtue of whatever kind which has raised another to eminence, the other, the desire of displaying like virtue ourselves. These two desires do not co-exist, or do not co-exist with equal ardour in the breasts of all men; and, therefore, all will not feel with equal force the objection I have stated above, as occurring to me when wandering among four or five hundred individuals, from whom it was required to make a judicious selection, and to become acquainted with those you had selected, all in the space of six days. I reached Hamburgh, fortunately, in sufficient time to enable me to see many on their arrival, and to me those days were by far the pleasantest which I was enabled to spend with a few whose time was still at their own disposal.

Among this few was the amiable Agardh, with whom an acquaintance begun in Lund, was here strengthened and improved.

In Hamburg, a city of merchants, among whom a rise or fall in the funds of which it is the mart, is of absorbing interest; a luxurious people, of whom it may truly be said that "their God is their belly,"—it was difficult—it was impossible to find a man who, on account of mere scientific distinction, deserved to occupy the chair of president at the meeting of the Naturforscher. But in the head burgomaster, Dr Bartels, they found a man who, having formerly written a book of travels, might with propriety be chosen,—who, from his knowledge, was capable of appreciating the objects and value of these scientific meetings;—from his forensic talents was capable of conducting the proceedings with ease and dignity,—and who, from his influence as chief magistrate of the city, was most able to provide for their accommodation, and to secure them that attention from official men to which they were entitled, and to which they had been accustomed. The president, therefore, was well and prudently chosen, and it is an act of justice to a very worthy, kind, and talented old man to say, that his general conduct in the chair, his attention to individuals, and the judicious arrangements made under his superintendence for the comfort and enjoyment of all, were such as to give general satisfaction. Where any thing was found fault with, or there appeared any thing deserving of criticism, the universal feeling was, that he at least was not to blame.

But in the choice of a secretary they were not so happy; they made indeed a very unfortunate, and, as it proved, a very unsatisfactory choice. It is not my intention to say any thing harsh of Dr Fricke, but certainly his temper, his manner towards the strangers, and his general conduct in the discharge of his office, showed him to be entirely unfitted for so distinguished and peculiar a charge. Fricke stands high in Hamburgh as a surgeon, and is esteemed a successful operator; but his fame chiefly rests on his practice in curing syphilis without the aid of mercury. This practice was the subject of considerable discussion in the medical section during some of their visits to the Krankenhaus, (Hospital) of which Fricke is surgeon, Its route in the north, for it has been a travelling practice, was from England to Copenhagen, some twenty years ago, thence to Stockholm, and after being rejected in both places, it has taken refuge in Hamburg with Dr Fricke. That he has employed it successfully there is no doubt, and so, in certain circumstances, may any practitioner; and the precautions taken in regard to this disease by the authorities, and the consequent general mildness of the cases, sufficiently account for the success with which it has been attended in this city.

Of the professional men in Hamburg, Dr Lehmann of the Botanic Garden was the person best fitted, as well by his scientific reputation as by his amiable and gentlemanly manners, for the office of secretary. But there are always minor and unavowed reasons for such appointments, even among men of science; and Dr Fricke having the honour of bearing to the meeting at Heidelberg the invitation of the Hamburg burgomasters, was almost as a matter of course appointed secretary to the ensuing meeting. To assist the secretary, a committee of directors was also appointed, chiefly medical men residing in Hamburg, that the arrangements for the reception of so many strangers might be more easily and more fully completed.

On reaching Hamburg, the first duty of the stranger was to repair to the *Stadthaus*, the seat of the police and other minor courts, where, after elbowing his way through a tribe of ragamuffin-looking officers and still more wretched culprits, he found his way to the main staircase; and, on announcing himself as a *naturforscher*, he was shown up one or two flights of steps, and ushered into the grand room of state, where the banners of the Hamburgers wave from the walls, and a series of portraits commemorate at once the illustrious friends of the Hanse towns, and testify at the same time the gratitude of the sovereign senate of the Merchant Queen of Germany.

It depended entirely upon the day of the month whether the scene which presented itself on entering this room were worthy of especial notice or the contrary. If it were still only the 13th or 14th of the month, he would see perhaps a dozen or twenty people standing in groups of three or four in different parts of the room, and an occasional rare ejaculation would reach him as some communication of interest was made,—probably regarding what persons were on their way to the meeting. Such was the case when Agardh and I on the 12th entered the room. To all we were immediately introduced by the directors—each found some pleasant person or persons to converse with; and in cultivating personal acquaintance with men whose names you had probably often heard of, an hour passed quickly away. There were as yet no other public meetings than these two morning hours from nine to eleven, and they were chiefly for the purposes of enrolment, and the delivery of their tickets of admission to the strangers as they arrived.

But every succeeding day the interest of these *mornings* increased exceedingly, and I consider it a strong inducement to be early in repairing to the place of meeting, that the scenes which ensue on every fresh arrival may be seen and enjoyed. A man in his travelling-dress walks into the room, and goes straight up to a group on his left, where he recognizes a well known face. A scream of joyful recognition, and a host of loud exclamations, and a mutual behugging and *beslobbering* with salutations, first on the one side of the face and then on the other, with various shaking of hands and other such gestures attract the general attention; and "who is that?—who is that?" goes from one to another; and then there is a move of the men who know him, or who have heard of and wish to know him, and the rest are beginning to resume their conversation, when a second interruption arises from the entrance of a *great man* in another science, and another set of men is set on the *qui vive*, and thus perhaps an entire hour may be most delightfully spent in merely looking on, in studying the physiognomy, and in watching the phases of expression and deep interest that pass over the countenances of different individuals by the mere presence and contact of others, votaries of the same branch of study, whom they have hitherto known only by their labours, but whom, though unseen, they have deeply venerated.

The varied forms of salutation too are an interesting feature of such an assemblage, at least to us islanders. Saluting among the men is no where uncommon, I believe, from Torneo to the Straits of Gibraltar, but in some places it is more general than in others; and among some of the northern, the

Scandinavian people especially, it is ridiculously frequent. Were it not that these people smoke perpetually, and therefore disregard the trifling affair of breath, I should think it must in many cases prove a very disgusting custom, at least I who am no smoker have found it so. One little Polish professor from Warsaw, with whom I got very intimate at Hamburgh, used to inflict upon me a regular salute on both sides at every meeting and parting, and on bidding him farewell, and obtaining his blessing, I received a triple portion twice told from the worthy kind-hearted man. Fortunately for me *his breathings* were of the less tainted character.

Then, on presentation to a stranger, there is the *bowing*, and the *bowing*, and the bowing-interminable. First make your bow in front, then take a step to the left and make another, then two steps to the right and make a third, then one step to the left and make another bow in front. This is Scandinavian, and is the least you can do to a gentleman; where ladies are concerned, a Swede begins at the one end of a long room, and bows slowly all the way till he comes in front of the ladies seated at the other. Or in Germany, you see two real bowing men come close up in front of one another till their heads almost touch as they begin to bob, and bob, and bob again like so many Chinese Mandarines. An old man with a powdered head and only a few long teeth in front,—a little man with an interminable smile upon his phiz,—an apothecary from Brunswick—might set up, I think, as a model of this kind of bobbing, for he finished it off in the most characteristic style of any man I saw at the meeting. He is, however, a very worthy and kind-hearted man; and should any of my readers ever find themselves in the city of Brunswick, an hour devoted to visiting him they will not think ill spent.

And of verbal salutations, it is curious to hear so many different in the same apartment. "Mycka Tjenare" says the Swede,—"Hvorledes befinner de Dem" adds the Dane,—"Gut Tag, Gut Tag, wie gehts, lieber," says the German,—while the French "Comment vous portez-vous," serves as a general form of address among those who do not understand each others tongue. Then there is the mixing up and compounding of languages where so many are spoken, and so few can speak them all. In walking about in the large saloon where several hundreds are met together, you meet first a Swede, perhaps, and as he prefers his own tongue where he has an opportunity of using it, you do your best at a few sentences, making good use of the words you have still retained rusting upon your memory since you left the western shores of the Baltic. Then you encounter a German, and in two minutes you set him a laughing, and in two seconds more you join him yourself, when he tells you of a couple of Swedish and one Danish word you have popped into the sentence. You commence again with a third tongue only to make similar blunders, of which you never steer entirely clear, until you meet some one who can understand your native language. Such blunders in such a place, are unavoidable, and you hear them made so often that they cease to afford the amusement at first derived from them.

The arrivals were occasionally by single individuals from the smaller cities, sometimes by pairs; more generally a band of men from one university came together, headed by an acknowledged leader. In all cases, the great men formed the centres of little systems of other men, well content to play the second fiddle for the honour of going along with him. In other words, they came like little chieftains attended by their *tails.* Thus Agardh had his little tail of two men, as many as the university of Lund could afford. Berzelius could muster but one recruit at Stockholm, for the journey was expensive; but at Berlin his body guard was increased to three, while Pfaff and Wiedeman brought with them almost all the scientific men in their university.

Pfaff and Wiedeman are the ornament and pride of the University of Kiel. Pfaff is known for the depth and extent of his knowledge in natural science, and for his works on physiology, pharmacy, and chemistry. He is an extremely lively and pleasant person, and has sometimes, it is said, expressed his opinions of things more openly than was agreeable to certain governments, by which his character was neither so well known, nor his worth so well appreciated, as by his own paternal monarch. Travelling in Prussia some few years ago, when secret societies were all the order of the day, and the German governments in great alarm, he talked as usual,—more freely and boldly than was encouraged in that country. The Prussian government was offended, and Pfaff having got safe home, the ambassador at Copenhagen was charged to make a remonstrance on the subject; but the King paid no attention, and his ministers, therefore, could give the ambassador no satisfaction. Determined on pushing the affair, the ambassador had an audience of the King, and signified that the Prussian government expected Pfaff should be punished. "Oh," said the King, "Pfaff is my very good friend, he has only been a little *distrait;* he has fancied he was in his own country, where he might say anything:" a terrible satire, coming as it did from the most absolute monarch in Europe. Pfaff paid a visit to London in the summer of 1829, and on his way home again, Dr Bowring boasts of the honour of saving from a watery grave, one of the lights of Kiel and of the first men of his country.

Wiedeman, by some called the Astley Cooper of Germany, is the most celebrated *accoucheur* in Germany, and the only surgeon who has performed the Cæsarean operation twice with success, upon the same individual.* He was formerly attached to the medical school of Brunswick, and being employed professionally by the late ducal family, had the honour of bringing into the world the now expelled Duke Charles. Unfortunately he has been long afflicted with bad health, which has impaired his activity and usefulness; still his devotion to science remains, and his enthusiasm as ardent as upwards of threescore winters will permit. It was an effort which brought him to the meeting, for he had been complaining much. "I am in bad health," he wrote to a friend in Hamburgh, "but I will come to the meeting, if it should be on men's shoulders." Besides his medical pursuits, Wiedeman is also a zealous and learned entomologist, and my judicious friend Dr Traill of Liverpool, has remarked to me, as illustrating one of the grand points of distinction between medical men in this country and their brethren on the continent, that Professor Wiedeman, at the late meeting in

* The patient, I believe, was a little deformed ricketty woman, well known in Hamburgh.

Hamburgh, besides exhibiting drawings of the species of the genus *Mydas*, distributed also copies of a memoir in which he describes several new species of insects, confirming the propriety of the genius *Achias* of Lamarck, which has hitherto rested on a single specimen, and that one imperfect.* There is scarcely an eminent medical man in Germany, who is not also distinguished for his researches in some branch of natural history, and " what a contrast is this," he adds, " to our profession at home."

Berlin sent about twenty members, among whom, however, were but few of her eminent men. In the list are the names of Lichtenstein, Encke, Chamisso, and Otto, of the Botanic Garden. From Copenhagen came Professors Oersted of physics, Zeise and Forchhammer, of chemistry, Horneman of botany, Rheinhardt of zoology, and Jacobsen of physiology and anatomy. Christiania was unrepresented, as was also Upsala—the ancient seat of natural science, possessing now but the shadow of its former fame. Stockholm, besides its Berzelius, sent Professor Eckström the head of the surgical school, and Wickström, the botanist; Helsingfors in Finland, to which city the university has been removed since the fatal fire at Abo in 1827, sent Bonsdorf its professor of chemistry; Petersburg sent Fischer of the Botanic Garden; Moscow, Fischer the zoologist and President of the Academy of Sciences; not the *vegetable* but the *animal* Fischer, as he wittily observed to me when presented to him; while Warsaw sent its Jarocki, Mill, and Schubert, professors of zoology, physiology, and botany. Even Cracow sent Estreicher, its professor of botany, and Dorpat its Struve, well known to astronomers. Breslaw, so distinguished of late years among German universities for its eminence in natural science, sent not many men; but Otto the celebrated anatomist was of the number, and few places therefore were more worthily represented. The little university of Rostock, sent two of its professors, Dr Vogel of general medicine, a name honourable among German physicians, and Professor Siemsen of mineralogy. Griefswald sent one only of its thirty teachers, Hornschuch, professor of zoology.

* Wiedeman is also the author of an excellent work on *non-European* insects.

From Giessen came Wilbrand, professor of botany and zoology, the propounder of a new theory of the tides, and Professor Liebeg, of chemistry, a young man whose name is already familiar to chemists for several important researches and discoveries, of great devotion to his science, of great labour, and of greater promise. The university of Königsberg, the farthest north of the German seats of learning, but once raised to such eminence by the prelections of Emanuel Kant, kept entirely aloof from the Naturforscher, and the city itself was represented by a single medical man. Saxony was in a state of confusion, revolution was at work in Dresden and Leipzig, and there came few from the universities to the meeting at Hamburg. Jena sent but two members of its professional body; Leipzig, the same number with a few physicians; Freyburg and Marburg sent each one professor; and from the great university of Göttingen, (die perle deutschen hochschulen,) so near the place of meeting, and so famed for science, came also but one—Professor Osiander! Munich likewise sent only one, but he was the father of the assembly—Oken. Halle, celebrated for natural science, where Schweigger teaches chemistry—Nitzsch, zoology—Curt Sprengel and Kaulfuss, botany,—which boasts of 65 professors, and 1300 students, sent only two of its learned men to the meeting, Germar, professor of mineralogy, and the celebrated Krukenberg of clinical medicine. The city sent also my good friend Dr Meissner, editor of a pharmaceutical annual. From Vienna came one professor, Jacquin of botany and chemistry, whose father's works are known and prized by botanists, and with him young Dr Vivenot, a general favourite. Prague, the mother of the German universities, the rival towards the end of the fourteenth century of the famed schools of Bologna and Paris, and the proud nurse of 20,000 alumni—Prague now ranking in the second class of the German schools, in regard to natural science perhaps even lower, but numbering still 1500 students, and 55 public teachers,—the university of Prague sent but *one* man, Professor Presl, of botany and zoology. Shame on thee proud Prague, and shame on thee too haughty Göttingen, even Archangel shames you, for from the shores of the White Sea she too sent her one man; even the city of Baltimore, beyond the far Atlantic, where the sun looks down upon a new world, the city of Baltimore shames you, for she too sent her one professor! The *city* of Prague was more worthily represented in Batka, its talented and well known pharmacologist, from whom chemists have till lately been accustomed to receive their supplies of Selenium in the form of small medallions of Berzelius, its discoverer.

From Heidelberg came the celebrated Tiedeman, with Professors Muncke, of physics; Leuckart, of zoology; and Geiger, of pharmacy and pharmaceutical chemistry,—a man in high and deserved repute among his countrymen. Bonn sent three members of its professional body, among whom was Harless, known for his many works on practical medicine. The universities of Tubingen, Wurtzburg, Erlangen, and Basel, were wholly unrepresented, while Erfurt, which preserves still a trace of the university it once boasted, sent forth the venerable Trommsdorf to preside over the section of pharmacy; and the commercial town of Bremen its Müller, professor of physics, and its distinguished botanist, Mertens, who was called to the chair of the botanical section. The academy of Soroe in Zealand, deputed its professor of zoology, Hauch; while the school of medicine in Brunswick sent Sillem, its professor of mineralogy, and Marx, of chemistry and physics. Marx is zealously devoted to optics, and to the examination of the optical characters of minerals. "You know Dr Brewster," he said to me; "I esteem him more than all other scientific men. Does he come here?—I should like much to see him.—Is he professor in Edinburgh?"—"No, he has no public function." "But he will teach people when they come to him."—"Oh, but he lives in the country, far out of the way, many miles from Edinburgh."—"Then he gives no lessons, but he should give lessons to spread the knowledge which nobody else can give. There is only Herschel and he in your country who have occupied themselves with these subjects."—"But suppose he were in Edinburgh, and were to announce lectures, he would not obtain perhaps above two or three pupils on subjects generally supposed so abtruse."—"Ah, is it so; still he should try to spread his knowledge."

What Marx says is indeed true: as things now stand much valuable knowledge must die with Dr Brewster. There are many things in practical science which books can never make known at all, and still more which they can neither make known so soon nor so well as a few short living sentences with references to instruments and experimental illustration. It were a desirable thing, therefore, if, in connection with our seats of learning, there existed certain overlying and available funds by which the services of eminent men might be occasionally secured, even in departments the most recondite and abstruse, so that they might have an inducement to dedicate a portion of their time to the instruction even of a very few. On the continent this is easily effected. It is represented to the government, the King perhaps, that such a person is eminent in science, and he is without hesitation honoured with the title of Professor, and a certain salary, with power to lecture in a particular faculty, that of philosophy for instance, which includes all natural science, except the strictly medical departments. If his manner or his subject be unpopular, or if from any other cause none take his tickets, he is at liberty to pursue undisturbed his own investigations, while the title conferred on him is a just tribute of respect to his scientific reputation. In either case the state suffers little—his salary dies with him—science is advanced and benefited—the country is honoured as the means of that advancement—while it is provided also with a talented teacher, likely to keep the true scientific spirit alive in the land—and bound to instruct in the mysteries of his peculiar department any one who may feel himself drawn by congeniality of disposition to similarity of pursuit.

The worthy community of Hamburgh could not well understand the meaning of all this gathering together from the four corners of the land ; from either shore of the Baltic, and from where, with its broad belts, it girds and embraces the isles of Denmark. The notes of preparation had been sounded for months before, and occasional notices in the Journals of the day, when business gave them leisure to catch a glance at them, told of a coming of medical men and Naturforscher, but, as it did not relate to corn, sugar, or currency, they turned to something else and thought no more about it. But when the time arrived, and there was a talk of public attention to be paid to these strangers ; of the *Stadthaus*, being set apart for the place of enrolment and *rencontré ;* of the Boursen Halle, for the great mid-day assemblies ; of the *Apollo Saal*, for the *mittags essen* and the Soirées, and various other apartments, public and private, for certain minor *sectional* meetings as they were called ; above all, when it was whispered abroad that there was likely to be some good eating and drinking, some dancing too and music, and a *chance* that the citizens of Hamburgh might be called upon to pay for all this,—then—then to be sure, it became a matter of every day business with them, the stomach and the purse were equally concerned, and inquiries were neither few nor far between about the objects and intentions of all these strangers, and the probable expence they might cost them. You might hear the matter discussed over a shipping-list, or a newspaper, in the Boursen Halle ; over a sample of coffee, probably on the Exchange, or a beef steak in a restauration. " So many men come together to see one another, come so far merely to look at one another—nonsense !" And then said another, as he took up the thread of the affair, " They say we are to feed them, but if the senate spend our money in that way, the town will be about their ears ; the people will not stand it in these revolutionary times. When you or I go a travelling on our affairs to a strange place, nobody will think of treating us, and why should we treat these Naturforscher as they call themselves." " And I see," said a third, " why they elected old Bartels to be their president ; they thought he could manage best to squeeze a lot of good dinners out of us." Thus the wise ones talked, pushing their hands into their pockets every now and then to see if their purses were safe. But the judicious and thinking men, though they did not pretend to understand all the objects of the meeting ; though many of them were not qualified to appreciate them ; and though many could not regard with an auspicious eye this taking by storm as it were, and forcing light, and learning, and liberality, into the very sanctuary of Momus ; yet they thought, generally, that these strangers, being once within the walls, it would be for their own credit to use them well for a few days, when they would soon be off again.

The young men at the desk and the counter, as little instructed at least as their masters, caught another species of infection, and "what is a *Naturforscher?*" became the common question among them. And when, in the mornings, they repaired to the pavilions on the Alster, for their matitudinal cup of coffee, or in the evenings, when the letters were written, to sip their vespertinal glass of punch or sugar water, still the question was, "have you heard any thing about these *Naturforscher*, or what kind of fellows they are?" and then at the cry "*da geht ein Naturforscher*—there goes a Naturforscher," there was a hustling and a justling, a knocking over of chairs and tables, and a scrambling for hats, as every one hurried to the door to see what the animal was like, and if it walked on two legs or four on its way up the Jungfernstieg. These, and similar traits of naiveté, as they occasionally reached our ears, were a source of infinite amusement.

As it was impossible for one individual to attend more than two or three sectional departments, so it is impossible for one person, who has not more ample means of information than a stranger can be supposed to possess, to give an account of much more than what passed under his own immediate observation. In continuing my remarks, therefore, I shall throw what I have to offer in regard to the proceedings of the assembly into the form of a journal, which will enable me to give more easily, and with more appearance of method, several little notices which could not without confusion be introduced in any other way.

13th, 14th, and 15th—days of preparation and greeting. Every thing worth seeing in Hamburgh is thrown open to the Naturforscher during the ensuing ten days, and the strangers, formed into little parties, spend their time in visiting such collections and sights as best suit their dispositions and pursuits. In Hamburgh these collections, of a public kind at least, are neither numerous nor remarkable. That such should be the case was to be expected in a city wholly swallowed up in the pursuit of gain. And yet there are some private collections which would do honour to any town, and which do double honour to the individuals who have formed them. Of this kind is the well known and splendid mineral collection of Von Struve, the Russian minister, a collection which is now understood to be sold to the Russian government. This cabinet has cost Von Struve twenty-five years diligent collection, is especially rich in Norwegian and Siberian minerals, and contains 7000 or 8000 specimens, many of them finely crystallized, of great value and beauty. The pleasure I had derived from examining this collection during a former visit to Hamburgh was shared on my second visit in common with all the strangers who felt an interest in mineralogical science—the minister having kindly consented that the section of mineralogy should meet in his house. Inferior to that of Struve, yet deserving of mention as collected with zeal and in less favourable circumstances, is that of Pastor Muller, containing 2500 specimens. This collection I had not the pleasure of seeing.

Among eminent collections also must be particularly noticed the rich and extensive entomological cabinet of Mr Wilhelm von Wintem. This collection embraces the entire range of entomology, and possesses a degree of completeness in all its branches which is rarely to be met with. "Von Wintem is an exceedingly young man, and a merchant," said a young Swede to me, a zealous entomologist, "I cannot understand how he has been able to amass so splendid a collection." It would be the work of a lifetime at least in most countries and to most persons, but Hamburgh has communication with all the world, and the zeal of Von Wintem has known how to improve the advantages of his situation. No entomologist will visit Hamburgh without thinking of Von Wintem's collection, and they will find its possessor equally courteous in his attention and willing to contribute to their gratification. Professor Lehmann has also a general collection of insects, but it is less worthy of mention than the rich private collection of the Messrs Sommer in Altona, which, being almost within gun-shot, may be spoken of in the same paragraph with the collections in Hamburgh. This collection comprises only the Lepidoptera and the Coleoptera, but it is nevertheless reckoned one of the richest private collections in Germany. The best collection of birds is that of Mr Amsink, which is certainly splendid for a private gentleman and a merchant. It contains many fine birds in fine order, but its riches consist chiefly of European species.

The only other collection worthy of particular notice is the museum of Mr Röding. This museum consists of two subdivisions, containing natural productions and works of art, and is certainly a wonderful result of the patient and persevering industry of one private man, and he by no means rich. Röding, however, is rather a collector of curiosities than a scientific naturalist. There are indeed many birds—many fishes—still more shells—some quadrupeds—a few minerals—with anatomical preparations and various other things crowded in the natural history apartment, and all these are named and classified after some author, but no one department approaches completion. None of the different collections, except perhaps the shells, can even set up a claim to represent a department. Most of the specimens susceptible of the attacks of age, are also showing symptoms of decay, for while Röding has been advancing in years, his favourite collections have been growing old also, and unless some helping hand step in to his aid, the work of his whole life will not long survive himself. It would take a large sum to keep even this collection in good condition, and it would only show a proper and becoming liberality in the city of Hamburgh to purchase it from its highly meritorious and industrious collector, and by spending a little money in repairing, save from destruction so interesting a memorial of one of its worthiest citizens. Röding's desire for rarities is still unsatisfied, and the money he has to spare he expends rather in the purchase of new curiosities than in the reparation of the many he already possesses. The part which composes the works of art is more perfect, because less susceptible of decay, and is far more surprising as the work of a private individual than the natural history portion. His works in amber, ivory, silver, and wood, are both very rich and very worthy of being visited. The whole forms a kind of *Omnigatherum*, in which every one will find something to interest him, and with this view it is thrown open to the public once or twice a week at a trifling expence.

On one of these days I visited Dr Schmeisser, who gives lectures on chemistry in Hamburgh, and in whose auditorium the chemico-physical and pharmaceutical sections held its sittings. Dr Schmeisser is an old pupil and friend of the venerated Dr Black, and has many pleasing recollections of Edinburgh. It is exceedingly interesting to hear old men talk of the chemistry of their youth, and of the wonder with which every new discovery was regarded. "Soon after the discovery of the phosphuret of lime," said Schmeisser, "I was exhibiting its decomposition by immersion in water, and the spontaneous combustion of the phosphuretted-hydrogen formed—"We must have you German fellows sent out of the country," said a witty person to me, "or you will be setting the Thames on fire." And he told, with much glee, how, when the method had become newly known, he formed a quantity of artificial *spermaceti* from some half-decayed muscles by means of nitric acid, and making it into candles, sent some of them to Blumenbach, with a notice that they were prepared from the legs of a man who in his life time had done no good, and how Blumenbach punningly replied to him, "Mortui lucent qui in vita obscuri fuerunt." Poor Schmeisser, he has not been too fortunate in the world, and bad health confining him to his room, prevented his taking any share in the proceedings of the Naturforscher.

On the evenings of these three days there were *reunions* in the Hotel de Russie, where a large room had been secured for the purpose. During this time our numbers were but few; there was more quiet conversation, therefore, and less bustle and looking about for friends than when the numbers had become much greater. The only regret was, that one did not see among those assembled the persons he most anxiously looked for;—there were many eminent men—but chemists did not desire chiefly to see eminent botanists—nor did the pure zoologist care much for the presence of the mere practical surgeon or physician.

16th.—Dr Traill of Liverpool is the only Englishman yet arrived, and great disappointment is expressed that Edinburgh has sent forth so few. Dr Duncan has evidently been expected, and many inquiries have been made concerning him. Among the surgeons, there is a considerable desire to see some of our Edinburgh men of the knife and lancet. "Your surgeons in Edinburgh are very bold," said an eminent professor to me, the head of the Swedish school of surgery, "bolder than we want here on the continent. Your Lizars cares nothing for common operations; he likes only the most hazardous. He performs an operation very daringly and very cleverly—goes home at night—writes it out for the *Edinburgh Medical Journal*, and all is going on very well;—but the next number comes—and the patient—is dead!"

The strangers having now collected in considerable force, arrangements had been made for commencing the public dinners on this day. The directors had superintended the preparation of the Apollo Saal, and a suite of rooms connected with it for this purpose. Dinners, wines, and refreshments for the *soirées*, held from this time in the same place, were provided by the landlord of the Hotel de Russie, and the treasury of Hamburgh, notwithstanding the alleged complaints of a few individuals, had come liberally forward to defray certain expences unavoidable in fitting up such a place for such an occasion. The charge for dinner was fixed at two merks, about half a crown, for each person, exclusive of wine, and it was said that, to secure good dinners, the city gave something more. Had the rate been higher, the object in view, that of bringing the strangers as much together as possible, would have been defeated, as many would have preferred dining more quietly and more comfortably at a restauration, which they could have done for a good deal less. At four o'clock, we began to take places at the different tables, of which about eighteen were ranged up the middle and along the sides of the room, but so bad was the attendance, that before every one was served with wine and could boast of a plate of soup, at least a full hour had elapsed. I expected that we would have more regularity on the ensuing days, but on each succeeding one, as the numbers augmented, the noise and confusion, the running about, and the scrambling for places, increased to such a degree, that, when 500 or 600 assembled to dinner, it became perfectly intolerable. None were admitted but those who were members, and no ladies but such as were wives or sisters of members. Burgomaster Bartels presided at the principal table, and each of the directors had his place assigned him at one of the others. At the conclusion of dinner, it was announced by Dr Fricke, that such gentlemen as chose to spend the evening at the theatre, would receive tickets from the directors at a reduced price. Of this offer many availed themselves, the consequence of which was, that the *soirée*, by far the most pleasant of all our *reunions*, was, on this evening, unusually dull and insipid.

17th.—Among the arrivals this morning at the Stadthaus, were Berzelius, Oersted, Pfaff, Wiedeman, and many other eminent men whom all were glad to see, and old friends particularly, to meet again with kind greetings. An attempt was made during dinner to-day to drown the noise by the introduction of an excellent band of music, vocal and instrumental, which in some degree succeeded. But even this subjected us to another petty annoyance. During the interludes, parties of the performers went round the room with plates soliciting contributions, as any street-fiddler or ballad-singer might do. Such is indeed the custom in the caffés in Germany where music is found; the performers take their *chance;* but it ought to have been avoided on so particular and public an occasion as this; and whoever caused the music to be introduced, should also have caused it to be paid for.

The evening *reunion* passed off very pleasantly. There was a large assemblage—every one in a humour to please and to be pleased. A considerable sensation was created by the entrance and presentation of Oken, the founder of the society. It is very interesting to stand by and witness the various degrees of familiarity and pleasure with which, where so many meet, different persons recognize the same individual. Some at once shaking hands—others saluting—others waiting till they have made out the name of a man they have never before seen, and then bursting out into an exclamation of delight to be heard at the other end of the room.

18th.—This morning's was the last of our meetings at the *Stadthaus*, the regular session of the assembly commencing on the 18th, and the entire mornings during the session of eight days being taken up with the business of the various sections. In the morning, Dr Traill and I, with our countryman, Mr

Palk, drove out to the suburb St George, to pay a visit to the hospital or Krankenhaus.

Large and richly endowed institutions are not to be looked for in a free town whose territories include but a few miles of ground without the walls, and the greater part of whose revenue must necessarily be expended in keeping up a shadow of sovereignty and independent power; yet the hospital of Hamburgh, both for its magnificence and its general economy, would do honour to any city. The old hospital, or pest-house as it was called, having been burnt by the French in 1814, the present spacious building has been erected to supply its place. It was completed in 1823, at an expence of L. 75,000, and is intended to receive 1000, though it often contains 1200 or 1400 patients. It is situated in a fine airy and dry situation on the suburb of St George, and on the shore of the Lake Alster, from which it is supplied with water. The internal arrangements correspond with the outward appearance. The common wards are $40\frac{1}{2}$ feet by 24, with a height of 13 feet, and contain 13 beds, being at the rate of 972 cubic feet for each bed; the largest wards are 47 feet by 49, and contain 32 beds; and the entire number of rooms, with from 1 bed to 30, is about 200. Of these beds, 500 are set apart for medical, and 200 for surgical cases. The air is kept pure by common ventilators in the windows and in the floors of the upper story; and the possibility of stagnation is prevented by a spacious corridor of 10 feet in width, which runs lengthwise through the middle of the one floor and along each side of the other, into which the doors of the chambers open, and to which the air has at all times free access. There seemed much regularity in all parts of the house, and much subdivision of labour. One room, for example, was fitted up solely for the making of poultices, in which it was the business of one man to have them hot and ready at all hours. Another was set apart for the bandages, under charge of a person who was answerable for all it contained, and who kept a regular account of all his transactions with the different wards. The bandages of linen, flannel, &c. were all numbered and kept in separate dove-cots, ready of every length and breadth, at a moment's notice. The number of male and female nurses is from 80 to 90, and there are at least 50 other people constantly employed in various occupations connected with the establishment.

The vast number of patients is accounted for by a portion of each wing being set apart on the one side for male and on the other for female lunatics, who amount in all to about 300; and by the circumstance of its being an hospital for the support of incurable, as well as for the treatment of hopeful, cases. Patients of the former class, if allowed to accumulate, would very soon either destroy the efficiency of our hospitals, or swell them to a magnitude even greater than that of Hamburgh.

The chapel for divine worship struck me as an exceedingly commendable part of the institution. It is a large handsome room of 55 feet by 34, is 30 feet high, and has inclosed galleries. Divine service is regularly performed in this chapel on Sundays and holidays; and the sick are at other times attended to by the pastor of the hospital, who has a very respectable salary of 4500 merks, or L. 260 a year.

The salaries of the medical men differ in amount. The chief physician is allowed L. 380 a year, but he must live near the hospital, and is forbidden to practice. Three other physicians, for a daily visit of one or two hours, are allowed about L. 30 a year. The principal surgeon receives L. 120, and has his practice. Three surgeons also live in the house, one of whom must always be at hand. They are allowed about L. 30 a year and their board.

Crossing the Alster in a boat, we returned to the city by the Damm Thor, and reached the Boursen Halle soon after two o'clock, where we found the President Bartels delivering the inaugural discourse. It was short, friendly, unambitious, and without pretence,—a striking contrast to the splendid and elaborate oration of Tiedeman the preceding year at Heidelberg. "We have here in Hamburgh," said the worthy old man, "no rich museums and collections to boast of, such as you have met with in the metropolitan cities, and the seats of universities, where your former anniversaries were held; nor am I at all fitted for filling this chair after the many eminent scientific men by whom it has previously been occupied; but we shall only esteem you the more, and show you the greater kindness, that you have thus so honoured both the city and myself by your choice; and shall endeavour, by a reception worthy both of ourselves and you, to testify our sense of the important practical advantages to be derived from the prosecution and advancement of science."

Dr Fricke, as secretary, then read the laws of the society, which, as they may interest many of my readers who have never met with them in an English dress, I shall here transcribe.

1. At a meeting of German naturalists * and physicians held at Leipzig on the 18th of September 1822, it was resolved, that a society be formed, to be named the Society of German Naturalists and Physicians.

2. The chief object of this society is to afford an opportunity to the cultivators of natural science and medicine in Germany to become personally acquainted with each other.

3. Every person who has written upon natural science or medicine is admissible as a member.

4. The composition of a mere inaugural dissertation does not entitle any person to be considered as a writer.

5. A particular election is not necessary, and no diplomas will be given.

6. All persons are *admissible* to the meetings who employ themselves with natural science or medicine.

7. Only *members* have the right of voting at the meetings.

8. Every thing shall be decided by the majority of voices.

9. The society shall meet every year, and deliberate with open doors; to commence on the 18th September, and continue for several days.

10. The place of meeting shall be variable. At each anniversary the place of meeting for the ensuing year shall be determined.

11. A president and a secretary, resident in the place of meeting for the time being, shall conduct the affairs of the society till the ensuing anniversary.

* Deutscher *Naturforscher und Aertze*. We have no words in our language corresponding to these two. The former means a cultivator of natural science in any of its branches, being much more comprehensive than our word Naturalist, as generally understood; while the latter includes all cultivators of the healing art,—surgeons as well as physicians.

12. The president shall appoint the hours and place of meeting, and arrange the business, and every one who has any thing to bring forward must notify the same to him.

13. The secretary shall have charge of the minutes, the accounts, and the correspondence.

14. Both office-bearers shall subscribe only in the name of the society.

15. They shall make known as early as possible the authority conferred upon them by the immediately preceding assembly, and at the same time take measures for making the ensuing place of meeting as generally known as possible.

16. At each anniversary the office-bearers for the ensuing year shall be appointed. Should the appointment not be accepted, the office-bearers shall select another individual, and must at the same time appoint a new place of meeting.

17. Should the society lose one of its office-bearers, the survivor shall nominate another. Should it lose both, those of the preceding year shall resume their office.

18. The society shall form no collections, and, except its records, possess no property. Whatever is laid before them shall be again withdrawn by its owner.

19. The expences of the meeting shall be defrayed by the contributions of the members present.

20. These regulations shall remain unaltered for the first five years.

After the reading of the laws, and the list of members already arrived, the rostrum was occupied by Professor Struve from Dorpat, who delivered a long oration on the history, the importance, and the present state of astronomy. After magnifying astronomy above every other science that either was, is, or ever will be cultivated, he adverted to its history during the last hundred years. From this review he concluded, that during that time the main advancement of astronomy was due to Germany;—that at the present day Germany cultivated it most assiduously, and made the best astronomical instruments, —a circumstance we are supposed to acknowledge, by engaging Repsold of Hamburgh, (whom they dignify with the name of *immortal* Repsold,) to furnish a transit instrument for the

Edinburgh Observatory;—that after Germany Russia came next as a patron of astronomical science, by the building and equipping of observatories;—then follow England and Italy, France being lowest of all, having only two observatories at Paris and Marseilles. This discourse was neither judicious, nor, I believe, in general well received. No one science needs now-a-days to be exalted at the expence of others. Every man naturally ranks highest that particular branch of science to which he has dedicated himself; but he cannot expect to take other men along with him when he depreciates the departments to which they have with equal ardour addicted themselves. Nor is it necessary to drag in every name to exalt the scientific character of one country above that of other countries. Granting, as Sir James South has done in the Literary Gazette, that Germany deserves better of astronomy than England does,—yet why claim for that country the honour of names and labours which other countries will not concede?—"Why claim for Germany," said a Polish professor to me, "men who were countrymen of mine?" And though the Herschels, we may add, be of German extraction, their labours at least are English.

Professor Wendt from Breslau followed next, and read a memoir on Animal Magnetism. Of this paper there were various opinions. Some thought it very wonderful and very interesting,—a greater number thought it all sheer nonsense and delusion,—and some did not scruple to call the man a fool. "If the twentieth part of what he told us were true," said an eminent individual to me, "I would forgive him." But Wendt is known as one of the first medical men in Germany, and the author of many valuable medical treatises, of which I have a list before me of eleven published between 1803 and 1826. He professed himself to be no magnetiser, and therefore many thought him not only as a competent judge, but, as a man of honour, entitled to some degree of credit, when he related merely the effects he had seen exhibited through the agency of third parties. But no man *will* believe, nay *can* believe, the marvellous effects of the mysterious influence said to be evolved during the manipulations of this *science*, unless he have seen them with his own eyes; and yet it is hard to brand honourable men, who affirm they have witnessed them, with the epithets of fool and deceiver. Believers in the science are not fond of talking it over with the uninitiated; but, among the medical men of Germany, there are many secret converts who are only withheld, by the fear of ridicule, from openly avowing their faith. One of these I met with—an individual who had in one instance magnetized a patient—and certainly the details of the case were very extraordinary, and some of the more striking features of it attested by other medical men he had called in as witnesses. But he spoke of the power of magnetizing as a secret and almost sacred power of which he understood nothing,—which he had never employed but this once, though with success,—and which he never would employ again, except in some case of urgent, and otherwise hopeless, necessity. When one cannot assent to statements which appear incredible only, perhaps, because we have not had the same evidence of the senses as has brought home conviction to others, we ought at least to treat the judgments of honourable men with some degree of respect.

These two orations, followed by some announcements respecting future proceedings, closed the business of the first public sitting. The members then retired, the mineralogists, the botanists, the zoologists, &c. into separate apartments connected with the great hall, that the sections might be constituted, and choose their presidents. Berzelius was elected president of the chemico-physical section, and, on his declining, Pfaff was named in his stead, and Oersted, who had presided over the same section two years before at Berlin, took the office of secretary. The mineralogists chose Mons. von Struve, Russian minister to the Hanse Towns, to preside, and Mr Hartman of Blankenberg in the Hartz to be secretary. Counsellor Sachse from Ludwigslust in Mecklenburg Schwerin, was appointed president, and Dr Schmidt of Hamburg, secretary of the medical section. Dr Mertens of Bremen took the chair among the botanists, and Dr Siemers of Hamburg officiated as secretary. The zoologists adopted a more liberal, and perhaps a more considerate, plan. They chose Dr Leuckart of Heidelberg for secretary, and agreed to name a daily president, Professor Fischer of Moscow being appointed to preside at their first sectional meeting. These preliminary arrangements being made, we adjourned forthwith to the Apollo Saal to dinner.

As the first public day, this was one of the great days of the feast, and, but for the bustle, and confusion, and crowding, and the impossibility of procuring seats near those of your own science, or with whom you wished chiefly to converse—to which inconveniences I have already alluded—it would have been a delightful entertainment. The dinner was good and plentiful, the music from the orchestra was excellent, and a score or two of amateurs had lent their voices for the occasion, and, seated at one of the long tables in the middle of the room, entertained us during the *entremets* with some of their best German songs. Among these "Was * ist des Deutschen Vaterland?" was sung with great spirit,—a truly national song composed by one of their *great* poets called Arndt,—to use the language of a party of young Swedish students with whom I once spent a merry evening at Upsala. They had been singing our "God save the King," which is a great favourite in Sweden, when one of them remarked to me, "the song," I think, "was composed by one of your *great* poets called Brown."

This evening again the soirée was thinly attended, it having been announced that a new prologue in honour of the Naturforscher was to be delivered in the Theatre, and that tickets, as before, might be had at a reduced price.

19th.—This day being Sunday, there were no public meetings; but it had been previously arranged by the office-bearers, that it should be spent in an excursion of seven or eight miles down the Elbe. The Booths, two young Scotchmen, proprietors of a large botanic garden and nursery grounds at Flottbeck in the Danish territory, five miles from Hamburg, had handsomely invited the whole body of Naturforscher to stop in passing, view their gardens, and partake of a *dejeuner à la fourchette*, from which we were to proceed to the grounds and gardens of Mr Bower, occupying one of the finest and most romantic situations to be met with on the banks of the Elbe.

* The reader will find this song with an English translation towards the latter part of this paper.

To see these grounds is a favourite Sunday excursion with the Hamburgers, to whom they are thrown open on that day on payment of one merk, about 15d. From this charge the Naturforscher were on the present occasion to be exempted.

At half-past nine A. M. the Naturforscher might be seen making their way from all parts of the city to the Nicolai Kirchhofe, where the Polizei had provided a large assemblage of carriages of all descriptions, droshkies, barouches, and open holsteins to convey the party, and, to prevent imposition, had already fixed and intimated the fare, (2 merks) which each person was to pay, for the entire excursion. Thus as they arrived they formed themselves into parties, and each party put in requisition the carriage which suited them best, paid their *merks* in advance, took note of the number of their vehicle, to prevent confusion on the return, and drove off merrily to Flottbeck. It was a fine morning, and the entire day continued delightful—a charming contrast to the perpetual rains which rendered almost every day disagreeable in Hamburg during the past summer. All this on a Sunday in Germany is mere matter of course, and, therefore, nobody made the slightest remark on the subject, either as regarded ourselves or the troops of men we passed here and there busily repairing the roads.

It was a fine sight as we drove along the rich and cultivated country, with here and there pleasant grounds and country houses, and an occasional peep of the Elbe on the left, sparkling through the trees—to see a long line of open carriages of all descriptions,—interminable before and behind, crowded all with happy faces, enjoying and anticipating enjoyment, with ladies head dresses appearing now and then—a sort of *point de vue*, among the dense grove of hats—and throwing a still more cheerful air over a scene which merry hearts, a bright sun, and a fair land, contributed all to enliven.

The garden of the Booths proved well deserving of a visit, and the breakfast was arranged and went off in a style, not only highly creditable to themselves, and, as we Britons thought, to their country, but to the satisfaction and admiration of all present. The garden is very rich in plants of all countries, cultivated for sale in great numbers. Among these were reckoned 12 species of Dryandriæ, 30 species of Banksiæ, 70 varieties of Camellia, near 400 of Pelargoniæ, and 800 of roses, making alone many thousand specimens, arranged according to the natural orders. The nursery was equally rich in trees of every description. The hot-houses, *Kalthauses* as the Germans call them, are extensive, one of them glazed on both sides, being 200 feet long. One of the greatest curiosities exhibited was a model of the *Rafflesia Arnoldi*, taken from the well known cast in the possession of the *Horticultural Society of London*, and which deservedly attracted universal attention. The Booths gained great credit by their attention to the Naturforscher, and it is to be hoped that their repeated kindness to the botanical section will only make their establishment better and wider known, and secure it more extended patronage.

After an hour spent at Flottbeck, we drove again in cavalcade four miles further to the garden, or more properly the ornamented grounds of Mr Bower. The walks here were delightful, and laid out with great taste; and the view of the Elbe from the rising grounds was one of the finest which the banks of the river any where afford. An ornamental tower and Chinese pagoda, erected on two elevated spots, and commanding a fine view, were objects of great attraction to the party; but on repairing thither, we found ourselves, with the majority of our friends, shut out. Mr Bower chose to open them only to a select few—a prohibition which he regretted when too late, and which, with one or two other trifling things of the same sort, thought to show more of the narrow-mindedness of the Hamburg merchant than any one then and there expected, obliterated any slight feeling of obligation we should otherwise have felt to Mr Bower for the privilege of walking in his grounds.

Having separated from my party, and joined some other friends, I found, on repairing to the gate, that I had wearied out the patience of my fellow voyagers, and that the carriage of which I was a shareholder had gone off without me. Fortunately I found three distressed Germans in a similar condition, and after walking a couple of miles, we succeeded in discovering their vehicle. This brought us all back to the Apollo

Saal by 5 P. M. in time to take part in the usual feeding operations, which on this day, from many being delayed longer even than ourselves, were carried on more quietly, and with less crowding than on either of the preceding days. The evening, as Sunday evenings often are in Germany, was spent by the nimble ones of the Naturforscher in dancing with the fair Hamburgesses; music being provided, and a room fitted up for the occasion behind the dining-room in the Apollo Saal. Each thus had his mode of amusing himself at his own choice—those who chose danced in the ball-room—those who liked tobacco and strong waters partook of their segars and punch in the smoking-room—and those who chose none of these things, betook themselves to a quiet confabulation in the apartments where it was forbidden either to smoke or dance.

20th.—This morning the different sections met to discuss matters connected with their several sciences. They were arranged as follows:—

1. The section of mineralogy met from 8 to 10 in the morning, in the house of his excellency M. von Struve, the Russian minister.

2. The botanical section, from 10 to 12 in the morning, in the house of Professor Lehmann.

3. The section of zoology, zootomy, anatomy, and physiology, from 8 to 10, in the anatomical hall of the Kurhaus.

4. For *practical medicine*, in the Boursen Halle at the same hour—from 8 to 10. This section had also occasional meetings in the evening.

5. For physics and chemistry, from 10 to 12, in the auditorium of Dr Schmeisser.

6. The pharmaceutical section, afterwards formed and presided over by the venerable Trommsdorf of Erfurt, whose journals of pharmacy have been long known, and his system of pharmacy so much esteemed in Germany, met in the same place from 12 to 1.

By this arrangement, had any one wished it, he could not easily have attended more than two sections, except on alternate days, and the hours could not have been otherwise or more conveniently arranged. From 12 to 2 was dedicated to seeing sights—visiting the hospital and other institutions, or examining collections, and was the only leisure time that could be so employed. At two o'clock the general public meeting took place in the Boursen Halle—at four, dinner was in waiting at the Apollo Saal—and again between dinner and the evening *reunion*, you might have an hour or two to dispose of for any purpose of your own.

It is not my intention to give any detailed account of the proceedings of the several sections. For this no one individual can be qualified, simply because it is impossible for him to be present to witness them; and a mere list of papers read, and subjects discussed, which forms the substance of the report drawn up by the secretaries of the sections, would possess little interest for the general reader. In the *Isis* only are these papers given at any length; but Professor Oken possesses advantages over even the secretaries of the sections, in the willingness of every one to furnish copies or abstracts of their papers to the father and founder of the society. And though much interesting matter is at times brought before the sections, yet the communications thus made, form neither the main object of these yearly assemblies, nor the most important of the benefits to be derived from them. Men learn to know, to esteem, and better and more justly to estimate each other—jealousies are removed—friendships are formed—and thus personal rivalry—harsh language and controversial sparrings are diminished in philosophical writings; "for you cannot," said Oken to me, "so harshly speak of or condemn in so unqualified a manner the theoretical speculations or experimental results of a man with whom you have held agreeable personal intercourse, as we are too prone to do of those whom we have never seen or conversed with."

I attended the mineralogical and chemico-physical sections. The proceedings of the former consisted chiefly in the exhibition of new, rare, or beautiful minerals, and of some optical instruments by Professor Marx. Few papers were read, and of these few some were unworthy of the place. Among these was one on primitive formations, by Menge, the well known mineral-dealer from Lubeck, who had also, by some means or other, contrived to convert the place of meeting into a shop for the sale of minerals.

The chemico-physical section was more worthy of the time and place. At its meetings many interesting notices were given, and a few important subjects discussed. There also, however, some rather lengthy specimens of trash were inflicted upon us, especially towards the end. Most of the time, indeed, was taken up by inferior men, as is to be expected where they form so decided a majority.

The botanists, I believe, went on very smoothly. They are an enthusiastic class of men, and captiousness or feelings of personal dislike probably discover themselves less frequently among them than many other tribes of naturalists. There is little in their science, indeed, to call such forth,—it is all beautiful,—a roaming among flowers,—and requiring little deep thought, few disappointments are met with in the study, and something more than the science, therefore, is to blame when a botanist's equanimity is disturbed.

The zoologists (in number 52) also, from all I could learn, were generally well satisfied with each other, and with their labours. The only case of discontent or dislike with the proceedings of which I am aware, was that of Professor Leuckart of Heidelberg, the secretary of the section, who fretted himself, and endeavoured to disturb others, about a matter in which he should rather have cheerfully acquiesced. To this I shall have occasion to advert when I come to speak of the proceedings of the last public day.

The medical section was the most numerous, and the discussions occasionally assumed a very animated character. The great amount of business made it necessary to have occasional meetings in the evening. There, too, as in the other sections, some papers would have been willingly dispensed with; and a very general dissatisfaction was expressed, fortunately in the absence of the author, at the double reading, first in English and then in German, of a lengthy paper on the non-contagious nature of the yellow fever, by the only American who attended the meeting. Whatever the merits of a paper might be, indeed, it was rather too much to occupy two hours with it, when the whole time the section could command for transacting all its affairs could hardly exceed twelve hours. When alluding to the yellow fever, I cannot help jotting down a very good New York pun told me by Dr Jamieson of Baltimore, the author of the above paper. " A countryman walking along the streets of New York, found his progress stopped by a close barricado of wood. ' What is this for,' said he to a person in the street. ' Oh that's to stop the yellow fever.'—' Aye! I have often heard of the *Board* of Health, but I never saw it before.' "

Of the pharmacologists I heard nothing. Under their president, Trommsdorf, they discussed tinctures and electuaries; and people seemed to think it was soon enough to have to do *with* them when they could no longer do *without* them.

In the Boursen Halle, at two o'clock, Professor Oersted of Copenhagen first addressed the meeting, in a long discourse on the application of mathematics to physical science.

Of Oersted I have given some interesting notices in a former paper on the " Scientific Men and Institutions in Copenhagen." * I shall here add a slight sketch of his career. In 1799, he took his degree of Doctor of Philosophy, and the following year began to lecture as a *privatim docens* on metaphysics, to which his mind retains still a decided inclination. In the same year, he was named Adjunctus Lector of pharmacy in the medical faculty. The three following years he spent in travelling through Germany, Holland, and France, and returning in 1804, began to lecture on physics and chemistry. The history of the chair of physics in the university of Copenhagen is rather curious. In 1736, it was suppressed by Christian VI. and an additional professorship of Divinity instituted in its place. At the same time it was ordered that a professor of medicine or mathematics should give lectures on physics. Accordingly, Professor Krutzenstein of the medical faculty gave lectures for thirty years, and dying in 1795, was succeeded by Aasheim, also professor of medicine, who died in 1800. Professor Bugge of astronomy was then appointed to lecture on physics, and in 1806, Oersted was appointed professor extraordinarius. In this year Zeise, now professor of chemistry, became his first pupil, and under his care commenced a course of study, which, afterwards extended and completed in France under Chevreul, promises at no dis-

* See this *Journal*, New Series, Vol. iii. p. 1.

tant period to yield him a reputation honourable alike to himself, to his talented instructor, and to the university of which he is a member. Besides his lectures on physics, his proper department, Oersted has at different times lectured on chemistry, principally on general principles, or what he calls the philosophy of chemistry, and on electro-chemistry, and his lectures have been much esteemed and numerously attended. The heir-apparent, Prince Christian Frederick, has frequently honoured him by his presence, and he has also lectured in German and French to the diplomatic body. The Society for the Diffusion of Natural Science founded by Oersted, and patronized by Prince Christian, has organized a system of popular lectures, not only in Copenhagen, but also in other towns of Denmark. These lectures in the capital are delivered by Oersted, Zeise, and Forchhammer, are open to all, and command an attendance of 60 or 80 auditors.

Oersted's experimental are far more valuable than his theoretical memoirs. "Il fait des belles experiences," said a German doctor to me, "c'est ce qu'il fait bien—mais quand il ecrit—nons ne trouvons ordinairement que des phantasies." This expression is no doubt much too strong; but it shows the general opinion of his tendency to speculation.

To Oersted's philosophical memoir succeeded a sort of nondescript essay on the tides, by Professor Willebrand of Giessen. He laboured to show that the theory of lunar attraction was not sufficient to account for the phenomena. He considered them to be caused by some unintelligible principle of *circulation*, which he invited the members to discuss with him in the section, or in the steam-boat, during a proposed trip to the island of Heligoland. But I believe most people were so perfectly satisfied with what they heard from himself, that they never thought of introducing it in the physical section; and on board of the steam-boat, most of the *inlanders* found themselves so much occupied with another kind of *circulation*, that they had no leisure to attend to that of Professor Willebrand.

Professor Pfaff, of Kiel, next came forward, and in an extempore discourse of a lively, humorous, and interesting kind, spoke of the application of chemical analysis to vegetable substances of every-day consumption, adverted then to the peculiar principles found in coffee, a substance so generally used,—and exhibited some beautiful pure white crystals of caffeine, which he recommended to practical physicians as likely to prove valuable in medicine as a mild febrifuge. He exhibited also a new caffeic acid which exists in the coffee in combination with lime and magnesia, and to which is owing its peculiar aromatic smell. This address, enlivened with many witty remarks, gave general satisfaction, being intelligible, not only to all the members, but to the auditors, male and female, who crowded the galleries.

Caffeine is generally supposed to be a discovery of Pelletier, but its true discoverer was Runge, a young professor of chemistry at Breslau in Silesia. Several years ago this young man published a book, in which he described various new principles obtained from vegetable substances, and, among others, also from coffee; but the book was written in so peculiar a style that very little attention was paid to it. The substances described were also often impure, so that the properties he attributed to those which he obtained, are not always to be found in the purer substances since prepared by others; yet still the honour of the several discoveries, and of making the first steps in this interesting field, is due to Runge;—he should not, therefore, be forgotten in the history of the science. Runge was at the meeting in Hamburg, which, I believe, is his native place—a true specimen of the German student—long lank hair—a careless free manner—fond of his pipe, his friend, and his bottle of beer. He exhibited in the physical section, the results of a long-continued and elaborate examination into the chemical nature of various natural orders of plants, gathered in the different months of the year, and their reactions with the metallic salts, those of copper, tin, iron, bismuth, lead, as shown in their colouring powers upon cotton cloth. The changes that take place in the juices of plants from the first months of spring to the end of autumn, as exhibited in the change of their colouring properties, was very striking and very interesting.

The dinner to-day was crowded and uncomfortable. A diversion was created after we had finished our coffee by a cry of fire; and curiosity led many even of the Naturforscher to the spot. It proved to be a large building in the centre of the city, which was entirely consumed. The regulations for fires, as they must necessarily be in so crowded a city, are very strict. A double guard is called out, and none are allowed to approach the spot. Should any one contrive to force his way in, a bucket is immediately put into his hand, and he is set to work.

21st.—On the breaking up of the sections to-day at noon, all the Naturforscher adjourned to the botanic garden to partake of an elegant *dejeuner*, prepared at the expence of the good city of Hamburg. The breakfast at the Booths had no doubt given occasion to this,—the city could not be out-done by two private individuals. On a pleasant slope facing the city, and having hot-houses on either hand, were erected two large tents, gaily and tastefully ornamented with flowers in festoons, and garlands of all descriptions, in which were set out two long tables groaning under eatables of every kind and flavour, pleasing at once to the eye and the palate, and liquids of every strength from the French *eau de vie* to the lightest claret. The one tent was monopolized by the ladies, the botanists, and the zoologists; the other, into which I happened to stroll, was the resort of the heavier, but not the duller, men of the mineralogical and physical sections. Not that this separation was strict; it was only general, and probably accidental, for we had with us Chamisso of the botanic garden in Berlin, a poet, botanist, and traveller, who accompanied the expedition of Kotzebue round the world, a most amusing, witty, and cheerful man. After a short time, the champagne began to flow among us, and presently came the drinking of toasts, and hobnobbing, and making of speeches, and bandying of wit, and roaring of laughter, to such a degree that the sober ones wondered, and the merry ones came to share, if possible, in the amusement. Pfaff and Chamisso were the leading men in this display of wit and humour. And though some of the grave ones shook their heads at what they were pleased to term our riotous behaviour, yet I look back to that hour as one of the happiest I spent in Hamburg.

The botanic garden of Hamburg is in high order, and does much credit to Professor Lehmann, who superintends, and has formed it. It was established so late as 1821, and is already one of the richest in Germany. Dr Lehmann gives lectures on botany, and Oldendorf, the managing gardener, has a school of practical gardening—a species of institution which might very well be connected with our botanical gardens in this country. It would not only train up a race of practical gardeners well instructed in botany, but might also be so managed as to cause not only a material saving, but probably an actual increase to the funds of the institutions.

Two o'clock P. M. saw us again assembled in the Boursen Halle. Dr Simon of Hamburg first addressed us in a long prosy oration in praise of natural science and medicine, which was by no means well received. It is a pity that the office-bearers should not have some controlling power over the papers brought before the general meetings, that a proper and worthy selection might be made to be read in public, that men of talent might not be condemned to sit by hundreds, listening to *delirations* spun out by the hour, and by men of no reputation; while at the same time they have the mortification to think that such exhibitions go forth to the world as specimens of what so grave, and learned, and philosophic a body can do. I have spoken of the general simplicity of the Hamburgers in matters of science; and yet, even among them, I learned after the meeting was over, that the impression had gone forth, that the transactions of the public sittings, to which only they were admitted, were in general unworthy of a society of such high pretensions. A similar idea seems to have entered into the mind of Tiedeman, for, at the meeting in Berlin in 1828, he proposed that such a power of selection should be intrusted to the office-bearers and certain others. After a long discussion, however, the motion was negatived by a small majority of eighteen, it being supposed by many to interfere with the general liberty and equality of all.

Next came on the appointment of the place of meeting in 1831. The subject was introduced by Count Sternberg of Prague, who expressed the wish of the imperial government, that the society should assemble in the ensuing year at Vienna. After some little discussion this was agreed to, and Baron Jacquin of Vienna was appointed to the office of president, and Von Littrow to that of secretary.

I have repeatedly spoken of presidents; but "we have no presidents," said Oken to me; "no man above another. We are all on an equality; we have a first and second *geschäftsfuhrer*, to manage the business for us, but it is no elevation."—" It is nothing of which another need be jealous," said a botanist to me; " the newly appointed geschäftsfuhrer feel, I daresay, nothing elevated." So it is the custom of some to talk of these official situations, and yet it is a high honour nevertheless,—and an honour to be proud of,—and one which is felt as such, as well by those on whom it is bestowed, as by those who think themselves unjustly passed over. And, disguise the name as we may, in what age, or in what country, would it not have been an object of ambition—a laudable and praiseworthy object—to preside at the meetings, and to direct the deliberations of four or five hundred of the most learned and intelligent men of the time?

These meetings were for some years an object of jealousy to the German rulers, and their proceedings were carefully watched during several successive anniversaries before they paid them any outward attention. Learned professors, it is said, were sent to the assembly, not as spies of course, but merely to bring home intelligence from so interesting an association! The minister felt much interested in the advancement of scientific intercourse, and was anxious to hear what passed at these large assemblies. If his friend, Professor ———, would like to go, he would procure him a grant of money to defray his expences. The professor, a man after his own heart, jumped at the proposal, went to the meeting, and came back eagerly to satisfy the minister's *amiable curiosity*. One hears such stories occasionally when sitting *tête-â-tête* with a German naturalist, during the intervals of puffing a segar, or sipping a glass of punch; but it is chiefly the young men who are indiscreet enough to tell them, not having yet experienced how necessary it is to have the fear of arbitrary power continually before their eyes.

That the German rulers now patronize these meetings, is an evidence that their former jealousy was without foundation, and that science alone is sought to be promoted by these comings together. Yet even now a species of unfelt, perhaps, yet nevertheless, real control and *surveillance*, are exercised over them by the governments of the places to which they are invited. The king and his ministers agree to invite the meeting to their chief city; "but we must have professor so-and-so for president, and Dr so-and-so for secretary, and then we can keep all things right." Accordingly, a deputation of three or four persons is sent to the meeting—they deliver their commission—and, having made out a good case, the thing is agreed to. Then one of these men gets up and proposes another of them as a fit person to be president—a third rises and suggests that the last speaker be appointed secretary, and the matter is carried of course; for, besides the delicacy felt in regard to *personal* opposition, it is understood that these individuals have the confidence of the government, and will be able to do most for the reception and entertainment of the assembly.

The president and secretary have the sole and entire disposal of all the time of all the members during the appointed days of meeting; he who guides the president, therefore, moves all the others like so many puppets. Such control is the necessary consequence of their connection with, or dependence upon, men in power, *where power is regulated and checked by no constitutional law*. So long as they met in small cities as an independent body, unaided and unnoticed by those whom political power or wealth only had made great, they had a perfect control over their own "sayings and doings." But the German princes have found a sure way of taming the lion they feared when he arose among them shaking his mane; they have thrown him a sweet sop, and he has swallowed it, and laid him down to sleep. For the sake of mere natural science, perhaps, it is as well that it should be so; but why should men of science be gagged?—the lights of their age set "under a bushel?"—that they shall be permitted to congregate in this or that city, but shall be forbidden to hold colloquy on subjects the most intimately connected with the welfare of their race? Such restraint is not heard of or seen, yet it is secretly felt and laboured under by all. A naturally open or bold man, in some moment of excitation, shakes it off; but when he cools down, he feels surprise at once, and regret for his momentary rashness, sensible that now he has subjected himself to a suspicion that will cling to him for years, retard his advancement in life, and follow him wherever he goes.

In illustration of this, they tell a story of Prince Metternich or *Mitternacht*, (Midnight,) as the punsters call him in Germany. The Emperor, *it is said*, heard often, and saw many accounts of these meetings, and expressed his surprise that they were not resorted to by men of science from Vienna. One of the maps engraved for the use of the members, and containing only the names of the places from which individuals had come to the meeting, was brought to him, and he was nettled that his capital was not even mentioned in it. Supposing it to be the want of funds which kept his professors at home, he intimated that funds should be provided from the treasury for defraying their expences. On the approach of the next meeting, accordingly, several individuals applied for passports to the director of police. "Well, Doctor, you want a passport? What are you going to do at ——— ?"—"I am going to the Naturforscher Versammlung."—"Oh, you are going to this meeting, too, are you? But what do you think the minister will say to it? You know he dislikes all these meetings."—"He can have no objection surely, when his majesty has expressed a wish that we should go, and has granted money to defray our expences."—"Very true, very true, but I would recommend you to think better of it. You may have your passport if you choose, but I would advise you as a friend not to go. You are a candidate for so-and-so, and you are very likely to have the appointment; but, should you give offence, ————————"—"Il est comme un Roi ce *Mitternacht*," said a Halle man to me.

This story I have heard repeatedly, and it does not appear at all incredible; but whatever may have been the former feelings of the court and ministry of Vienna in regard to these meetings, it is certain that every thing will be done in September next to make the anniversary of 1831 an era in the history of the society.

After the nomination of the office-bearers, a discussion arose as to the most proper way of announcing to learned men the place of meeting, &c. for the ensuing year,—whether by particular and private letters from the president and secretary, or by general and public announcement. This matter was at length entrusted to the discretion of the geschäftsfuhrer.

It had been announced at the meeting of yesterday that, for the entertainment of those chiefly who, living in the interior, might *never have seen the sea*, or sailed down the Elbe, a trip to Heligoland had been projected; and that the Rotterdam company had placed a large steam-boat at their disposal for this purpose. To-day, it was intimated that those who intended to go must be on board by five o'clock to-morrow morning; and that there would be no public or sectional meetings for the three days it was intended the party should be absent. This announcement gave to many great dissatisfaction. It was kindly meant by the directors, but it was injudicious thus to separate the Naturforscher into two bodies; and, for half the time they were to be in Hamburg, to prevent them from holding communion with one another. Not more than half the number of strangers availed themselves of the opportunity of seeing the sea; and meanwhile the other half were left to employ themselves as they might. But the true *inlanders* rejoiced at the proposal, and it was amusing to hear their grave and earnest inquiries about the nature and mode of operation of the *see krankheit* they had so often heard of, and were now destined to experience.

The party sailed the first day to Cuxhaven, where they spent the night uncomfortably enough I believe; the second was spent in Heligoland, and they reached Hamburg again on the third day. There were few who on their return could not speak feelingly enough of the *see krankheit;* and some found one day on shore little enough to restore them to their propriety. I did not accompany the expedition, but Professor P——— of Edinburgh, has furnished me with the following lively account of the sufferings and privations it had to undergo.

" It would require a better memory or a more poetical imagination than mine, to infuse interest into an account of the excursion to Heligoland, or make it worthy of any but a very brief notice. Scientific interest it had none; for though we had Enke and Moll on board, and other less distinguished Astronomers, the bearings of the rock we were bound to had already been laid down too accurately, to give them even a pretext for making new observations: and the Geologist, though he might pick up from the needy natives a few *cornua ammonis* and *belemnites*, had little to glean, by his own industry, in the mass of loose friable sandstone, deeply tinged with a ferruginous red, which composes the island. And what could the Zoologist do, where the greatest variety, in his way, was the governor's cow—sole specimen of the genus Bos to be found in Heligoland. The Botanist, indeed, if he happened to be one of those *mediterranean* Naturalists who had never before seen the sea, was evidently filled with astonishment at those wonders of the vegetable world which you regard at Portobello with such stoical indifference; and loads of sea tangle and *fuci* are, I doubt not, now reposing in glass-cases in the interior of Germany, differing in no respect from those which we barbarously burn into kelp on the shore, or spread over our fields as manure.

" Nor can I say that the social pleasures of this expedition quite compensated for the want of scientific interest. A steamboat is not the best place in the world for making or cultivating new acquaintances, particularly where the majority of the party are Germans, and without any infusion of French vivacity; and when many of those best able to amuse and instruct were at one time suffering from the nausea of a first voyage, at another frightened out of their propriety by a breeze of wind and a swell of the sea, which must have been alarming enough to novices. Nor was there much on shore to make us forget the lugubrious aspect of things on deck. For want either of previous arrangement or of fit accommodation on the island, the party, which might amount to seventy or eighty, did not meet to dine together, but were scattered, in little knots of ten or twelve, often strangers to one another, over all the houses, private and public, of the village. This, I believe, was generally felt as but a poor compensation for the roar and merriment of the Apollo Saal at Hamburg: for though, even there, you and I may have missed those after-dinner speeches, which, in our country, do sometimes nobly redeem the clatter of knives and forks, and give an intellectual character even to a city feast, yet there was much to prize in the joyousness, good humour, and mutual kindliness that seemed to animate the whole company. Nor shall I readily forget the energy and intensity of feeling with which the songs, whether Bacchanalian or Patriotic, were sung in this assembly of *savans.* One of the latter class of songs pleased me so much, that I amused myself, during the dreary parts of the naval expedition, with turning it into hobbling rhymes. The original, however, I must say, is not much better, as far as the verse is concerned: it was the thought, and the enthusiasm it excited, rather than the measure, that delighted me. I send you the original to remind you of it, and add my own version.

Which is the German's Fatherland?
Swabia, perhaps, or Prussia's sand?
Where on the Rhine the wine-flood streams?
Or round the Belt the sea-bird screams?
  Oh no! not so:—an ampler space
  The German's bounding line must trace.

Which is the German's Fatherland?
Is't Pomerania's barren strand?
—Where "Munich all her banners waves?"
—Or Time and Conquest Austria braves?
      Oh no! not so, &c.

The German's country shall we seek
Where climbs the Swiss the Glacier peak?
Where high Tyrol her mountains piles?
Or Stiria's Alpine desert smiles?
  Oh no! these countries please me well,
  But German land must mightier swell.

Where then can be this Fatherland,
That knits its sons in filial band?
What is the silken cord that binds
In mutual love so many minds?

"Where'er is heard the German tongue,
"And German hymn to Heaven is sung,
"Whate'er the clime—the kindred—be,
"That land—that land is Germany."

Then blest be thou, from age to age,
Land of the Hero, Bard, and Sage!
Still loyal be thy sons and true,
Worthy the stock from which they grew!

Still foremost to pronounce the vow,
With fervent hearts, as *we* do now,
(Recorded let it be on high)
For Thee to live, for Thee to die! *

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* Was ist des Deutschen Vaterland?
Ist's Preussenland? Ist's Schwabenland?

"Having thus brought you back to the Hall of Apollo from the barren rock which you were lucky enough not to be banished to, I have only to regret, &c. &c."

22d. This day I spent with Dr Traill in a visit to Harburg, on the Hanoverian side of the Elbe. We crossed over in a steam-boat which plies regularly; and after wandering for some hours among the sand-hills beyond the town, collecting flints with impressions of shells, and other organized substances, which are by no means rare, we were brought back again to Hamburg

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Ist's, wo am Rhein die Rebe blüht,
Ist's, wo am Belt die Möve zieht?
O nein! nein! nein!
Sein Vaterland muss grösser seyn!

Was ist des Deutschen Vaterland?
Ist's Baierland? Ist's Steierland?
Gewiss, es ist das Oesterreich,
An Siegen und an Ehren reich!
O nein, &c.

Was ist des Deutschen Vaterland?
Ist's Pommerland? Westphalenland?
Ist's, wo der Sand der Dünen weht?
Ist's wo die Donau brausend geht?
O nein, &c.

Was ist des Deutschen Vaterland?
So nenne mir das grosse Land!
Ist's Land der Schweizer? Ist's Tyrol?
Das Land und Volk gefiel mir wohl!
O nein, &c.

Was ist des Deutschen Vaterland?
So nenne endlich mir das Land!
"So weit die deutsche Zunge klingt
"Und Gott im Himmel Lieder singt!
"Das soll es seyn!
"Das, wack'rer Deutsche, nenne Dein!"

Das ganze Deutschland soll es seyn!
O Gott vom Himmel, sieh' darein,
Und gieb uns ächten, deutschen Muth,
Dass wir es lieben, treu und gut!
Das soll es seyn!
Das ganze Deutschland soll es seyn!

by six o'clock in the evening. One curious circumstance I may mention in regard to the sand-hills around Harburg. Here and there among the brown sand occur small white spots of two or more feet in diameter, which are carefully dug out for household purposes. These spots penetrate the hills to a considerable distance, like large solid pillars, and they are followed in the process of excavation by shovels with handles ten or twelve feet long. The only one I saw in the act of being dug out was inclined to the horizon at an angle of perhaps 50°; but I did not learn whether such be their general directions. It would be difficult to assign any satisfactory reason for these singular deposits.

The soirée in the Apollo Saal this evening was quiet and pleasant, and I spent a couple of very agreeable hours with Professor Berzelius of Stockholm.

23d. The sections this day met as usual, having agreed, on seeing their own strength yesterday at dinner, to resume them even during the absence of the Heligolanders. In the chemical section Professor Pfaff endeavoured to show that the generally received theory regarding the developement of electricity by induction is erroneous, and that of two conductors brought near each other, if the first be positive, both extremities of the second are positive also. I regretted very much that I could not follow his language on this very interesting and very important subject, which the experiments of Biot were supposed formerly to have settled, but on which those present best qualified to judge were inclined to agree with Pfaff. It is to be hoped that a memoir on the subject may before this time have been published by him in some of the German journals.

The tower of St Michael's Church was to-day a place of considerable resort. It is 456 feet high, and gives a distant view over the Elbe and the surrounding flat country. No one can have any conception how the city of Hamburgh is packed together, unless he resort to some such elevated spot, where he can look down upon the limited space which daily and nightly confines 106,000 souls. This tower of St Michael's is interesting as the place from which Benzenberg in 1803 made his first experiments on the diurnal motion of the earth. But here it is well known he obtained no good results, from the constant presence of currents, which disturbed the true descent of the falling body. He was obliged, therefore, to have recourse to deep mines, in which he found that a heavy body in falling actually deviated from the perpendicular by a quantity agreeing very nearly with the formula of Laplace.

This evening also there were comparatively few at the soirée, and the ladies in the dancing room looked anxiously but in vain for partners, and it was really melancholy to see them all sitting so solitary and forlorn.

24th, The Heligolanders returned this evening, and many of them joined us in the Apollo Saal, but many also found it better to remain at home and recruit.

25th, This was the last day of the meeting, and many persons whose time was limited had already gone. The sectional business was entered upon as usual by all parties, and the animal and plant men exhibited the spoils with which their visit to the sea had enriched them.

At two the final assembly took place in the Boursen Halle. Professor Fischer read the first address, being an account of the botanic garden at St Petersburg. This garden, at present in so flourishing a condition, is entirely the work of Professor Fischer. Before his appointment there was a place called a botanic garden, containing at most 600 species. It now boasts upwards of 12,000. From Persia, Caucasus, Armenia, and Siberia, it has received great accessions—while M. Riedel, the botanist who accompanied M. Langsdorf to Brazil, has lately brought home upwards of a thousand living plants from that country. They have been preserved by the method already known in England, of planting them in pots, and rearing them so on the spot where they are indigenous. By this means their preservation is far more effectually secured than when they are dug up in the woods and sent on board before they have had time to take root.

Professor Fischer was succeeded by the secretaries of the several sections, who read to the assembly an outline of what had been done in each of the sectional departments. On reading his report of the proceedings of the zoological section, Professor Leuckart took occasion to animadvert in a few ill-natured words on the appointment of Englishmen to preside in that section: "It is the first time," said he, "that a foreigner, who did not understand the language, has been appointed to preside at a meeting of German naturalists." I know not what particular spite the worthy secretary could have against either Mr Gray or Dr Traill, the two gentlemen on whom the honour was conferred; but it was evidently spite, or ill-feeling of a similar kind,—for both of our countrymen knew something of the language, and even had they not, it would have been only consistent with that true courtesy which distinguishes the Germans, but which the professor seemed to have lost on his way from Heidelberg, to have dignified these foreigners, for their country's sake merely, with this *horary* honour.

The bad taste and bad feeling of Leuckart's allusion was generally felt, and Dr Siemers, who rose next to read the report of the proceedings of the botanical section, took the opportunity of inserting a few words, which made ample amends for all that had passed. In the name of the botanical section, he then proposed that the meeting should send a letter to the East India Company, returning thanks to that body for the munificent gifts of Indian plants which they had made to all the celebrated botanists in Europe, and to pray that Dr Wallich might be allowed to remain longer in England, to carry on the work he had begun so splendidly, and which no one was so well qualified to finish. A letter embodying the latter request was also proposed to be sent to the king of England, as likely to influence the Court of Directors, and copies of both, as drawn up by a committee of the botanical section, were read to the meeting. After some discussion, it was agreed to refer the matter to a committee, by whom the letters were ultimately dispatched. It is to be hoped that the Court of Directors will accede to the wishes of a body of men so capable of pronouncing correctly on the merits and labours of Dr Wallich.

A medico-philosophico-physico-juridical essay was then read by Counsellor Stierling of Hamburg, and a proposal made by Dr Stintzing, also of Hamburg, for the publication of an Encyclopædia or Journal of Science by the Society, neither of which gave rise to any observations. The business was now finished, and the President Bartels, after a short address, pronounced the anniversary for 1830 to be at an end. This was the signal for the *ex-president*, who in this case was Professor Tiedeman of Heidelberg, to rise and deliver the usual address of thanks to the town and authorities of Hamburg, for their kind treatment and general attention. This address was received with great applause.

The dinner table to-day was unusually crowded; many Hamburgers had been admitted, and all the adjoining apartments were put under requisition. The music and the songs, and the mere eatable part of the dinner, were of the best description, but nothing could reconcile me to the noise, crowding, and confusion, and to the necessity of sitting in a side room, and among men one had never seen before. After dinner some toasts were given, and the only speech I heard during all these feastings was by the lively Pfaff of Kiel, who, after lauding the city and its trade, concluded by proposing, "The apothecaries of Hamburg, who had contrived to change *chemistry* into *alchemy!*"

The whole affair was finished off at night by a splendid ball in the large room, hitherto devoted to feasting; and the beauty of Hamburg was all assembled to grace the departure of the Naturforscher. The dancers kept it up till a late hour, while the punch and segar men in their own apartment seemed equally unwilling to break up their pleasant fellowship. But they dropped away one by one, and the crowd of scientific men whom Saturday saw squeezing each other in the press, talking loudly, or joining with enthusiasm in the chorus of a patriotic song, were seen on Monday—solitary, silent, and far apart,—scattered to the four winds of Heaven.

Such is a general view of the proceedings of the Society of German Naturalists at their ninth anniversary. To me it proved exceedingly interesting. If I have been able to infuse a tithe of this interest into the above account of it, my readers will not regret that I should have spun it out to so many pages. It was said not to have been so splendid as that of Berlin, but this was owing to the locality, not to the members. Of strangers, there came to Hamburg 258,—a number nearly as great as met together at Berlin; and where the chief object is to see and to learn to know men, *their* presence is sufficient. So I found it, and I shall never regret my visit to Hamburg on this occasion, which gave me an opportunity not only of becoming acquainted with many men I had never before seen, but also of meeting with persons I had formerly learned to know and esteem, but whom I might otherwise never have had the pleasure of meeting a second time.

The first object of these meetings is to promote this acquaintance and friendly personal-intercourse among men of science; but other great and perhaps more important benefits grow spontaneously out of them. They draw public attention to science and scientific men, and make people inquire concerning both them and their pursuits. They exalt science in general estimation, and with it those who devote themselves to its advancement; and, above all, they spur on the governments of the different states to examine into and ameliorate the condition of their scientific institutions; and to seek for men of true science to fill the chairs of public instruction. Such and similar benefits have already resulted from the meetings in Germany. Might not similar results in our own country be looked for from a similar institution?

PORTOBELLO, *23d February* 1831.

ART. II.—*Project for facilitating the Manufacture of Achromatic Object-Glasses for Engyscopes.* By C. R. GORING, M. D. &c. Communicated by the Author.

I AM not in the habit of recommending things to the public which have not been thoroughly tried; nevertheless, I shall venture in the present instance to suggest a scheme of ameliorating and perfecting combined object-glasses of *short foci* purely from theoretical considerations.

Every optician who has turned his attention to the construction of object-glasses with large angles of aperture, must then have felt, that the state in which it is most easy to produce them is that of over-correction, both for sphericity and dispersion. I have little doubt, that a workman could at once hit off an object-glass in this state as easily as he could the figure of the metal of a reflecting telescope with an hyperbolic curve.

I have examined a great number of double object-glasses of

Chevalier's construction, and many others, and find three out of four to be in the condition I have alluded to, viz. with the concaves too powerful for the convexes, both in point of spherical and chromatic aberration, even with very moderate apertures.

Now it is known, that, in order to obtain a focal pencil of large dimensions, we are compelled to combine as many as three double object-glasses together. This complication is a necessary evil, unless we choose to dispense with seeing the more difficult kinds of lined objects. Moreover, light and aperture are always *acceptable*, (when not absolutely necessary,) if we examine opaque objects with high powers.

Proceeding, therefore, on the supposition, that we must have *three object-glasses* to have a perfect engyscope, *my project consists in supplanting one of the three by a common uncorrected lens of suitable focus, figure, and material*, caused to act against the excess of aberration which I suppose to be left in the concaves of the other two.

I do not mean to assert that we might in all cases be able to assume two over-corrected achromatics, and then make a simple convex which should be able to neutralize their aberration; but I think it would be perfectly feasible to assume the convex, and to make two achromatics accommodate themselves to their vulgar neighbour's humours, if the latter is unable to adapt himself to suit theirs. In the case of a triple object-glass we see one concave capable of reducing two convexes to a state of complete subjection,—Why should not two concaves master three convexes? Indeed, I cannot help thinking myself, that *one* highly over-corrected *double* achromatic might be combined with a common lens with vast advantage, and that this sort of triple object-glass might be made equal in power and aperture to two double achromatics, and greatly superior to a triple object-glass of the ordinary construction in the correction of the direct and oblique pencil, as well as in the aperture it would sustain. See Fig. 1 and 2 of Plate II. the former being a triple, and the latter a quintuple object-glass of the new construction.

It was ever my wish to contrive some sort of object-glass which should of itself, without the assistance of any other, do all that might be required of it; and, with the exception of four or five objects of the lined kind, this sort of object-glass would perform completely well. The lines on the scales of the *Pieris brassicæ*, of the *Podura plumbea*, and a still more refractory species from the King's cellar at Windsor, together with those on the feathers of some of those small brown moths which infest our clothes, seem almost without any parallel in nature, and have tormented opticians to death to make engyscopes with apertures capable of exhibiting them, which are in a manner of no absolute use for any other purpose. I think an object-glass with an aperture of about 25°, well corrected, will show any other objects, save those mentioned, which by the by are perfectly visible with any common equi-convex lens of large aperture and short focus, nearly as well as one with an aperture of 50°, and therefore may be considered effective for all ordinary purposes.

It must not be supposed that I am insensible to the highly meritorious labours of Mr Lister, who, in his paper on the *Improvement of the Achromatic Compound Microscope*,* has given us a most splendid example of the extent to which experiments, scientifically conducted, may be made to supplant more rigorously demonstrative science; but his method of correction seems to me only applicable to object-glasses of *moderate focal length, which do not require to be placed in contact, to give room for the application of objects.* Mine is peculiarly adapted to those of short foci placed close together.

LAMBETH, *Nov.* 25, 1830.

P. S.—When I wrote the above paper, I had no means of putting my method of correction to the test of experiment; but having since procured a couple of over-corrected object-glasses from Chevalier, of about $\frac{4}{10}$ of an inch focus, I combined them with a common equi-convex lens of crown glass of $\frac{1}{2}$ inch focus, *placed in front of them next the object*, and found that this composition was still a *little over-corrected*, both for colour and spherical aberration with the naked aperture of the glasses. I am persuaded that a lens of a little shorter focus would have produced an exact correction. *One* of the aforesaid object-glasses also combined with an equi-convex lens of plate-glass of one inch focus placed in front gave a very good correction both for colour and spherical aberration, so that I

* *Philosophical Transactions* for 1830, Part I. p. 187.

think no doubt can be entertained of the feasibility of my scheme ; and, when I consider that the two lenses were the first that happened to come to hand, and that the curves of the object-glasses of Chevalier's construction are all alike, so that no error can well be committed except in the quality and thickness of the glass employed in their manufacture, I should hope that the construction of object-glasses for engyscopes will become a very simple matter.

LAMBETH, *Jan. 25th*, 1831.

ART. III.—*On the Phenomena and Laws of Elliptic Polarization, as exhibited in the Action of Metals upon Light.* By DAVID BREWSTER, LL. D. F. R. S. Lond. and Edin. Concluded from last Number, p. 165.

IN order to give a general view of the number of points of restoration, and of the other phenomena which take place after different numbers of reflexions, I have drawn up the following Tables.

TABLE I.—Showing the numbers of reflexions from silver at which elliptically polarized light is restored to a single plane of polarization, with the corresponding angles of incidence, and the position of the plane of restoration in relation to the plane of reflexion, computed for 20 reflexions.

(For angles less than the maximum polarizing angle.)

No. of Reflexions.		Integer Multiples.	Angle of Restoration.	
			°	′
— 2	2	+4—6+8—10+12—14+16—18+20	71	0
$2\frac{1}{9}$	2.111	+19 - - -	71	42
$2\frac{1}{8}$	2.125	+17 - -	71	32
$2\frac{1}{7}$	2.143	+15 -	71	23
$2\frac{1}{6}$	2.167	—13 - -	71	8
$2\frac{1}{5}$	2.200	+11 - -	70	48
$2\frac{2}{9}$	2.222	—20 -	70	34
$2\frac{2}{8}$	2.25	—9+18 - -	70	17

	No. of Reflexions.		Integer Multiples.	Angle of Restoration.
	$2\frac{2}{7}$	2.286	—16	69°53′
	$2\frac{2}{6}$	2.333	+7+14	69 29
	$2\frac{3}{8}$	2.375	—19	69 3
	$2\frac{2}{5}$	2.4	—12	68 59
	$2\frac{3}{7}$	2.428	—17	68 33
	$2\frac{1}{2}$	2.5	—5+10—15+20	67 54
	$2\frac{4}{7}$	2.571	—18	67 14
	$2\frac{3}{5}$	2.6	+13	6 58
	$2\frac{2}{3}$	2.667	—8+16	66 25
	$2\frac{5}{7}$	2.714	+19	66 0
	$2\frac{3}{4}$	2.75	—11	65 45
	$2\frac{4}{5}$	2.8	—14	65 23
	$2\frac{5}{6}$	2.833	+17	65 9
	$2\frac{6}{7}$	2.857	—20	65 0
+	3	3	+6+9+12+15+18	63 43
	$3\frac{1}{6}$	3.167	—19	62 29
	$3\frac{1}{5}$	3.2	—16	62 15
	$3\frac{1}{4}$	3.25	—13	61 15
	$3\frac{1}{3}$	3.33	—10	61 20
	$3\frac{2}{5}$	3.4	—17	60 53
	$3\frac{1}{2}$	3.5	—7+14	60 15
	$3\frac{3}{5}$	3.6	—18	59 38
	$3\frac{2}{3}$	3 667	+11	59 13
	$3\frac{3}{4}$	3.75	—15	58 42
	$3\frac{4}{5}$	3.8	+19	58 15
—	4	4.0	+8—12+16—20	57 16
	$4\frac{1}{4}$	4.25	+17	55 54
	$4\frac{1}{3}$	4.333	+13	55 29
	$4\frac{1}{2}$	4.5	—9+18	54 42
	$4\frac{2}{3}$	4.667	—14	53 54
	$4\frac{3}{4}$	4.75	—19	53 31
+	5	5	+10+15+20	52 27
	$5\frac{1}{3}$	5.333	—16	51 5
	$5\frac{1}{2}$	5.5	—11	50 27
	$5\frac{2}{3}$	5.667	—17	49 49
—	6	6	+12—18	48 38
	$6\frac{1}{3}$	6.333	+19	47 23

	$6\frac{1}{2}$	6.5	—13	-	-	-	46 57
	$6\frac{2}{3}$	6.667	—20	-	-	-	46 32
+	7	7	+14	-	-	-	45 35
	$7\frac{1}{2}$	7.5	—15	-	-	-	44 13
—	8	8	+16	-	-	-	43 0
	$8\frac{1}{2}$	8.5	+17	-	-	-	41 52
+	9	9	+18	-	-	-	40 51
	$9\frac{1}{2}$	9.5	—19	-	-	-	39 51
—	10	10	+20	-	-	-	39 0
+	11	11		-	-	-	37 15
—	12	12		-	-	-	35 50
+	13	13		-		-	34 33
—	14	14		-		-	32 30
+	15	15		-	-	-	32 15
—	16	16		-		-	31 17
+	17	17		-		-	30 30
—	18	18		-	-	-	29 42
+	19	19		-	-	-	28 56
—	20	20		-	-	-	28 10

TABLE II.—Showing the numbers of reflexions from silver at which elliptically polarized light is restored to a single plane of polarization, with the corresponding angles of restoration, and the position of the plane of restoration in relation to the plane of reflexion, computed for 20 reflexions.

(For angles greater than the maximum polarizing angle.)

No. of Reflexions.			Integer Multiples.				Angle of Restoration.
—	2	2	+4—6+8—10+12—14+16—18—20				73 0
	$2\frac{1}{9}$	2.111	—19	-		-	74 9
	$2\frac{1}{8}$	2.125	+17	-	-	-	74 18
	$2\frac{1}{7}$	2.143	—15	-	-	-	74 28
	$2\frac{1}{6}$	2.167	+13	-	-	-	74 44
	$2\frac{1}{5}$	2.200	—11	-	-	-	75 0
	$2\frac{2}{9}$	2.222	—20	-	-	-	75 12
	$2\frac{1}{8}$	2.256	+9+18	-	-	-	75 26
	$2\frac{2}{7}$	2.286	—16	-	-	-	75 43
	$2\frac{2}{6}$	2.333	—714	-	-	-	76 2

No. of Reflexions.		Integer Multiples.	Angle of Restoration.
$2\frac{3}{8}$	2.375	+19	76°19′
$2\frac{2}{5}$	2.4	—12	76 33
$2\frac{3}{7}$	2.428	+17	76 44
$2\frac{1}{2}$	2.5	+5+10+15+20	77 13
$2\frac{4}{7}$	2.571	—18	77 38
$2\frac{3}{5}$	2.6	—13	77 48
$2\frac{2}{3}$	2.667	—8+16	78 38
$2\frac{5}{7}$	2.714	—19	78 23
$2\frac{3}{4}$	2.75	+11	78 33
$2\frac{4}{5}$	2.8	—14	78 47
$2\frac{5}{6}$	2.833	+17	78 57
$2\frac{6}{7}$	2.857	—20	79 4
— 3	3.0	+6—9+12—15+18	79 40
$3\frac{1}{6}$	3.167	+19	80 37
$3\frac{1}{5}$	3.2	—16	80 24
$3\frac{1}{4}$	3.25	+13	80 34
$3\frac{1}{3}$	3.333	—10+20	80 50
$3\frac{2}{5}$	3.4	—17	81 2
$3\frac{1}{2}$	3.5	+7+14	81 19
$3\frac{3}{5}$	3.6	—18	81 35
$3\frac{2}{3}$	3.667	—11	81 45
$3\frac{3}{4}$	3.75	+15	81 57
$3\frac{4}{5}$	3.8	—19	82 8
— 4	4	+8—12+16—20	82 30
$4\frac{1}{4}$	4.25	+17	82 58
$4\frac{1}{3}$	4.333	—13	83 16
$4\frac{1}{2}$	4.5	+9+18	83 23
$4\frac{2}{3}$	4.667	—14	83 38
$4\frac{3}{4}$	4.75	+19	83 45
— 5	5	+10—15+20	84 5
$5\frac{1}{3}$	5.333	—16	84 27
$5\frac{1}{2}$	5.5	+11	84 38
$5\frac{2}{3}$	5.667	—17	84 48
— 6	6	+12—18	85 6
$6\frac{1}{3}$	6.333	—19	85 22
$6\frac{1}{2}$	6.5	+13	85 30
$6\frac{2}{3}$	6.667	—20	85 36

— 7	7	+14	-	-	-	85 49
$7\frac{1}{2}$	7.5	+15	-	-	-	86 7
— 8	8	+16	-	-	-	86 21
$8\frac{1}{2}$	8.5	+17	-	-	-	86 35
— 9	9	+18	-	-	-	86 46
$9\frac{1}{2}$	9.5	+19	-	-	-	86 56
—10	10	+20	-	-	-	87 5
—11	11		-	-	-	87 20
—12	12		-	-	-	87 35
—13	13		-	-	-	87 46
—14	14		-	-	-	87 56
—15	15		-	-	-	88 4
—16	16		-	-	-	88 11
—17	17		-	-	-	88 18
—18	18		-	-	-	88 24
—19	19		-	-	-	88 28
—20	20		-	-	-	88 33

The first column of the preceding Tables shows the smallest number of reflexions at which a pencil of elliptically polarized light is restored to a single plane of polarization at the angle contained in the last column; and consequently the half of these numbers is the number of reflexions at which light is elliptically polarized at the same angle. Thus at three reflexions the ray is restored to a single plane of polarization at 63° 43′, and 79° 40′, and consequently at $1\frac{1}{2}$ reflexion it is elliptically polarized at that angle. This is easily understood when the number of reflexions is an integer; but it requires some explanation when the number is partly fractional. It has been already stated, in page 151, that elliptical polarization may be completed at any fractional part of a reflexion; and since it begins to be restored the instant the polarization is complete, and again begins to be elliptically polarized after every restoration, the points of restoration may take place in the middle of a reflexion; and though we cannot possibly examine what takes place at these points, yet the effect must appear when the fractional number of reflexions in the first column has been repeated so many times as to become a whole number. Thus a ray elliptically polarized by $1\frac{1}{3}$ reflexion will be restored to a single plane at $2\frac{2}{3}$ reflexions at the same angle. It will also be restored at $2\frac{2}{3} \times 2 = 3\frac{1}{3}$, and at $2\frac{2}{3} \times 3 = 8$, in which case its restoration will be seen at the eighth reflexion at the same angle; and also at the sixteenth and twenty-fourth, &c. In this case the phase P will be $\frac{90°}{1\frac{1}{3}} = 67\frac{1}{2}°$, $R = 33° 45'$, and $\varphi = 11° 15'$, from which we deduce the angles of incidence to be 63° 43′, and 79° 40′. In order to ascertain the existence of these points of restoration, I made the experiment at five and seven reflexions as multiples of $2\frac{1}{2}$ and $2\frac{1}{3}$, and I found the angles to be for five reflexions 68°, and for seven reflexions 70°, in place of 67° 54′, and 69° 29′, as computed from the formula.

The numbers in the third column, with the signs + and —, are the integer multiples of those in the first column, and show the number of reflexions at which the elliptically polarized light is restored, the numbers being carried the length of twenty reflexions. The sign + shows that the plane of the restored ray is to the right, and the sign — that it is to the left of the plane of reflexion. In order to determine the sign of the restored ray, we must consider that in the same quadrant the signs necessarily alternate. Now at 73°, the maximum polarizing angle, the signs are —2, +4, —6, +8, —10, +12, &c.; and I have also found that all the integer numbers in column 1st, Table I. have their signs + or positive, as +3, +5, +7, +9, &c., and all the even numbers their signs — or negative, as —4, —6, —8, —10, &c.; whereas in Table II. all the integer numbers are negative whether odd or even, thus, —3, —4, —5, —6, &c. By setting out, therefore, from these points, and attending to the alternation of the signs, it easy to determine for any number of reflexions its proper signs, whether it is a multiple of an integer or of a mixed number.

In order to illustrate this Table, I have projected, in Fig. 3, Plate II. some of its results as far as six reflexions. The concentric arches II, II II, &c. represent the quadrant of incidence for one, two, &c. reflexions, B being the point of 90°, and C that of 0° of incidence. The point D or the line A D is the point or line of maximum polarization, viz. 73° for silver; and the figures 1, 2, 3, 4, 5, &c. show the points or nodes, and their distances from C, the angles of restoration. The loops or double curves lying between the points 1, 2, 3, are drawn to give an idea of the intensity of the elliptic polarization, which has its minimum at 1, 2, 3, &c. and its maximum at intermediate points. These points of maximum intensity do not bisect the loops, or are not equidistant from the minima 1, 2, &c.; but such is their relation to them, that the maximum for n reflexions is the minimum for $2\,n$ reflexions corresponding to the same angle. Thus the maximum for one reflexion, viz. 73°, is the minimum for two reflexions; and the maxima for two reflexions, viz. 82° 30′ and 63° 43′, are the minima for four reflexions. The maximum may be found directly by computing the angle of incidence, which corresponds to a phase intermediate between the two minima, within which the maximum lies.

Having thus determined the various points of the quadrant, at which elliptic polarization is produced, and at which it is destroyed, after any number of reflexions; and also the position of the plane of the restored ray, I shall proceed to investigate the cause of those brilliant complementary colours which accompany these phenomena.

As all transparent bodies have different values of their maximum polarizing angle, appropriate to the index of refraction for each colour of the spectrum, it is reasonable to suppose, that, as elliptic polarization is effected at the maximum polarizing angle, this angle would vary for the differently coloured rays. That this is the case may be easily proved by observing the angles of restoration for homogeneous light after two reflexions. In silver the difference of the angles for red and blue light is about 5° in the sun's rays; so that calling 73° the maximum polarizing angle for the mean yellow ray, the angle will be $70\frac{1}{2}$° for blue, and $75\frac{1}{2}$° for red light. Hence if we examine a pencil of white light twice reflected at $70\frac{1}{2}$°, and place the principal section of the analyzing prism in the plane —39° 48′, the blue rays will disappear and the red will remain visible. In like manner, at an angle of 75° 30′ the red will disappear and the complementary blue will be visible; while at an angle of 73° the yellow will disappear, and red and blue will be seen together, one on each side of the place where the yellow has vanished. At angles of incidence greater than $75\frac{1}{2}°$ and less than $70\frac{1}{2}$, and also at intermediate angles, the blue or the red will still predominate in the pencil, the blue being in excess at all angles greater than 73°, and the red in excess at all angles less than 73°. Such are precisely the phenomena which take place, as will appear from the following Table.

Angle of incidence of the two Reflexions.	Colours with ordinary Light.
63	Very pale yellow, growing whiter at less incidences.
64	Pale yellow.
65	Pale saffron yellow.
66	Saffron yellow.
67	Paler orange yellow.
68	Orange yellow.
69	Reddish orange.
70	Tile red.
$70\frac{1}{2}$	Vermilion red
71	Scarlet.
72	Bright pink.
73	Dark pink.
74	Deep China blue.
75	Indigo.
$75\frac{1}{2}$	Pure bright blue.
76	Paler blue.
77	Whitish blue.
78	Blueish white, growing white at greater angles.

It is obvious from what has been already stated, that with homogeneous yellow light the pencil will not vanish in passing from 73°, where it is evanescent, to 90°, and 0°, where it is also evanescent; but the intensity of the extraordinary pencil of the analyzing rhomb will increase from 0° to half the reflected light, from 73° to $82\frac{1}{2}°$, and from 73° to 57° 16′, and will decrease from the same points to 90° and 0°. The same is true of the red and blue rays, the former having its maximum intensity at an angle greater than $82\frac{1}{2}°$ and greater than 57° 16′, and the latter at an angle less than $82\frac{1}{2}°$ and less than 57° 16′.

In order to ascertain the phenomena in homogeneous light, let us suppose that polarized yellow light suffers four reflexions from silver, and let us consider what should take place in the loop 2, 3 of the quadrant IV, IV. (See Fig. 3.) At the node 2, or 73°, the inclination of the restored pencil is + 31° 52′, and at the node 3, or 82° 30′, it is—37° 22′, and the point of maximum between 2 and 3 is at 78° 8′. If at 73° we place the principal section of the analyzing prism in the plane +31° 52′ the extraordinary ray will vanish, and the light will pass into the ordinary image; and if at 82° 30′ we place it in — 37° 22′, the same effect will be produced. At 74° a small portion of light will pass into the extraordinary image, and this portion will gradually increase to 78° 8′, the principal section of the prism having been turned round gradually from +31° 52′ to 0°, as described in page 140. The ordinary and extraordinary images now approach most to equality, and they vary in intensity according to the same law in passing from 78° 8′ to 82° 30′, the axis of the prism having now come into the plane—37° 22. The very same phenomena take place with red and blue light, only the points of restoration and the maximum occur at different angles of incidence, so that the spaces between the minima have different lengths for the differently coloured rays. These spaces or loops, therefore, will overlap each other, as will be understood from Fig. 4, where they are shown separately, r r' being the red loop, y y' the yellow, v v' the violet one, the points r, y, v, r', y', v' the minima or nodes, and a, b, c, the maxima. When these loops are viewed superposed as when they form white light, then the tint in the extraordinary image will be white, minus the three quantities of light that have disappeared from the extraordinary ray. At the line m n, passing through the node of the red loop, the red will have vanished, and the mixture of the yellow and the violet which remains will constitute a greenish blue pencil, decreasing in its blue tint towards a, and becoming pink, and then red towards t s, in consequence of part of the light of the other red loop above r now passing into the extraordinary ray. At v and at v', where the violet disappears, the mixture of the yellow and the red will form an orange pencil, which will be reddest at v and v', and shading off to white at a. At the line s t the yellow vanishes, and across the upper part of the luminous disc, there will be light with an excess of red, and across the lower part of it, light with an excess of blue. This takes place with even numbers of reflexions; with odd numbers the blue light is uppermost and the red undermost.

The phenomena of colour, as seen by white light, vary greatly with the number of reflexions, both with respect to the depth of the colours themselves and the rapidity of their changes. In order to investigate the nature of these variations, let us consider what will take place at 2, 4, 6, 8, and 10 reflexions from silver in the loops above and adjacent to 73° the maximum polarizing angle. The following are the numbers which regulate the phenomena.

Fig. 6.	No. of the Reflexions.	Nature of the Reflexions.	Limits of the Loops.	Length of the Loops.	Inclination of the Plane, or ϕ.
a b	2	First of the series	73°— 90° 0′	17° 0′	39°48′
c d	4	First of the series	73 — 82 30	9 30	37 22
e f	6	Multiple of 3	73 — 79 40	6 40	32 25
g h	8	Multiple of $2\frac{2}{3}$	73 — 78 8	5 8	27 53
m n	10	Multiple of $2\frac{1}{2}$	73 — 77 13	4 13	24 16

This Table may be illustrated by Fig. 5, where A B passes through the incidence of 90°, and C D through that of 73°, the points *m*, *g*, *e*, *c*, *a* corresponding respectively with the incidences of 77° 13′, 78° 8′, 79° 40′, and 82° 30′, or those at which the ray is restored by 10, 8, 6, and 4 reflexions. The curvilineal spaces *a b*, *c d*, *e f*, *g h*, and *m n*, are the loops already referred to, whose breadths represent the intensity of the extraordinary ray, which is a minimum at the nodes *a*, *c*, *e*, *g*, *m*, and *b*, *d*, *f*, *h*, *n*, and reaches its maximum near the middle of the loops.

If the image reflected from the silver is a circular disc of white light of a given magnitude, then by two reflexions at 73°, or at the point *b* the extraordinary image will be red above and blue below, when the principal section of the analyzing prism is in the plane — 39° 48′ ; but these colours will be very faint, as the disc occupies but a small part of the loop *a b*. The disc indeed may be made so small, that the extraordinary image will entirely disappear in this loop. In this case the ordinary image will be white, as all the reflected light will pass into it. At four reflexions the loop $c\ d$ is little more than one-half of $a\ b$, and consequently the light will vary much more rapidly from d to the maximum. When the analyzing prism has its principal section in the plane $-37^\circ\ 22'$, the extraordinary image at c will be coloured with red light above and blue below; and when it is in the plane $+31^\circ\ 52'$, the extraordinary image at d will be similarly coloured: The colours will be much brighter than in the case of two reflexions, and consequently the extraordinary image will not vanish. The consequence of this is, that the ordinary image is not white as before, but yellow, because a considerable portion of red and blue light are left in the extraordinary image.

As the number of reflexions increase, and the loops $e\ f$, $g\ h$, &c. diminish, the disc will occupy a greater proportion of the whole loop, and the red and blue colours with which it is crossed grow brighter and brighter, and come closer and closer to their line of junction in the middle of the disc. Hence a greater quantity of red and blue light is left out of the ordinary image, which on this account becomes yellower and yellower, and at last of a greenish hue.

In order to determine the position of the principal section of the analyzing prism, when the extraordinary image is a minimum for any angle of incidence α, and any number of reflexions, let ψ, $\chi =$ the inclinations of the plane of polarization of the restored ray at the nodes a, b; m, $n =$ the inclinations or values of φ in the formula $\tan \varphi = \frac{\cos (i + i')}{\cos (i - i')}$ suited to the angles of incidence at the nodes; $x =$ the inclination φ suited to the incidence α.

Now it is obvious that at the one node, the position of the principal section of the analyzing prism, when the extraordinary image is a minimum, is $+\psi$, and that it gradually changes to 0° and then passes to $-\chi$, thus undergoing a change equal to $\psi + \chi$, while the inclination φ varies by a quantity equal to $m - n$. Hence calling I the inclination of the principal section to the plane $+\psi$ at the angle of incidence α, we have $m - n : \psi + \chi = m - x : \mathrm{I}$.

Hence $I = \psi + \chi\left(\frac{m - x}{m - n}\right)$

When $x = n$, $I = \psi + \chi$.

When $x = \frac{m - n}{2}$, $\frac{m - x}{m - n} = \frac{1}{2}$ and $I = \frac{\psi + \chi}{2}$.

When the nodes of the loop are on different sides of the maximum polarizing angle, which happens only in the middle loop of 3, 5, 7, &c. reflexions, then m and n have opposite signs, and consequently their difference is $m + n$, and, as in this case $m = n$, the formula becomes $I = \psi + \chi\left(\frac{m - x}{2\,m}\right)$.

It is impossible to determine the relative intensities of the ordinary and extraordinary image at any angle α, because this must depend on the relative intensities of the pencil by whose interference the elliptical polarization is produced. In silver these pencils approach to equality, but in steel and other metals they are very unequal.

Having thus shown how to determine the phenomena of elliptic polarization for any angle of incidence, for any number of reflexions, and for homogeneous light of any colour, I shall conclude this paper with some observations on a very remarkable anomaly which has presented itself in the course of this inquiry.

The phenomena which have been described, indicate very clearly that the angle of maximum elliptic polarization for one reflexion, or the angle of restoration after two equiangular reflexions, is the maximum polarizing angle of the metal, and consequently that its tangent is the index of refraction, as shown in the following Table.*

* This Table completely proves that the refractive index of metals cannot be deduced from their reflective power; for silver, which surpasses them all in reflective power, stands very low in refractive power. Mr Herschel has noticed the difference between the indices of refraction deduced by these two methods in the case of mercury, which he makes 5.829 as given by its reflective power, and 4.16 as given by its polarizing angle. He makes the index for steel 2.85. When we consider that metals reflect the light that enters their substance, it must be obvious that the quantity of light which they reflect is a function not only of their refractive power, but of their transparency, which will be proportional to the intensity of the reflected pencil that has entered the metal. If this is the case, the transparency will be proportional to the inclination of the plane of the restored

Names of Metals.	Angles of Maximum polarization.	Index of Refraction.
	° ′	
Grain tin - -	78 30	4.915
Mercury - -	78 27	4.893
Galæna - -	78 10	4.773
Iron pyrites -	77 30	4.511
Grey cobalt -	76 56	4.309
Speculum metal -	76 0	4.011
Antimony melted -	75 25	3.844
Steel - -	75 0	3.732
Bismuth - -	74 50	3.689
Pure silver - -	73 0	3.271
Zinc - -	72 30	3.172
Tin plate hammered	70 50	2.879
Jewellers' gold -	70 45	2.854

This conclusion is not opposed by any of the phenomena, when we consider merely the mean refrangible ray to which these numbers refer: but when we use homogeneous light, a very strange anomaly occurs. The maximum angle of elliptic polarization for red light in the case of silver is 75° 30′, and for blue light 70° 30′, giving

		Angle.
		° ′
Index of refraction for red light,	3.866	75 30
——————— mean ray,	3.271	73 0
——————— blue light,	2.824	70 30

the order of the refrangibilities being inverted.

The perfect similarity between the action of metals, and the total reflexion of the second surfaces of transparent bodies, promised to throw light upon this difficulty. I accordingly examined the formula of Fresnel for total reflexion, where the phase P is thus expressed:

$$\text{Cos P} = \frac{2\,m^2\,(\sin i)^4 - (m^2+1)\,(\sin i)^2 + 1}{m^2 + 1\,(\sin i)^2 - 1}.$$

From this formula it follows that when $m = 1.51$, and $i =$ 54° 37′, P will be 45° for one reflexion, and consequently for

ray after two reflexions at the maximum polarizing angle, and the order of the transparencies of the different metals will be that of the Table, p. 144. See Mr Herschel's *Treatise on Light*, § 594, 845.

two reflexions $2\,P = 90°$. If m increases as it does for blue light, then the phase will be 45° at an angle of incidence above 54° 37′, that is, the circular polarization of the pencil will take place at a greater angle of incidence for blue than for red light, which is the reverse of what takes place in metals. Upon making the experiment, however, with total reflexion, we shall find that the blue rays are circularly polarized by two reflexions at a less angle than the red rays, thus approximating the two classes of phenomena even with respect to this singular anomaly. Hence in order to accommodate M. Fresnel's formula to homogeneous light of different colours, let m be the index of refraction for the homogeneous ray, and d the difference between it and the mean index, then the formula for the phase P will become

$$\operatorname{Cos} P = \frac{2\,(m \pm d)^2 (\sin i)^4 - \left((m \pm d)^2 + 1\right)(\sin i)^2 + 1}{\left((m \pm d)^2 + 1\right)(\sin i)^2 + 1}$$

the sign + being used for the red or least refrangible rays, and — for the blue or most refrangible.

For the same reason, in calculating the phases of an elliptically polarized homogeneous ray by means of the formula tan $\varphi = \frac{\cos(i + i')}{\cos(i - i')}$, we must determine i' from the formula $\sin i' = \frac{\sin i}{m \pm d}$, the sign + being used for the red or least refrangible, and — for the blue or most refrangible rays.

As the theoretical considerations upon which M. Fresnel is said * to have constructed his formula, did not present to him the above anomaly, it would be in vain for me to seek an explanation of it. I may just mention, however, that at the second surfaces of bodies the angle of maximum polarization, or $\tan \frac{1}{m}$ is necessarily less for the least refrangible than for the mean rays, which is the reverse of what takes place at the first surface; and since the limit of total reflexion whose sine is $\frac{1}{m}$, or since the sphere or circular polarization commences

* I am acquainted with M. Fresnel's formula only from the account given of it by Mr Herschel.

sooner for the least than for the most refrangible rays, it might be expected that the angle of maximum circular polarization should be less for these rays, as I have found to be the case.

Although we do not understand the nature of the forces by which metals reflect the two oppositely polarized pencils, yet they act exactly like the second surfaces of transparent bodies when producing total reflexion. Setting out from a perpendicular incidence, the least refrangible rays begin to suffer the double reflection sooner than the mean ray, and they sooner reach their maximum of elliptic polarization, thus exhibiting the inversion as it were of the spectrum, which we have noticed.

The theory of elliptic vibrations as given by Fresnel, will no doubt embrace the phenomena of elliptic polarization; and when the nature of metallic action shall be more thoroughly examined, we may expect to be able to trace the phenomenon under consideration to its true cause.

ALLERLY, *February* 19*th*, 1830.

ART. IV.—*Account of other Four Cases of Spectral Illusion.* (Continued from No. vi. p. 245.)

I CONTINUE my communications to you of the singular spectral illusions to which Mrs ——— has been unhappily liable. The last of which I give you an account took place, I think, on the 14th March last. From that time to the 5th October, no phenomenon of the kind was experienced, and we began to hope that these symptoms of internal malady, with their cause, had disappeared. On that day, however, between one and two o'clock in the morning, I was awoke by Mrs ——— who told me that she had just seen the figure of my deceased mother draw aside the bed-curtains, and appear between them. The dress and look of the apparition were precisely those in which my poor mother had last been seen by Mrs ——— at Paris in 1824.

A few days afterwards, on the 11th October, Mrs ——— sitting in the drawing-room on one side of the fire-place, saw the figure of another deceased friend moving towards her from the window at the further end of the room. It approached the fire-place, and sat down in the chair opposite that in which Mrs ——— was seated. As there were several persons in the room at the time, Mrs ——— describes the idea uppermost in her mind to have been a fear lest they should be astonished or alarmed at her staring in the way she was conscious of doing, at vacancy, and should fancy her intellect disordered. Under the influence of this fear, and recollecting a story of a similar effort in Sir W. Scott's work on *Demonology*, which she had lately read, she summoned the force and resolution necessary to enable her to cross the space before the fire-place, and *seat herself* in the chair which appeared occupied by the figure. She did this; the apparition remaining perfectly distinct, till she sat down as if in its lap, when it was no longer perceived.

On the 26th of the same month, about two o'clock P. M. Mrs ——— was sitting on a chair by the window in the same room with myself. I heard her exclaim, "What have I seen?" and on looking towards her perceived a strange expression in her eyes and countenance. On inquiry, she told me that a carriage and four had appeared to her to be driving up the entrance road to the house. As it approached, she felt inclined to go up stairs to prepare to receive company, but found herself unable to move or speak, as if spell-bound. The carriage came nearer, and as it arrived within a few yards of the window, she saw the figures of the postillions and the persons inside take the ghastly appearance of skeletons, and other hideous figures. The whole then vanished entirely, and she made the exclamation which I heard.

October 30th.—Mrs ——— tells me that this morning, while sitting in her own room with a favourite dog in her lap, she distinctly saw the same dog to all appearance moving about the room, during the space of about a minute or rather more.

December 3d.—About nine o'clock at night, sitting near Mrs ——— in the drawing room, both, as I thought, occupied in reading, I felt a pressure on my foot as if intended to attract my attention. On looking up I observed Mrs ———'s eyes fixed with a strong and unnatural stare on a chair about nine or ten feet distant. I perceived immediately she was under the influence of a spectral illusion, and asked her what she saw. The expression of her countenance then changed, and, on recovering herself, she told me she had seen my brother, who was alive and well at the moment in London, seated in the opposite chair, but dressed in grave clothes, and with a ghastly countenance, as if scarcely alive.

This is the last apparition which has yet occurred. The first remark that suggests itself on these successive delusions is the extraordinary resemblance of the greater number of them to the usual circumstances of the ghost stories we have all heard repeated, with more or less of authority for them, from our cradles upwards. Here, however, the apparition of the *double of the lap-dog*, like the previous one of the cat, introduces itself most happily as a key to the mystery, and a guarantee to the imagination of the most credulous and bigotted believer in the world of spirits, of their strictly natural and physical causes, of their being in fact, beyond question, mere optical illusions induced by disease.

I may here also repeat what I likewise observed before of the previous apparitions, Mrs ——— confidence that in no one of these instances were her thoughts dwelling on, or tending in any way towards, subjects which could be supposed associated with the idea of the persons who appeared to her. Consequently the imagination, memory, and other faculties of the mind seem to be wholly unconcerned in the suggestion or production of the spectral forms.

I shall continue to keep a journal of any similar facts; and shall be glad if Dr Hibbert or yourself can suggest any experiments to be adopted in case they should continue to manifest themselves, with a view to ascertain their immediate causes.

ART. V.—*On the Electro-magnetic properties of metalliferous Veins in the Mines of Cornwall.* By ROBERT WERE FOX, Esq. of Falmouth, Hon. Mem. Plymouth Institution, and M. R. Geological Society of Cornwall. Communicated by the President. *

IN one of my communications to the Cornwall Geological Society on the high temperature of the interior of the earth,

* Abridged from the *Phil. Trans.* 1830, p. 399.

I ventured to express a belief that mineral veins, and the internal heat, are connected with electrical action. This opinion, founded as it was on the curious arrangement of the veins, &c. in primitive rocks, I have had the satisfaction to find confirmed by experiments made in some of the mines of Cornwall; and I doubt not that the existence of electricity in metalliferous veins similarly circumstanced, and capable of conducting it, will prove to be as universal a fact, as the progressive increase of temperature under the earth's surface is now admitted to be, much as my conclusions on this point were at one time controverted.

In my first experiment, I did not succeed in detecting any electricity; but in my second I had the gratification to observe considerable electrical action.

My apparatus consisted of small plates of sheet copper, which were fixed in contact with ore in the veins by copper nails, or pressed closely against it by wooden props, stretched across the "levels" or galleries. Between two of these plates at different stations, and a galvanometer, a communication was made by means of copper wire one-twentieth of an inch in diameter which was at first coated with sealing-wax; but afterwards this precaution was dispensed with. This galvanometer consisted of a magnetic needle three inches and a quarter long, one-eighth of an inch wide, and one-twenty-eighth thick. It was inclosed in a box four inches square, and one inch in depth, having a plated copper wire one-fiftieth of an inch in diameter coiled round it twenty-five times. No magnet was used to neutralize the terrestrial polarity.

The intensity of the electro-magnetic action differed greatly in different places:—in some cases the deviation of the needle was inconsiderable, in others it went completely round the circle. In general it was greater, *cæteris paribus*, in proportion to the greater abundance of copper ore in the veins, and in some degree perhaps to the depth of the stations;—and where there was little or no ore, there was little or no action. Hence it seems likely, that electro-magnetism may become useful to the practical miner in determining with some degree of probability at least, the relative quantity of ore in veins, and the directions in which it most abounds.

When the distance of the plates from each other in a horizontal direction was only a few fathoms, and the copper ore between them was plentiful, and uninterrupted by non-conducting substances, or the workings in the mine, no action occurred, owing no doubt to the good conducting power of the vein; but where a cross vein of quartz or clay happened to be between the plates under similar circumstances, the action was usually great.

When the communication was established between two plates at different depths on the same vein, or between different veins, whether at the same level or otherwise, the electrical action was in general the most decisive. In fact, veins which in some instances were almost destitute of ore, and did not affect the needle *per se*, did so, though perhaps only in a slight degree, when electrical communications were made between them.

It will be seen that the direction of the positive electricity was in some cases from east to west, and in others from west to east; and when parallel veins were compared, its general tendency was, I think, from north to south, though in several instances it was the reverse. In veins having an underlie towards the north, the east was commonly positive with respect to the west; but in veins dipping towards the south, the contrary was observed, with one exception only, and that under rather unusual similar circumstances. In comparing the relative states of veins at different depths, the lower stations appeared to be negative to the upper; but exceptions sometimes occurred when a cross vein of quartz or clay intervened between the plates, and the higher one was on the negative side with respect to the horizontal currents.

In such cases it may be supposed that there is an accumulation of electricity in different states, on the opposite sides of the non-conducting vein. Such intersections of ore veins, and their being often very rich to a great depth in one direction and not in another, added to their varying underlie at different depths, which is not unfrequently reversed, may tend to produce apparent anomalies in experiments of this nature.

At Huel Jewel mine, I obtained results between a heap of copper ore at the surface, and a plate fixed at different depths against the ore in the vein; the latter becoming more negative, in proportion to the depth at which it was placed. Piles of copper ore at the surface did not act on the needle when tried together, independently of veins, nor was it to be anticipated that they would.

It is not improbable that the progressive increase of negative electricity observed in descending into our mines, if hereafter confirmed, may be found to be connected with the progressive increase of temperature. I have not, however, discovered any distinct connection between them at the same level, but then the differences of temperature are comparatively small. Nor does the electricity appear to be influenced by the presence of the workmen and candles, or by the explosion of gunpowder, although some veins of copper ore were blasted on different occasions in the immediate vicinity of the copper plates. And at a very productive copper vein in Great St George Mine, the ground is so soft that gunpowder is not used; yet the needle was powerfully acted upon by the electricity it contained. On this occasion, as well as on some others, I remained with the galvanometer at the surface, letting the wires down through the shafts; and in this manner I have sometimes found the electricity act with considerable energy, so as even to cause the needle to revolve with some velocity.

In connection with the electricity of veins, I deemed it desirable to ascertain the relative power of conducting galvanic electricity possessed by many of the metalliferous minerals; and it appeared to be in about the following order, viz.

Conductors.

Copper nickel,
Purple copper,
Yellow sulphuret of ditto,
Vitreous ditto,
Sulphuret of iron,
Arsenical pyrites,
Sulphuret of lead,
Arsenical cobalt,
Crystallized black oxide of manganese,
Tennantite,
Fahlerz,

Very imperfect conductors.

Sulphuret of molybdenum,
Sulphuret of tin, or rather bell-metal ore.

Non-conductors.

Sulphuret of silver,
Ditto of mercury,
Ditto of antimony,
Ditto of bismuth,
Cupriferous ditto,
Realgar,
Sulphuret of manganese,
Ditto of zinc,
Mineral combinations of metals with oxygen, and with acids.

All the conductors of galvanic electricity were so likewise of common electricity; to which may be added the oxide of tin, and, in a less degree, the sulphurets of bismuth and silver, the phosphate of manganese, and a few of the oxides. Sulphuret of zinc appeared to be a more perfect non-conductor of common electricity as well as the sulphuret of antimony, than the red oxides of those metals.

Amongst the rocks prevalent in Cornwall, clay-slate or "killas" seemed to possess the property of conducting common electricity in a slight degree, but only in the direction of its cleavage, perhaps owing to the moisture it retained.

I mention these facts in some detail, because it is curious to observe that the conducting power of metallic ores appears to have no reference to any of the electrical or other properties of the metals in a pure state, or to the proportion of them in combination. Silver and mercury, for example, are combined with, comparatively, very small quantities of sulphur;—and zinc, which seems to hold an opposite place to silver in the electrical scale, is also found in combination with a much less proportion of sulphur than is contained in copper pyrites, though the latter is one of the best mineral conductors of electricity.

There are many other analogous examples, which prove that no conclusion can be drawn, *à priori*, from the nature or chemical arrangements of minerals, as to their relative electrical properties.

Much time and attention have been bestowed by geologists on the consideration of the origin and comparative ages of veins, and but little, I apprehend, on the purposes for which they are designed.

It appears to me that it will prove a vain attempt to reconcile a multitude of facts observable in our mines with any known natural causes.

I may refer to a few of them :—

1*st*, The very oblique descent of a large proportion of the veins into the earth, in some cases in very hard rock, and in others in ground so soft that it would immediately fall in, however small the excavation, without being completely supported by timber. Were it possible to conceive fissures to exist under such circumstances, it is not reasonable to suppose that they would not take the direction in which the resistance would be least, that is, either the vertical, or the line of the cleavage of the rocks.

2*d*, Veins are often divided into branches, which unite again at a considerable depth, including between them vast portions of rock perfectly insulated by the ore or vein-stones from the general mass: these, it is evident, could not have existed as fissures for a moment.

3*d*, Veins are continually subject to changes in their horizontal direction and underlie; their size also often varies exceedingly, one part being many times wider than another, without any reference to their relative position or depth under the surface.

4*th*. Although a portion of their vein-stones are usually quite distinct in their characters from the rocks they traverse, they are generally, in part, of the same nature, and vary with the containing rocks, whether granite, elvan, killas, &c.; and they are commonly too regularly arranged in the veins, and are found inclosing insulated portions of the ore, &c. in their very substance, to admit of the idea of their having been originally mere broken fragments of the inclosing rocks.

At Dolcoath Mine there is an instance of one ore vein intersecting another at different depths, and being itself intersected and even shifted by the same vein at a greater depth.

Many other facts might, if it were necessary, be accumulated, relative to the position and intersection of veins, as well as the nature and arrangement of their contents, which, with those I have stated, are calculated to throw entire discredit on the various hypotheses which have been invented to account for their origin. But my object is, rather to suggest whether the arrangement of veins, &c. does not argue design, and a probable connection with other phenomena of our globe.

Metalliferous veins, and those of quartz, &c. appear to be channels for the circulation of the subterraneous water and vapour; and the innumerable clay veins or "flucan courses" (as they are termed in Cornwall,) which intersect them, and are often found contained in them, being generally impervious to water, prevent their draining the surface of the higher grounds as they otherwise would, and also facilitate the working of mines to a much greater depth than would be practicable without them.

With respect to their electrical properties, it may be observed, that ores which conduct electricity have generally, in this country at least, non-conducting substances interposed in the veins between the ore and the surface. Thus a brown iron ochre with quartz, &c. named "gossan" by the miners, is almost invariably found resting on copper. Sulphuret of zinc occurs sometimes in the same situation, both with regard to copper and lead; but tin ore, which is a non-conductor, is without either, and is mostly found nearer the surface than copper.

Tin veins are usually intersected by those of copper when they do not coincide in their horizontal direction or underlie; thus, in this case, the conducting veins traverse the non-conducting ones. And when two veins of copper meet at opposite angles in descending, they are, I apprehend, generally found to be unproductive at and near the place of junction; but when they unite, proceeding downward in the same direction but at different angles, they are commonly observed to be enriched. These facts appear curious when regarded in connection with the opposite currents of electricity in veins having opposite dips.

There are some districts in this county in which the ore veins have generally a north underlie, and in others the south prevails; and it often happens that when lodes occur which deviate from the prevalent underlie of the others, in any district, the former are intersected, and sometimes shifted by the latter. This is strikingly the case in numerous mines in the parishes of St Agnes and Perran.

The usual horizontal bearing of the copper and tin veins in our principal mining districts, appears to be nearly E. and W., or rather from E.N.E. to W.S.W. but in others they deviate materially from these directions, sometimes to E.S.E. and W.N.W.: indeed, in some places this is the prevailing course of the veins of ore.

When veins containing the sulphuret of silver occur, (which as I have before stated is a non-conductor of electricity,) they are generally found nearly at right angles to the copper and tin veins, and seem thus to assume in great measure the character of cross veins of quartz, clay, &c.

With respect to the two latter, it has been observed that when they shift the ore veins, there is frequently to be found in them scattered stones of ore, or a small vein of it, or "leader" (to use a mining term,) between the dislocated parts of the lode. This is also the case often with slides; so that, although the horizontal transfer of the electricity may be much impeded, it does not seem to be wholly intercepted. The quartz contained in cross veins is usually of a fibrous or radiated texture, and differs materially from that found in the east and west veins.

All our mining districts abound more or less with veins or dykes of a rock generally possessing a porphyritic character, termed by the miners "Elvan courses." Their width is extremely various, sometimes as much as fifty fathoms and upwards. Their direction in general is nearly N.E. or E.N.E. to S.W. or W.S.W., and their underlie is with few exceptions towards the N.W., and at various angles from the perpendicular, often exceeding 45°. They are penetrated by ore-veins in almost every direction, from their greater underlie, and usually more considerable deviation from an east and west bearing than the latter. It has been observed that copper and tin lodes generally become changed in quality whilst in the elvan; and indeed this remark applies to any change of rock: thus a vein productive in granite commonly becomes barren in killas, and *vice versâ*.

Many of the phenomena above referred to bear striking analogies to common galvanic combinations, and the discovery of electricity in veins seems to complete the resemblance.

I have been informed by intelligent persons who have visited some of the mining districts of Mexico, Guatimala, and Chili, that there is a general resemblance between the veins, elvan courses, &c. in some parts of those countries and our own; and I think it has been noticed by Baron Humboldt, that the stratification of primitive rocks in different, and far distant parts of the world, has a general tendency from the N.E. towards the S.W.

Such analogies become highly interesting when regarded in connection with terrestrial electricity, magnetism, and heat; for if it be granted that the two latter increase in intensity at great depths in the earth, they are evidently so connected with electrical action that the augmentation of it also, in the interior of the globe, may be reasonably inferred.

However this may be, assuming that metalliferous veins exist more or less in primitive rocks generally, (and experience favours this assumption, whether we refer to the new mines which have been discovered in various parts of North and South America, Siberia, Ireland, &c. or to the mining county of Cornwall, in which whole districts have comparatively of late been found abounding with mineral treasure, where none had been formerly suspected to exist,) it may I think be presumed, that the electrical currents, which so affect the needle in the galvanometer, may likewise influence the direction of the magnetic needle on the surface of the earth: at least no explanation of this phenomenon appears to be so plausible, or so well connected with ascertained facts. Even the cause of the variations of the needle, mysterious as it has hitherto appeared to be, may probably be referred to the relative energies of the opposing electrical currents, which are perhaps subject to occasional modifications; and the appearance of earthquakes and volcanic action, from time to time, seems to countenance the probability of such changes.

Nor should it be overlooked in reference to this view of the subject, that the oblique bearing which is generally observable in the strata and veins, with respect to the equator, causes them, as it were, to cross at opposite sides of the globe in the same parallels of latitude, so that their tendency, if any, must necessarily be to produce more than one magnetic pole in each hemisphere. Thus, in this respect also, the hypothesis accords with the interesting fact lately announced;—of Professor Hansteen having ascertained the existence of a second magnetic pole within the arctic circle. The revolution of the earth on its axis from west to east seems moreover to harmonize with the idea of oblique electrical currents; since rotation in the same direction may be produced by corresponding electro-magnetic arrangements.

Before I conclude, I will briefly mention a few facts relative to the temperature of some of the mines in Cornwall.

At Tingtang copper mine, in the parish of Gwennap, at the bottom of the engine shaft, which is in killas, and 178 fathoms deep, the water about two months ago was at the temperature of 82°. In 1820, when the same shaft was 105 fathoms deep, the temperature of the water was 68°: thus an increase of 14° has been observed in sinking 73 fathoms, which is equal to 1° in 5 fathoms.

At Huel Vor tin mine, near Helston, the water was 69° at the bottom of a shaft 139 fathoms deep, in the year 1819. It is now 209 fathoms deep, and the temperature is 79°, which gives a mean increase of 1° in sinking 7 fathoms. This part of the mine is in killas.

The highest temperature of the water at the bottom of Poldice copper and tin mine in the parish of Gwennap, in 1820, which was then 144 fathoms under the surface, was 80°. It is now 176 fathoms deep, and the temperature is 99°; and in a cross level 20 fathoms, further north, the water is 100°.

The two last-mentioned temperatures are the highest hitherto observed in any of the mines of this county; and the increase is equal to 19° in one case, and 20° in another, in sinking 32 fathoms, or 1° for $1\frac{1}{2}$ fathom. Three persons only were employed at a time near each of these stations, and the water pumped up from this part of the mine was estimated at

1,800,000 gallons in twenty-four hours; and I found on examination that it contained a considerable quantity of common salt in solution.

Art. VI.—*Observations on the Action of the Voltaic Pile.* By M. Matteucci.* In a Letter to M. Arago.

Permit me to communicate to you some critical observations on the chemical explanation of the developement of Voltaic electricity, given by M. De La Rive.

The experiments of M. Pfaff, published in your annals for July 1829, sufficiently satisfied me that we may develope electricity by means of contact only, and without chemical action.

In order to convince me still more of this, I made some experiments on this species of electricity, by taking the frog for a galvanometer.

With this view, I assured myself before-hand that there was no chemical action between water distilled and well purged of air, and zinc either alone or in contact with copper; and I was unable, indeed, even after several hours contact, to discover, by the aid of the most sensible reagents, the presence of zinc or of the oxide of copper. After this it would be erroneous, (in trying to show that chemical action is the cause of electricity by contact,) to conclude that there is chemical action because there is a developement of electricity. Being thus convinced that there is no chemical action between water distilled and purged of air, and zinc or copper, I began by suspending a prepared frog by a hook of zinc, which was fixed in the bottom of a receiver for gas, and soldered to a copper wire of greater length. In this manner, in order to prevent contractions, I had only to touch the muscles of the thigh with the copper wire.

In order to remove all suspicion of chemical action, I washed the prepared frog in water distilled and purged of air, in order to carry off any animal fluid. I afterwards suspended it by the nerves on the zinc hook, and I filled the receiver with distilled water, and afterwards with pure hydrogen gas. When the thigh was then touched with the copper wire, I observed

* *Ann. de Chimie,* tome xlv. p. 106.

the same contractions as if the experiment had been made in pure air. I tried these experiments in vacuo, in carbonic oxide, carbonic acid, and in oxygen either humid or dry, and I always observed the same contractions in the frog.

Hence I am led to believe, that the contact alone of different metals may develope electricity.

I also find an objection to the theory of M. De La Rive in the limited change which the electromotive force may develope and retain free.

Chemical action, however, does not cease to exert a marked influence on the developement of this force, as heat does on the phenomena of thermo-electricity.

Forli, *August 9th*, 1830.

Art. VII.—*On the Spontaneous Inflammation of powdered Charcoal in great masses.* By M. Aubert, Colonel of Artillery.

Spontaneous inflammations of charcoal have taken place in gunpowder manufactories under different circumstances, but most commonly when this substance, introduced in pieces, was crushed by the first strokes of the bruiser. Spontaneous inflammations of pulverized charcoal, however, took place in 1802 at the powder-work of Essone, in 1824 at that of Bouchet, in 1825 at that of Esquerdes, and in 1828 at that of Metz.

Various experiments were made at Metz to ascertain the circumstances under which these inflammations took place, and the following are the general results given by Colonel Aubert.

Charcoal triturated in tons with bronze bruisers is brought to a state of extreme division. It has then the appearance of an unctuous fluid, and occupies a space three times smaller than in rods of from fifteen to sixteen centimetres long.

In this state of division it absorbs air much more readily than when it is in rods: The absorption is, however, still very slow, and requires several days to be completed. It is accompanied with a disengagement of heat, which rises to 170° or 180° centigrade, and ought to be considered as the true cause of the spontaneous inflammation.

The inflammation begins about the centre of the mass, at twelve or fifteen centimetres below its surface, and the temperature is always higher at this place than at any other.

There ought, therefore, to be established towards the borders of the mass a descending current of air which bends itself towards the centre, and becomes vertical without penetrating to the lower parts of the mass, where the temperature rises a very little. It is from this cause that a portion only of the charcoal is concerned in the phenomenon. The rest performs the part of an insulating body, and preserves the heat at the centre.

Variations in the barometer, thermometer, and hygrometer, appear to have no sensible influence on the spontaneous inflammation of the charcoal. If such an influence exists, the experiments have not been sufficiently multiplied to enable us to perceive it.

Black charcoal, strongly distilled, heats and inflames more readily than the orange, or that which is little distilled, or than the charcoal made in boilers.

Black distilled charcoal, the most inflammable of the three, ought to have a mass of at least thirty kilogrammes, in order that spontaneous inflammation may take place. With the less inflammable varieties, the inflammation takes place only in larger masses.

In general the inflammation is more certain and active in proportion to the shortness of the interval between the carbonization and trituration of the charcoal. Air is not only indispensable for spontaneous inflammation, but it must also have free access to the surface.

The increase of weight which takes place in the charcoal is owing not only to the fixation of the air, but also in part to the absorption of water.

During the trituration the air experiences no alteration from the charcoal, nor even at the moment of inflammation.

Sulphur and saltpetre added to the charcoal deprive it of the property of inflaming spontaneously, yet there is still an absorption of air and a generation of heat; and though the rise of temperature is not great, it would nevertheless be prudent not to leave these mixtures in too large masses after trituration.—Abstracted from the *Ann. de Chimie*, Tom. xlv. p. 73.

Art. VII.—*History of the Brown Coal Formation of the Lower Rheinland.* By S. Hibbert, M. D., F. R. S. E. &c. Communicated by the Author.

No tertiary deposit in Europe is perhaps so difficult to explain in its various relations, as that which bears the name of the Brown Coal Formation. As almost every writer who has taken up his pen on the subject has differed from his predecessor in the views which he has adopted of its relative age, it will be easily imagined that I have imposed upon myself a task of no little intricacy. This is indeed my own persuasion, and I enter upon the investigation with a corresponding diffidence.

I have said thus much as an apology for the rather novel mode in which I shall enter upon a description of the Brown Coal Formation of the Lower Rhine. It will be considered in connection with the general geology of the district, with the view that its earliest manifestation may be recorded. In conformity, therefore, with this plan, I shall, as a preliminary measure, attempt,

1*st*, *A Sketch of the Geological History of the Lower Rheinland previous to the developement of the Brown Coal Formation.*

The brown coal deposit, which is the subject of the present dissertation, is found on both sides of the Rhine, from the neighbourhood of Coblentz to that of Cologne. The fundamental rocks on which it is placed consist of argillaceous and grauwacke schist, the latter containing in some places organic remains.

Of secondary rocks there are no indications. If they ever did exist, we must attribute their disappearance to causes of degradation, which, there is no doubt, have removed many such deposits in various parts of the globe, to fill up distant seas and lakes. The circumstance, however, of strata corresponding with the upper green sand of England, which occur in the adjoining district of Aix-la-Chapelle or Dusseldorf, may sanction a vague suspicion, and nothing more, that the deposit might have extended to the low lands of Bonn or Cologne.

But this is almost a fruitless speculation. The newer strata which actually subsist, are referable to a far later geological epoch, to which we shall now confine ourselves.

The earliest sub-period of the tertiary epoch has its date from the time of the breaking up of the chalk basins. In the west and south of Europe, it is indicated by a considerable change which the surface of the globe appears to have undergone in its constitution. Much new land was raised above the level of the sea, while vast fresh-water basins were constituted, which became receptacles for peculiar deposits. Thus in the Loire, the Seine, the Rhone, and various other rivers, we trace along their respective courses, incontestible evidence of chains of tertiary lakes.

The geological features of the Lower Rhine are pretty nearly the same. But, as our investigation is confined to a very limited portion of its banks, the only basin to which I shall advert, as forming a sort of early link in our history, is that of Mayence.

The lowest formation characteristic of the tertiary epoch, if we may be allowed to advert to the basin of Paris as a standard of comparison, is that which Brongniart has designated by the name of the *Terrain Marno-charbonneux*, or *d'eau douce inferieur ;* while a succeeding one is that of the *Terrain Marin Tritonien*, of which the *calcaire grossier* is an example.

During this united interval, the plain which extends from Hanau to Mayence, and even much farther south, formed a small portion of an inland sea, which originally had been either caused by the simple retreat of pelagic waters, or, upon a different hypothesis, the same effect might have been produced by some elevation of the land, whence it was separated from the wider expanse of ocean which bounds the western shores of Europe.

Neptune, however, did not uninterruptedly assert his dominion over this Caspian lake. Alternations of marine and freshwater beds, severally calcareous, and even commixtures in the same strata of the shells of land, of rivers and of seas, show that his sovereignty over these waters was disputed. And, if we may judge from the predominance of the fluviatile beds which overtop the series, the mountain-nymphs and river-gods were eventually triumphant.

This lake, during its subsistence, had no communication with the present channel of the lower Rhine, in consequence of a barrier of high land stretching across the present site of the straits of Bingen, and thus filling up the small geographical space intervening between the chains of the Hundsrück and the Taurus. And hence, the Rhine at Bingen was little more than a continuation of the minor stream of the Nahe, which, in taking its rise from the hills of the Hundsrück, is to be regarded as the original source of the river, which now derives its waters from the far distant and towering Alps.

The original elevation of the ancient river, which furrowed for itself a channel along the course of the present Lower Rhine, may be naturally enough supposed to have been very great. There is existing evidence of its having maintained the height of many hundred feet above the present level of the Rhine at Coblentz.

Between the present site of Coblentz and Andernach, the river, owing to the basin-shaped disposition of the land, was expanded into a lake, which was not less than nine miles from north to south, and about twenty miles from east to west. The present gorge at Andernach, through which the waters conveyed by the Rhine now make their escape, did not then exist, in the place of which a barrier of continued cliff rose to a considerable elevation.

The basin thus formed, which I shall name *The Andernach Basin*, was fed by many streams, of which the Rhine can scarcely be affirmed to have been the principal.

The Moselle was then a river of at least an equal importance with the Rhine, deriving its waters from a southerly origin far more remote, the ancient bed of which, where it was lost in the lake of Andernach, being still observable in the high ground to the west of the peak of the Carmelenberg.

A third subsidiary stream was that of the Nette, which rose from the hills of the Eyfel near the Nurburg; while a fourth was the Wied, which had its source in the Westerwald.

The discharge of the overflowings of this lake appears to have been effected in a northerly direction across the barrier of Andernach. And probably not far from the present site of

Dusseldorf, it was conducted to the ancient sea of the present German ocean.

But it may now be remarked, that throughout the whole course of the lower Rhine, from its most early rise in the Hundsrück mountains down to its junction with the ancient waters of the German ocean, very striking marks of an ancient channel may be detected, which can only be accounted for on the supposition, that the corroding cause was prolonged through the immeasurable lapse of ages contained in a geological epoch.

This process which was going on appeared unvaried by any event, except the volcanoes which were kindled in the heights of the Siebengebirge, or in the vicinity of the Laacher-see. In the latter site, violent rents are discernible which the earth sustained from elevating forces, accompanied by the shivering of resisting strata, and the developement of a crater; whence pent up gases with tremendous violence issued.

But this event was merely the forerunner of more extensive convulsions;—convulsions which were not confined to this limited portion of Germany, but which were apparently employed in elevating submarine lands, by which a considerable portion of the German sea, and the lands bounding it, became the site of a spacious fresh-water lake.—This is at least the most plausible theory;—but, whether correct or not, we must repose upon the proofs which will be afforded us, that lacustrine waters rose to a height of at least a thousand feet, and not only filled the channel of the Rhine to the south of the Siebengebirge, but even extended to the basin of Andernach, in which, as well as in the volcanic crater of Laach and its connected fissures, an earthy deposit may be traced.

The commencement of this deposit forms a new sub-period in the tertiary epoch of our history. Here then we shall pause, and inquire what might be the state of vegetation in the lower Rheinland during the interval which we have traced;—an interval when the imagination can dwell upon little more than a rugged assemblage of rocks torn in every direction by mountain-torrents, which, at length collecting in one common channel, furrowed out a deep bed for the descending waters.

That a flourishing vegetation existed during the united sub-periods of the *Terrain Marno-charbonneux*, and the *Terrain*

Marin Tritonien, there can be no doubt. The former is characterized by its clays, by its marls, and marly sands; by its gypsum, its lignites, or its amber. It is also the earliest forerunner of an epoch, which, according to M. Adolphus Brongniart, differs from every preceding one in the absence of any organic forms foreign to the vegetation which is now in actual progress. Of this formation, however, no indications are afforded in the Lower Rheinland; and hence we must conclude, that if it has ever existed, subsequent convulsions have swept all traces of it away.

Such is a faint sketch of the geological history of the Lower Rheinland, previous to the developement of the Brown Coal Formation. I shall therefore consider,

2dly, The Sand and Sandstones of the Brown Coal Formation of the Lower Rheinland.

The brown coal formation may, with various interruptions, be traced from the basin of Andernach along the course of the Rhine, where it occurs on both sides of the river, particularly near the Siebengebirge, covering the declivities of the schistose mountains. Along the ridge of hills which extend from Godesberg to Bergheim, it forms deep beds, and is then lost in the flat ground of the lower lands.

Its general nature may be summed up in a few words. It consists of a fine sand, which in some few places, by the siliceous agglutination of its materials, passes into a firm sandstone, succeeded by beds of plastic clay, which occur in different relations of superposition, along with shale, with thin layers of sphærosiderite, and much thicker seams of lignite, the latter being commonly named brown coal.

The relative age of this formation, which by Brongniart has been referred, though evidently with hesitation, to that of the lower fresh-water beds of Paris, or *Terrain Marno-charbonneux*, rather dates, as I have endeavoured to show, from the cessation of the *Terrain Marin Tritonien*. Consequently, I refer its commencement to the succeeding period of the *Terrain Palæotherien*, when great revolutions took place in various parts of Europe, favourable to the developement of lacustrine deposits, some of which are not very dissimilar to that of the Brown Coal formation of the Lower Rhine.

Many details relative to the mineralogical character of this interesting deposit have been furnished by Professor Noeggerath of Bonn, who, in his office of superintendant of the mines of that district, has become familiar with all its localities and various appearances. Other incidental notices regarding it are to be found in the works of Professor Steininger of Treves, and in the system of geology published by Professor Leonhard of Heidelberg. As the labours of these several writers are much less known in this country than they ought to be, I gladly avail myself of any opportunity afforded me to communicate the valuable information which they have imparted, where my own researches may have proved deficient.

The lowest member of the brown coal formation is an incoherent SAND,—showing that it was the earliest deposit of the lacustrine waters. It consists, according to the summary of its characters given by Professor Noeggerath, of fine, round, clear transparent quartzose grains, generally mixed, though not abundantly, with minute silvery scales of mica. Party coloured varieties may be also met with. Grains, for instance, of a common yellow colour are sometimes found in abundance, and impart their hue to a large mass;—those of a wine-yellow variety are comparatively scarce. Other tints are indigo-blue, blueish-grey, hyacinth, or flesh-colour,—though these are sparing occurrences. Among none of the coloured sands which I have enumerated is to be detected any mineral substance, save quartz. There are, however, occasionally disclosed certain blackish or brownish particles associated with quartzose grains slightly attrited, which have been judged to be carbonaceous, or, in other words, to have the character of brown coal.

The origin of this deposit is a difficult subject of investigation. As this sand is to be traced from the commencement of the basin of Andernach, as far down the Rhine as the low beds below Bonn, where it becomes lost, it is natural to look for its origin to the rivers by which this basin is fed;—which origin, (infinitely the most simple one to comprehend,) is, if possible, rendered the more plausible, by the indications which the fine quartzose particles of the sand, and their diffused scales of mica afford, of their having been derived from a primary class of rocks. And, if we are entitled to suppose, that, by the gradual degradation of the original barrier of cliff which filled up the geographical space between the plain of Mayence and the Straits of Bingen, the lower Rhine began about this period to be fraught with materials conveyed from more distant hills, the accumulation of sand in the lower Rhine may not only indicate the opening of the communication, but also the drainage of some upper basin, gorged with the fine materials of primary rocks, which the disintegrating operations of infinite ages had accumulated. This theory I consider the more probable, as I have succeeded in detecting on the site of a kindred deposit near the Carmelenberg, and in the crater of the Laacher-see, where a bed of fine sand occurs, fragments of limestone detached from the older tertiary beds of Mayence.

That during this deposit a vegetable creation subsisted upon the lands which were left dry, the commixture which has been detected of the particles of brown coal with those of quartz, gives us every reason to infer.

But the most interesting circumstance is, that the close of this deposit furnishes us with the earliest known date when those vast animals were called into existence, which ranged among the ancient forests and swamps of Europe, before its soil was adapted to the residence of man.

In a quarry at Liedberg, in the circle of Gladbach, there was found upon a bed of fine quartzose sand, the depth of which was not ascertained, lying between its surface and some superimposed beds of sandstone, the bones of immense animals, many of which crumbled to pieces on exposure to the air. Among such as were in a state of integrity, was identified a tooth of the *Elephas Primigenius* of Blumenbach. (*Das Gebirge in Rheinland Westphalen, &c.* vol. iv. p. 375.)

This discovery gave additional weight to the opinion which, from other sources of information, began to be entertained, that animals, whose dawn of existence was referred to the Diluvian period, were actually contemporaneous with the Paleotherium and the Anaplotherium of the ossiferous gypsum of Paris.

Whether the latter were beginning to grow extinct when the former were called into existence, is a question of geological history yet remaining to be fully determined.

Such is the character of the deposit of SAND which forms the lowest member of the brown coal formation of the Lower Rheinland. Other, and succeeding beds, though by no means uniformly present, are those of sandstone.

THE SANDSTONE OF THE BROWN COAL FORMATION.—This sandstone, which has been particularly described by Professor Noeggerath, differs in the fineness or coarseness of its ingredients; in the nature of its cement; and in its degree of firmness or cohesion.

The structure of the finer variety, which is the most prevalent kind, may be described as granular; the grains being like those of sand which are connected either by a quartzose, a ferruginous, or an argillaceous cement. When the cement is quartzose, which is its predominant character, the cohesion is oftentimes so intimate, that a distinct granular structure is not always distinguishable; the rock having an imperfectly conchoidal and splintery fracture, and approaching to the appearance of hornstone. In other instances, however, the granular particles are so incoherent, that the stone admits of being frittered to pieces with the fingers.—The cement may also consist of the hydrous oxide of iron, when the sandstone has less of a spotted yellow than of a streaked colour.—And, lastly, strata are found which have an argillaceous as well as ferruginous cement;—whence the yellow and brown tints which they exhibit.

The structure of the coarse variety of sandstone, which is comparatively rare, is best observed in the Siebengebirge. It is distinguished by fragments of coarse quartz and hornstone often an inch in thickness;—the fragments resembling the quality of the finer and firmer sandstone described, the grains of which are connected by a quartzose cement. The colour of these coarse ingredients shows various commixtures and shades of blue, gray, and milk white; less frequently, the blendings of gray, black, brown and yellow; and least of all, those of yellow, green, or rose-red.

The sandstone of the brown coal formation is nearly horizontally disposed in the form of beds from one to three feet in thickness;—which in the Siebengebirge exhibit fissures that open wedgewise towards the upper surface, and appear like yawning clefts.

Vegetable remains have met with conservation in this sandstone. The Siebengebirge beds inclose pieces of wood-opal and semi-opal a foot or more in extent, which often contain in their clefts coatings of stalactitic milk-white calcedony. The silicified wood which I obtained from this site resembled the internal structure of the coniferous tribe. Other specimens which I procured had well-marked impressions upon them of leaves. Such impressions are generally covered over by a yellow hydrous oxide of iron.

Having at length described the sand and sandstone of the brown coal formation, I may repeat, that, whenever they are associated with any other members of the brown coal formation, they form the lowest strata. The sandstone is not always the concomitant of the quartzose sand, being, in fact, found only in a few places; but wherever it occurs, it is the uppermost bed.

There is again another bed to be noticed, though a very partial one, of a still later date, which has hitherto met with less attention by German writers than it deserves, probably owing to its very obscure relations. This is the loose pebbly mass which surmounts the sand and sandstone.

Kiesel-Gerölle.—This pebbly bed, named Kiesel-Gerölle, is of interest in showing the altitude to which the deposit brought down by the Rhine or the Moselle attained, when the lacustrine waters into which they flowed were maintaining their high level. While the lighter suspended matters, with which they were fraught, would become diffused through the wide expanse of the basins through which they flowed, larger pebbles or boulders would not travel far from the course of these streams, but would remain to indicate their ancient route. Thus, the site where the basin of Andernach first received a portion of its deposit from the Moselle is to be detected in the remarkable accumulation of rounded pebbles and boulders of quartz, which are to be observed on the west of the peak of the Carmelenberg, in a spot many hundred feet higher than the present low level of the river; the substance whence these stones are derived having survived a disintegrating process, which has reduced the argillaceous schist it once traversed in the form of veins, to the comminuted state of sand and clay.

At Liedberg, however, we find quartz pebbles surmounting a series of beds of sand and sandstone at a much lower level; the hill being said to rise to no more than 120 to 130 feet above the level of the surrounding plain.

To explain this diminution of level, we must return to the geological history of the Lower Rheinland.

We have traced in the bed of the German Ocean the rise of lacustrine waters, which not only occupied the valley of the Rhine near Bonn, but even extended to the basin of Andernach. During this elevation, the comminuted earthy materials brought down by the Rhine and the Moselle would become first deposited in the basin of Andernach, where these two currents met. Here, therefore, the accumulation would be the greatest; and, hence, the remains of the deep deposit of sand which we trace in this basin;—a deposit which has even filled the elevated crater of Laach.

Again,—from the basin of Andernach, the deposit, in proportion as we descend along the channel of the Rhine, would naturally dwindle in thickness; and, accordingly, near Bonn, the boulders which surmount the sand and sandstone appear at a much less elevation. Lastly, if we would, at a still remoter site, continue our inquiries into the ultimate state of the deposit, we may possibly, if we choose to travel so far, identify some part of it among the deep mass of boulders of an unknown thickness, which the philosopher Leibnitz has recorded as the lowest strata explored in a well sunk near Amsterdam, to the depth of two hundred and thirty-two feet.

I shall conclude my account of the siliceous beds of the Brown Coal formation, by a glance at the strata of Liedberg, to which so frequent a reference has been made:—it is the abstract of a detailed communication on the subject by Professor Noeggerath.

In a descending series, the beds were found as follows:—

(*a*.) *Kiesel Gerölle*, mixed with coarse yellow sand, and at the foot of the hill, loam.—Depth 10 to 35 feet.

(*b*.) *Falscherstein*. A very incoherent sandstone, 8 to 10 feet thick, pervaded by thin seams of red or yellow ochre.

(*c*.) *Haustein*. A firmer sandstone, fit for architectural purposes, of a greyish white colour, with yellowish streaks. Passes gradually into the upper bed.

(*d*.) *Klinkert*. (quartz-sandstone) Of uncommon hardness; being only fit for turnpike roads. The fracture splintery and conchoidal.—4 to 5 feet thick.

b. c. d. are sandstone beds, conjointly 2½ to 3 fathoms thick, with an inclination from 4° to 5°.

(*e*.) A beautiful fine, white, quartzose sand. On the surface of it were found bones of extinct animals.—7 feet of the sand have been opened. The depth is unknown.

These are all the particulars which it is necessary for me to give, relative to the lower lacustrine beds of the brown coal formation. The period when the vast lake, which apparently occupied a portion of the bed of the German ocean, had attained its summit level, and when the deposit brought down by the Rhine and Moselle had acquired its greatest thickness, was probably the close of the particular epoch distinguished in geological history by the formation of the *Terrains Palæotheriens* of Brongniart. At the same time, the *Elephas primigenius*, whose remains have been found in the lower tertiary beds of the Rhine, must have been coëval with the ancient animal, the name of which has been imparted to the peculiar deposit of the Paris basin, in which the ossiferous gypsum is included.

To the same period we must also refer,

3dly, The deposit of Plastic Clay which succeeded to the Sand and Sandstone.

Concerning the origin of the plastic clay belonging to the brown coal formation, there is perhaps some little difficulty The disintegrated materials brought down by the Rhine and the Moselle having been diffused through a great expanse of waters, grains of quartz would, from their gravity, be the first precipitated; while this precipitation would be the greatest in the depressions nearest to the mouths of these rivers;—in such depressions, for instance, as those which the basin of Andernach, or the declivity of Bonn, must have presented during the terraqueous state which I have described. But more levigated particles, of a siliceous as well as of an argillaceous character, would remain longer suspended in the superambient fluid, and would therefore be borne by currents to considerable distances, and dispersed through the body of the great lacustrine waters. Hence, probably, the thick bed of fine clay which has been traced so far as Holland, and which at Amsterdam exists at a depth of 130 feet, resting upon a still more ancient accumulation of rolled fragments.

In such particular lacustrine sites, however, as were not exposed to the direct force of currents, much argillaceous matter remaining suspended would tranquilly subside, and form the beds covering substances before accumulated, or even, as we find at Mayen, in the vicinity of Coblentz, constituting an independent local deposit of plastic clay.

But the greatest accumulation of plastic clay must have ensued when the lacustrine waters which deposited the sand and sandstone were retiring.

The cause of the extensive drainage which these lacustrine waters underwent, is necessarily veiled in great obscurity. In its operations, it appears to have been extremely gradual, and to have been continued during the whole of the succeeding epoch of Brongniart's *Terrains Thalassiques Proteiques;*—an epoch which is indicated by the Nagelflue of Switzerland, the marine deposit of the English *Crag*, or the *Terrain Marin Superieur* of Paris.

During the gradual diminution of level which lacustrine waters thus underwent, the Rhine, in its obligation to undermine for itself a channel at a reduced level, would, by the force of its currents, remove most of the loose sand or overlying sandstone which it had deposited, to fill up distant beds of the ocean. In short, it is impossible to traverse the Lower Rheinland without being convinced, that only a small portion remains of a large deposit, which occupied a wide tract of country extending from Coblentz to Cologne, and perhaps much farther north; the removal having taken place when the retiring waters of a fresh-water lake were doomed to mingle with the waves of an encroaching sea.

The immense beds of sand or sandstone thus carried away by corroding streams, were in some few sites replaced by other depositions. During the retiring of the waters, limited basins were in many places formed, favourable to the production of lesser lakes or pools, and being filled with waters in which lighter matters were suspended, various local deposits became the result.

In fine, from these various causes conjoined, the strata of plastic clay existing in the lower Rheinland are found less in continuous strata than in insulated patches; covering also prior deposits of sand or sandstone.

But we may now take a brief glance at the mineralogical character of the plastic clay.

This substance may be regarded as a commixture of the finer particles of silex, alumina, and even other earthy ingredients, with the addition of iron, and, perhaps, manganese. It is also observed to pass into sand, similar to that which I have described as consisting of quartzose grains and minute scales of mica. The colours which it displays are various; the most common being milk-white, or yellowish. At Mayen, crimson-red variegations are exhibited. Lastly, much of the plastic clay is mixed or otherwise associated with carbonaceous matter, the origin of which I shall shortly consider.

4th, The beds of Compact Sphærosiderite associated with Clay, &c.

Very little need be said about strata, which chiefly occur in one site, namely, in the Geistinger wood, north east of the Siebengebirge.

Professor Noeggerath has remarked, that although spherical and kidney-shaped nodules of sphærosiderite or carbonate of iron, from an inch to a foot in diameter, have been found in an isolated form in most of the clay beds of the brown coal formation, whole beds of this ironstone have not before been described.

The colour of the sphærosiderite is yellowish grey. It is compact, and of a flat conchoidal fracture, of a dull aspect, and having a specific gravity of 3.568. When fresh quarried, it shows faint coloured stripes or streaks parallel to the stratification, but which, by the action of the air, come sharply out, and acquire a reddish brown hue, giving the beds a banded appearance like that of ribband jasper. The analysis is carbonic acid, 32.231 ; oxydulated iron, 52.128 ; siliceous earth, 5.676 ; argillaceous, magnesian, and calcareous matters with vegetable remains, 9.965. Total 100 parts.

Near the Siebengebirge, from 11 to 13 beds of this substance, from a few inches to about a foot in thickness, have been worked. They are alternated with strata of clay, as well as of volcanic tufa.

5th, The Carbonaceous or Brown Coal Beds.

Previous to a history of the brown coal beds, I shall glance at their mineralogical character, in describing which, I shall avail myself of the excellent account of them which has been published by Professor Leonhard.

This distinguished mineralogist divides brown coal into (*a*) pitch coal, or jet ; (*b*) common brown coal ; (*c*) bituminous wood or fibrous brown coal ; (*d*) moor-coal ; (*e*) earthy brown coal ; (*f*) alum earth.

(*a.*) Of *Pitch-coal, or jet*, I shall say little, as its character is well known. It only appears in small layers or nests in the common brown coal.

(*b.*) *The common brown coal*, which is the predominating species, appears in beds of great thickness and extent, and is chiefly distinguished by the form of wood being only in part recognizable, by the texture being only occasionally fibrous, or by the complete absence in it of the well known fibrous structure of wood. Its specific gravity is 1.28. It is blackish brown and compact. Its fracture is earthy, and approaching to the conchoidal, and it has a greasy lustre. In burning, it first gives out a little smoke, but afterwards brightens up with a tolerably pure flame, yielding an ash very like that of wood, but more earthy, and containing, somewhat plentifully, iron and potash. It yields from 45 to 50 per cent. of carbon and earthy materials, and 55 of volatile matter ; leaving, after being consumed, from six to eighteen parts of a residue.

(*c.*) *Bituminous Wood, or Fibrous Brown Coal.*—This substance marks the first degree of change from an organic to an inorganic substance, in which the history of brown coal is to be read. It is of a blackish brown colour, showing distinct fibres of wood. The bark and annual rings are not unfrequently distinguishable. The stem, branches, or pieces of the roots are in general flatly pressed. The plants to which these remains are referable have been already noticed. Beech, oak, the fir cones of the *Pinus picea*, and more rarely of the *Pinus abies*, also Sumach, (Schwartzholz) and birch. There is also often found in the same bed with the lignites, innumerable seeds of the *Erica vulgaris*, and even the remains of earth-beetles.

The bituminous wood is susceptible of some few modifications. At the Püzberg near Friesdorf it contains a more or less plentiful diffusion of particles of clay ironstone, and, in the same place, a substance like leafy anthracite appears in dark coloured layers. It has also been found, though elsewhere, penetrated by sulphur.

(*d.*) *Moor-coal, (Moorkohle.)* Specific gravity 1.2 to 1.3; colour between pitch-black and blackish-brown; compact; fracture even; lustre dull or glimmering. This substance has been considered as a decomposed brown coal without any ligneous structure. But its character is best recognized by regarding it as composed of reeds and swampy plants.

(*e.*) *Earthy Brown Coal.*—This has been described as nothing more than a common brown coal, decomposed to a higher degree than moor-coal; to which belongs the Cologne umber, or Cologne earth. It has also been regarded as a bituminous substance consisting of destroyed vegetables, such as seeds, and leaves, and stalks of swamp-plants, and rinds of the branches of trees.

The earthy brown coal is remarkable for containing the trunks or stems of bituminous wood, and, according to M. Faujas, the remains of Cervi and other animals.

Both the moor and earthy brown coal occur in beds of great thickness and extent, only yielding in this respect to the common brown coal.

(f.) Alum Earth (Alaunerde.)—This is nothing more than a clay, rich in alum, through which much bituminous matter is diffused. Or, rather, it is a clay with which vegetable matters have been mixed. That which I examined at Altwied was of a bluish, or of a brownish colour.

German geologists have also enumerated other varieties of brown coal, as the *Bast-coal*, consisting of the twisted rinds of pines and alders, and the *needle-coal*. But as it is doubtful if they exist in the Lower Rheinland, and as the distinction is at best a subordinate one, I shall pass them over.

The thickness of the brown coal beds is various. One German author has affirmed that they do not exceed 6 or 8 feet; while another, who appears more familiarly acquainted with them, mentions beds 18, 24, 26, or even 32 feet thick.

6*th*, *The origin of the Carbonaceous, or Brown Coal Beds.*

So late as the period indicated by the ossiferous gypsum or palæotherian beds of Paris, the west of Europe enjoyed a temperature so far exceeding that which at present prevails, as to render it the region of palms. This is proved by the numerous arborescent monocotyledons which have been found in the brown coal beds of the Lower Rheinland. Geologists have at various times supplied us with the names of such remains as have been thus entombed, to many of which it has been found rather difficult to assign a correct place in the vegetable kingdom. The list of them which I have collected is as follows:

Cocos Faujasii, found at Lieblar in the Cologne district; *Carpolithes Arecæformis*, *C. cocoiformis*, Cologne district; *C. amygdalæformis*, *C. pisiformis*, *C. pomarius*, *C. lenticularis*, Osberg, not far from Erpel; *Endogenites ? bacillaris*, Cologne district.

In the higher lands, however, from which the Rhine derived its origin, there is every reason to suppose, as I shall very soon show, that a perfectly different vegetation, corresponding to a colder climate, subsisted. The description of trees which flourished, comprised the *Pinus picea* or the *Pinus abies*, the beech, the oak, or the alder. With these, the common heath (*Erica vulgaris*) was contemporary.

During the period when the lacustrine waters of the Lower Rheinland were maintaining their high level, vegetation appears to have made some little advance;—which is indicated by the diffusion of carbonaceous matter through some of the lower beds of sand, while the silicified wood of the Siebengebirge sandstone points to the drifting which had then commenced of the Coniferæ of Alpine heights. At the same time, new races of Mammalia were called into existence; among which were the Elephas primigenius, the rhinoceros, the hippopotamus, and Cervus Euryceros,—animals severally adapted to the early lacustrine, or marshy state of the surface of Europe.

The period, however, when the most profuse indications are afforded of a flourishing vegetation, may, with much reason, be referred to the era of the *Terrains Protéiques* of Brongniart.

The climate of the Lower Rheinland must then have gradually cooled, so as to approach that of the temperate regions of the globe. This may be inferred from the proofs which are afforded that the oak, the beech, and other forest trees of less warm climates, were once contemporaneous with the fossil palms of Cologne, which they far exceeded in abundance;—a circumstance which renders it highly probable that the temperature of this district nearly resembled that of the southern coasts of Italy, or of Spain, which can still tolerate the growth of plants of opposite regions. Thus, at S. Remo, in the Genoese States, dense plantations of palms had long subsisted, which were latterly encouraged for the sake of the branches required for the papal processions of Palm Sunday. And at Murcia, the palms which many ages ago had been particularly noticed by Pliny, continued to be fostered for the sake of a similar pious traffic with Italy, so late as the year 1775. "We stopped at Elche," says the intelligent Swinburne, "a large town belonging to the Duke of Arcos, built on the skirts of a wood, or rather forest, of palm-trees, where the dates hanging on all sides in clusters of an orange-colour, and the men swinging on bass ropes to gather them, formed a very curious and agreeable scene. *The palms are old and lofty; their number is said to exceed two hundred thousand.* Many of the trees have their branches bound up to a point, and covered with mats, to prevent the sun and wind from getting to them."

But to return to our history.

There are still other circumstances to be kept in view, if we would fully explain why the remains of Coniferæ, of the beech, the oak, or the alder, are so much more abundant in the brown coal beds, than those of arborescent monocotyledons.

Much of the vegetable matter, indicative of temperate rather than of tropical regions, must have been brought down by the Rhine during periodical or occasional floods, from the remote and elevated lands of the European Alps, where the temperature differed greatly from that of the low declivities of the Rheinland; having been deposited while the lacustrine waters maintained their high level, after the manner of the immense accumulation of drift-wood incidental to the embouchures of the great rivers of North America, which has been transported from the region of the pine to that of the fig or the olive.

This view will be confirmed by the circumstances under which much of the timber of the fibrous brown coal beds is found; while, on the other hand, some trees have been disinterred, consisting of palms as well as oaks, which show that they must have flourished simultaneously. Thus, at Lieblar, near Cologne, a palm was found in an erect position, and, under similar circumstances, the dicotyledonous plants of temperate regions have been discovered.

We must conclude then, that the same floods, which, from remote elevations, differing considerably in temperature, would transport the spoils of overgrown woodlands, would also undermine the densely planted margins of contiguous embouchures; —or, that the swollen Rhine, in its impetuous course, would sweep away the foundations of much adjacent soil; causing land-slips, or even bearing with it numerous floating islands, with multifarious trees still clinging in an erect posture to their native soil; and that these, when their further progress was resisted by shoals or any other impeding cause, would be mingled with the far imported drift-wood of alpine heights.

This view would meet with some support, if it could be shown that trees occur mingled indiscriminately in the same bed in both a vertical and horizontal position; the former indicative of a growth *in situ*, and the latter of distant transportation.

An observation of this kind has, indeed, been already made. Professor Noeggerath has recorded, that at the Pützberg, near Friesdorf, the upper beds consist of variously alternating beds of earthy brown coal, bituminous wood, alum earth, and potter's clay, in which are found isolated trunks of trees, some of them resembling the oak, of enormous thickness, varying from seven even to twelve feet in diameter, and destitute of their upper parts, which appear as if broken off or split. While some of these trees are horizontally imbedded, others are found standing upright, and passing through all or most of the associated beds of brown coal, alum earth, or potter's clay.

7th, The alternations of beds of Brown Coal with those of Plastic Clay.

The occurrence of beds of brown coal alternating with plastic clay, suggests an investigation of other circumstances under which the ancient vegetation of this district was developed.

It has been assumed, that, at the era of the *Terrains Proteiques*, the lacustrine waters of the Lower Rheinland had begun, from some obscure geological causes, to undergo a gradual and long-continued process of drainage, during which, a considerable waste or removal took place in the beds of sand, sandstone, and plastic clay, successively deposited. Being liable to be acted upon by the periodical or extraordinary inundations to which all large rivers are subject, considerable removals of the beds, particularly of the upper ones, would ensue. Thus, at Roisdorf, loose blocks only remain of the continuous bed of sandstone which reposed upon the sand, and, at Friersdorf, no trace whatever of the same has been left. Nay, in some places, the waste appears to have extended to a much greater depth; removing the subjacent sand altogether.

The proof that the vegetation of the Lower Rheinland must have flourished most during this succession of changes, is, that the lowest brown coal beds in the neighbourhood of Cologne may be seen to rest upon the loose sand from which the sandstone has either been removed altogether, or appears in the form of severed or insulated blocks. This is shown at Roisdorf, where, upon the loose sand from which the sandstone has been removed, rests bituminous clay; or at Brühl, where, un-

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der similar circumstances, repose powerful clay and brown coal beds.

In short, there is reason to suppose, that during the protracted retreat of lacustrine waters, while numerous forest trees, such as the oak, the beech, or the pine, began to occupy the firmer shores which were slowly laid bare, sandy or mud-formed tongues of land and islets were developed, in the soft materials of which palms fixed their roots, along with an abundance of aquatic reeds or sedges, the debris of which may be traced in the thick existing beds of earthy brown coal.

The vegetation which had thus taken root, would, in the next place, be liable to be submerged during adventitious periods of inundation, beneath the materials of the sandy or loamy beds thus removed; and, more particularly, beneath the beds of plastic clay, which, in forming the upper layers of the lacustrine deposits of the Rhine, would be the first removed;—while accumulations of drift-wood, transported by the rush of inundations, and covered over by renewed earthy deposits, would induce the frequent alternations of clay and brown coal beds, which are so observable in the district of the Lower Rheinland.

Two examples of these alternations, on the authority of Professor Steininger, may be quoted, the beds of which are given in a descending series.

At Friesdorf.	At Walwerberg, Lieblar, and Bruhl.
Gerölle, (gravel,)	Gerölle, (gravel,)
Brown coal,	Brown coal; 26 to 32 feet thick,
Potters' clay,	Potters' clay; unknown depth.
New brown coal floetz; not worked through at 20 feet.	

8th, The beds of Shale alternated with those of Brown Coal.

Still other effects would result during the retreat of the lacustrine waters. Much of the surface of the sandy or clayey deposits, which had been left exposed by the diminution of level which the Rhine had undergone, would present concavities of greater or less depth, which would be filled with the waters which remained upon the occasion of their emergence.

Into these minor lakes or pools, generally formed by depressions made in the upper strata of plastic clay or brown coal, the disintegrated materials, derived from the gradual waste of adjoining hills of primary schistose strata, appear to have been washed; and these, mingling more or less with the bituminous matter of brown coal beds, or co-existing vegetation, or with the earthy particles previously suspended in the waters of these small basins, appear to have given rise to corresponding strata, which are to be regarded as little more than varieties of common shale, generally bituminous.

German geologists have, however, subjected these strata to very forced distinctions, as into (*a*) *Klebschiefer*, adhesive slate; (*b*) *Polierschiefer*, polishing slate or Tripoli; and (*c*) *Papierkohle*, paper coal.

(*a*.) *Klebschiefer*, Adhesive slate, so named from adhering to the lip when moist, has been described as of a light yellowish grey, greyish white or smoke-grey colour, thin and slaty in its texture, and in its fracture flatly conchoidal; easily triturated, and shivering readily in the direction of its laminæ. Menilite is sometimes inclosed by it in small roundish and flattish nodules. The analysis given of Adhesive slate is so various that it is not worth stating. It must necessarily differ in different places, according to the ultimate nature of the substances from which, as a shale, it is derived.

(*b*.) *Polierschiefer*, Polishing slate or Tripoli.—I cannot find that this substance differs materially from adhesive slate. It is described as of a yellowish or reddish white colour, easily separable into thin and slaty laminæ, which are so tender that they may be rubbed to a powder by the fingers. The notion of its having assumed this condition from the operation of fire, is not on the present occasion to be entertained; the effect being more like that of dryness or weathering.

Both the adhesive and polishing slate are described as absorbing water with avidity, and throwing out air-bubbles.

(*c*.) *Papierkohle*, Paper coal.—This is of a blackish brown colour, with a dull, as well as glistening lustre; divisible into uncommonly thin and tender leaves, whence its name of paper-coal.

All these three substances, viz. adhesive slate, polishing slate, and paper-coal, pass into each other.

Such is the character of the strata which form the beds incidental to the pristine pools in which they were deposited. While the process was going on, these basins were stored with numerous fish, frogs and lizards, of species still existing, which are now discovered interposed and flattened between the folia of the shale which I have described. In a quarry near Unkel, I was so fortunate as to obtain the impression of an insect about the size of a common bee, and resembling an individual of the Hymenopterous, or perhaps Dipterous order.

Plentiful impressions of leaves and trees also appear under similar circumstances, which, as well as the beds of brown coal, associated with the shale, seem of an extraordinary freshness; having been apparently derived from the later plants which flourished around the margins of these pools.

I shall conclude this account of the associated shale and brown coal beds, with the following section of a pit near the Siebengebirge, from the surveys of the German geologists. The beds are stated in a descending series:

Loamy soil containing brown coal.

Loam strata.

Brown coal, consisting of the carbonized wood of trees.

Shale (Adhesive and Polishing slate;) containing impressions of fresh-water fish and plants.

Paper coal, with impressions of fish and plants.

Greyish-white potters' clay; the lowest observed bed.

9th, *The volcanic eruptions which were coëval with the Brown Coal Formation.*

Upon this portion of our geological history, I shall say very little.

It has been explained, (page 279,) that trachytic eruptions preceded the commencement of the brown coal formation. Subsequently, the phenomena took place of mud volcanoes, similar to those which are still recognized in South America;—and that these were contemporaneous with the lacustrine deposits of the Rheinland, is shown in various sites. It would appear from a section at Queggstein in the Siebengebirge, that the overflow of mud, (named trachyte conglomerate,) took place at the close of the sandstone deposit, which this volcanic product covers. At the Ofenkulerberg, beds of trachyte conglomerate contain a layer of altered leaves and other remains of plants; and at the Langenberge, altered wood may be found under similar circumstances. At Geistinger, where twelve or more thin layers of sphærosiderite are alternated with clayey beds, chiefly derived from decomposed trachyte, the whole is surmounted by slaty brown coal and paper coal; the latter containing impressions of leaves and of fish. The later eruptions of basalt which took place in the Rhine district appear to have been contemporary with the upper beds of brown coal; for at Utweiler a flow rests upon the same, the substance of which has been converted into a sort of pitch-coal or jet. Lastly, basalt blocks are found in the Gerolle which covers the Brown Coal formation.—(See *Noeggerath's Rheinland Westphalien*, vol. iv. p. 383, &c. and *Steininger's Memoirs, for farther details.*)

10*th, Sequel of the History of the Plants which flourished at the time of the Brown Coal Formation.*

Our geological history of the brown coal beds must now be considered as brought down to the close of the period of the *Terrains Protéiques*, or upper marine formations of the London and Paris basins. At this time, lacustrine waters had subsided before the renewed inroads of the sea; while the corroding torrent of the impatient Rhine, by removing most of the soft materials of the brown coal formation, and transporting them to the deep bed of the German ocean, had again occupied the channel which it had formerly excavated for itself through firmer strata of argillaceous schist. It is highly probable that at the close of this epoch, the climate of the Lower Rheinland had been deteriorated; that its palms had disappeared;—the oak, the beech, or the pine having remained in indisputed possession of the soil.

A succeeding epoch, to which Brongniart has referred his *Terrains Epilymniques*, is characterized by the upper freshwater beds of the Isle of Wight or of Paris, by the ancient Travertino of Italy, and perhaps by such deep beds of common peat as can be shown to have subsisted immediately preceding the great diluvial deposits of the north of Germany, or of the British islands. Upon the heights of the Veen, situated to the west of the Rhine, an ancient deposit of clay is surmounted by beds of turf to the depth of sixteen feet. The lowest of these, which contain wood of an obscure character, much resemble beds of true brown coal, of which they were probably the immediate successors. In this instance, however, there has been no interruption whatever to the vegetation indicated by these beds from any diluvial waters, although, in the valley of the Rhine, traces of such a catastrophe are evident,—particularly in the immense quantity of transported mud, named *Britz*, which has been deposited near the present site of Andernach, though to no greater a height than five or six hundred feet. The vegetation of more considerable elevations must have therefore remained uninterrupted. Accordingly, on the Hohe Veen, in the strata of turf succeeding to the deepest ones described, birch-wood, fir-cones, or hazelnuts are found, while the uppermost layers of hard or swampy moss connect the series of beds with the vegetation of the present day. And thus, also, may the modern forests of the Rheinland boast a derivation from the ancient stock which is to be recognized in the fossil wood of the brown coal formation, and which has an antediluvian date, fully as remote as that of the Palæotherian beds of Paris.

In concluding this very difficult history, it might be expected that I should describe the economical uses to which the different beds of the brown coal formation are applied. But as the object of this memoir was a very different one, I shall content myself with the following brief statement. Some of the sandstone is used for architectural purposes, but the harder variety is the most valuable, being so much indurated by a siliceous cement as to be applicable to the making of roads. The extraction of the purer clay, which is in great demand as a pipe-clay, or as a potters' clay, takes place in deep pits at Bannerhof, at Dreckenach near Coblentz, and at various other places. The lignites have long been in popular use as a fuel, while the variety named the earthy brown coal has been worked for the valuable pigment named burnt umber, or Cologne earth. But of late years, the brown coal beds have acquired more importance from their affording the materials for the extensive alum-works of the neighbourhood of the Siebengebirge. The most powerful strata of this deposit were found out nearly forty years ago, and it is honourable to the Rheinland to record, that a handsome monument has been erected near the mines as a tribute to the humble individuals to whom the country has been indebted for the discovery. From this memorial I copied the following inscription: DEM ANDENKEN DES ERSTEN FINDERS DER HARDTER BRAUNKOHLEN-LAGER, JOHANNES KIRSCHBAUM UND SEINER EHEFRAU ANNA MAGDELENA LÜTZ. [To the memory of the discoverers of the Brown Coal deposit of the Hardt, John Kirschbaum, and his wife, Anna Magdalena Lütz.]

This tribute displays the popular feelings of the district, and is strikingly contrasted with the neglect which is exhibited on the British shores for even far more important benefits; a neglect which is now operating to the serious prejudice of the kingdom, by annually thinning the ranks of useful or scientific contributors.

ART. IX.—*Observations on the Mean Temperature of the Globe.* * By DAVID BREWSTER, LL. D. F. R. S. Lond. & Edin.

IF no provision has been made by the Great Author of Nature, for equalising the light and heat projected upon the different bodies of our system, we may consider the earth as receiving, from the direct action of the solar rays, a degree of heat, intermediate between the condensed radiations sustained by Mercury and Venus, and the attenuated warmth which reaches the remoter planets. The heat which our globe thus acquires from its locality in the system, is again tempered by the obliquity of its axis, and is distributed over the same parallels of latitude by its daily rotation. When the sun is in the Equator, his rays, beating on the earth with a vertical influence, impart to it the full measure of their action; and as his meridian altitude decreases, their intensity suffers a corre-

* Read before the Royal Society, February 1, 1820.

sponding diminution. The burning heat at the Equator becomes moderated in higher latitudes. In passing through the Temperate Zone, it declines with great rapidity, and between the Arctic Circle and the Pole, the rays of the sun are unable even to temper the piercing cold which reigns in these inhospitable regions.

This gradation of temperature, so obvious to the sensations of those who have visited southern climates, is still more distinctly indicated by its physical and moral influence. The arid plains of a tropical climate, where vegetable life is almost extinguished by excessive heat, gradually shade into the more luxuriant regions of the vine and the olive. The mild and uniform temperatures of Spain and Italy, are again followed by the variable climate, and the more verdant kingdoms in the north of Europe. Then succeeds the region of blighted vegetation, where nothing can exist but the birch and the pine; and the chain of vegetable life terminates in the hoary desolation of the Arctic Zone.

The progression of climate is no less distinctly marked by the developement of the human faculties. Indolent and impatient of thought under the debilitating influence of a sultry atmosphere, man begins to unfold his capacities as he is removed from the Torrid Zone. In more temperate climates, he cultivates those faculties which do not lead to very rigorous application; and under the invigorating influence of a colder sky, the mind attains that maturity of its powers, which fits it for the most abstract and profound speculations. From this region, distinguished as the seat both of ancient and modern civilization, the mind again sinks under the torpor of extreme cold, and the distinctive characters of our species disappear among the diminutive inhabitants of the Polar latitudes.

The investigation of the mean temperature of the earth, connected, as it thus appears to be, with many interesting inquiries of a moral and physical nature, has not been pursued with the same zeal as other branches of knowledge. Long after the invention of the thermometer, no attempt had been made to apply it to meteorological purposes; and though, for more than half a century, its indications have been registered in many parts of the world, yet the observations with it have commonly been guided by no settled principle, and are, therefore, frequently unfit for the purposes of generalization. Philosophers were satisfied with deducing a law of temperature from theoretical considerations; and disdained the humbler and more laborious task of interrogating the mass of facts which had been accumulated by zealous and active observers.

The first person who attempted to deduce from observation a general expression for the mean temperature, at all latitudes, was the celebrated astronomer Tobias Mayer of Gottingen. Assuming that the heat varies as the square of the sine of the latitude, he obtained the formula $T = 58° + 26° \times \overline{\text{Cos. } 2 \text{ Lat.}}$, in which 58° is the mean temperature of 45° of north latitude, and 26° the difference between the temperature of that parallel and the Equator. M. Lichtenberg, the editor of Mayer's posthumous works, applied this formula to 13 observations of mean temperature made between the Cape of Good Hope and Stockholm, and their agreement was considered at that time to be remarkable. The sum of all the errors was 26°.8, or a little more than 2° on each observation; but as the errors in excess amounted to 22°. 3, while those in defect were only 4°.5, it should have been obvious that the formula was founded upon an incorrect assumption.

The formula of Mayer was implicitly adopted by Kirwan, in his able work *On the Mean Temperature of the Earth*; and has been more recently brought forward, as connecting together, "in a most harmonious manner," the results of distant temperatures, although the fine series of observations, collected by Humboldt, had demonstrated its inaccuracy, and proved, that even in the parallel of 63°, it erred in excess no less than 9° of Fahrenheit.

The beautiful memoir of Humboldt on the *Isothermal Lines*, or lines of equal temperature, and on the distribution of heat over the globe, has given a fresh impulse to this fundamental branch of meteorology, and will, no doubt, introduce a new degree of precision into those loose and indefinite records of temperature which have been so generally accumulated in every part of Europe. In attempting to reconcile the formula of Mayer with the observed results as given by this celebrated traveller, I expected to succeed, at least for the western regions of the Old World, by the adoption of more correct coefficients; but as I proceeded in the inquiry, I saw that the principle of the formula was entirely irreconcileable with well established facts, and I therefore sought for a law different from the duplicate ratio of the sines.

In comparing the temperature of the Equator with that of 45°, and with that of the highest latitude in Humboldt's series, it was obvious that the cold increased much more rapidly towards the Poles than had been believed; and upon extending the comparison to the intermediate temperatures, I found that the mean heat of any place was well represented by the radius of its parallel of latitude, or, in geometrical language, that the temperatures varied with the cosine of the latitude. In expressing this law I have assumed $81\frac{1}{2}°$ as the mean temperature of the Equator, the very same number which Humboldt has preferred as the mean of various results under distant meridians.* The formula therefore becomes

$$T = 81\tfrac{1}{2} \text{ Cos. Lat.}$$

The following table contains the observed mean temperatures of thirty-one places, situated between the Equator and the parallel of 70° of north latitude, together with the calculated results, as given by the preceding formula, and by that of Mayer, in its latest form.

TABLE OF MEAN TEMPERATURES.

	Lat.	Obs. Mean Temp.	Calc. Mean Temp.	Diff.	Mayer's Formula.	Diff.
Equator,	0° 0′	81°.50	81°.50	0°.00	84°.2	2°.7 +
Columbo,	6.58	79.50	80.90	1.40+	83.4	3.9 +
Chandernagore	22.52	75.56	75.10	0.46—	76.3	0.74—
Cairo,	30 .2	72.82	70.56	1.76—	71.1	1.31+
5 Funchal,	32.37	68.54	68.62	0.08+	69.0	0.46+
Rome,	41.54	60.44	60.66	0.22+	60.7	0.36+
Montpellier,	43.36	59.36	56.03	0.33—	59.4	0.04+
Bourdeaux,	44.50	56.48	57.82	1.34+	59.0	2.52+
Milan,	45.28	57.18	58.28	1.10+	57.7	0.52+

* Additional grounds for this assumption will be found in this *Journal*, No. xi. p. 117, and No. xv. p. 60.

		Lat.	Obs. Mean Temp.	Calc. Mean Temp.	Diff.	Mayer's Formula.	Diff.
		° ′	°	°	°	°	°
10	Nantes,	47.13	54.68	55.35	0.67+	56.10	1.42+
	St Malo,	48.39	54.14	53.85	0.29—	54.80	0.66+
	Paris,	48.50	51.89	53.65	1.76+	54.60	2.71+
	Brussels,	50.50	51.80	51.47	0.33—	52.90	1.10+
	Dunkirk,	51 .2	50 54	51.25	0.71+	52.70	2.16+
15	London,	51.30	50.36	50.74	0.38+	52.30	1.94+
	Bushey Heath,	51.37¾	51 .2	50.58	0.62—		
	Kendal,	54.17	46.02	47.58	1.56+	49.8	3.78+
	New Malton,	54.10	48.28	47.53	0.75—	49.9	1.62+
	Lyndon,	54.34	48.90	49.37	0.47+	49.5	0.60+
20	Dublin,	53.21	49.10	48.65	0.45—	50.6	1.50+
	Copenhagen,	55.41	45.68	45.95	0.27+	48.6	2.92+
	Edinburgh,	55.57	46.23	45.64	0.59—	48.32	2.09+
	Carlscrona,	56.16	46.04	45.46	0.58—	48.2	2.16+
	Fawside,	56.58	44.30	44.26	0.04—	47.5	3.20+
25	Kinfauns,	56.23½	46.20	45.12	1.08—	47.9	1.70+
	Stockholm,	59.20	42.26	41.57	0.69—	45.5	3.24+
	Upsal,	59.51	42.08	40.94	1.14—	45.1	3.02+
	Abo,	60.27	40.00	40.28	0.28+	44.6	4.60+
	Umeo,	63.50	33.08	35.96	2.88+	42.1	9.02+
30	Uleo,	65 .3	33.26	34.38	1.11+	41.3	8.04+

This singular agreement between the observed and calculated results, and the equilibrium of the positive and negative errors, shows that the formula embraces the leading causes which affect the mean temperature of the west of Europe. The sum of all the positive errors is 13°.12, and the sum of the negative errors 9°.11; and their total amount is 22°.23, which gives only an average error of $\frac{8}{10}$ths of one degree of Fahrenheit upon each observation.

The results of Mayer's formula give all the errors positive except one, and their sum is no less than 70°.7, being 2°.3 for each observation. The error of the formula becomes so great as 9° between the parallels of 60° and 70°, which proves, in the most convincing manner, that the temperature of 32°, which Mayer assigns to the North Pole, is very far above the truth. The formula which I have given above makes the polar temperature so low as *zero*, or 0° of Fahrenheit's scale, differing 32° from the preceding measure; but the circumstance of its representing with accuracy the mean temperatures at very high latitudes, inspires us with some confidence even in this extreme result.

In this state of uncertainty respecting the probable temperature of the North Pole, and of the accessible parallels between 70° and 80°, I proposed to examine the most northern meteorological journals, with the view of finding some general rule by which the mean temperature of the year might be deduced from one or two months observations. I had previously communicated my formula to Mr Scoresby, and requested from him some information respecting the temperature of the Greenland Seas; and I had the satisfaction of finding, that this subject had engaged his most particular attention, and that he had actually deduced the mean temperature of the parallels of 76° 45′, and 78°, from a series of 650 observations made by himself, in *nine* successive years. *

In the latitude of 76° 45′ he found the mean temperature to be 18° $\frac{86}{100}$dths. My formula makes it 18° $\frac{68}{100}$dths, deviating only $\frac{16}{100}$dths of a degree.

In the latitude of 78°, Mr Scoresby found the mean temperature to be 16° $\frac{99}{100}$dths. My formula makes it 16° $\frac{95}{100}$dths, deviating only $\frac{4}{100}$dths of a degree. Mayer's formula makes the mean temperature of these parallels above 34°, erring no less than 33° upon both observations, whereas the error of my formula is only $\frac{1}{5}$th of a degree.

It appears, then, from the evidence of direct observation, that the temperature of the North Pole must be considerably lower than 16° $\frac{99}{100}$dths, and must therefore be more correctly indicated by the new formula than by that of Mayer. Mr Scoresby has attempted, by a very ingenious analogical process, to deduce the temperature of the Pole from that of 76° 45′. He considers the difference between the actual temperature of that parallel, viz. 18°.86, and the temperature given by Mayer's formula, or 33°.8′, as an anomaly produced by the frigorific influence of the ice; and having found what this anomaly should be at the Pole, he subtracts it from Mayer's po-

* This interesting investigation is now published, in Mr Scoresby's excellent *Account of the Arctic Regions*, vol. i. p. 356.

lar temperature, in order to obtain the real polar temperature, which he thus finds to be 10°. This result, however, is obviously too great, upon Mr Scoresby's own principle; for since Mayer's formula errs greatly in excess in those parallels where there is no accumulated ice to produce an anomaly, it must give at least an equal error in excess for the parallel of 76°.45′. Now, this error in the latitude of 63° and 65° in Lapland is 8°; and therefore calling it also 8°, which is far too low for the latitude of 76° 45′, we have for the mean temperature, uninfluenced by the ice, 33°.8 — 8°.0 = 25°.8; from which subtracting the polar anomaly of 21°, as computed by Mr Scoresby, and we obtain 4°.8 for the mean temperature of the Pole.

In the preceding paragraphs, we have compared the results of the formula with the temperatures of individual places, which must often be influenced by local causes. We shall therefore compare the formula with the temperatures of the four parallels of 30°, 40°, 50°, and 60°, which Humboldt has deduced from a great variety of observations, and which he considers as well established.

	Lat. N.	Observed Mean Temp.	Calculated Mean Temp.	Differences.
	30°	70°.52	70°.56	0°.04 +
	40	63 .14	62 .43	0 .71 —
	50	50 .90	52 .39	1 .49 +
	60	40 .64	40 .75	0 .11 +
Scoresby	76 45′	18 .86	18 .68	0 .18 —
Do	78	16 .99	16 .95	0 .04 —

The differences between the observed and calculated temperatures, both in this and the preceding Table, are frequently owing to the circumstance of the thermometer having been observed at two periods, the average of which does not give the mean temperature of the day. The Reverend Mr Gordon has found, from a series of very accurate observations, that the mean temperature will be obtained most correctly in this country, when self-registering thermometers are not used, by observing at 10 o'clock in the morning and evening; and it is highly to be desired that this principle should be adopted in all our meteorological journals. Another source of difference arises from local causes, which often occasion a difference of temperature under the same latitude. In the case of Edinburgh, for example, the mean temperature, deduced from Mr Playfair's observations, is 47°.8*, differing considerably from the formula, while the mean temperature, according to the observations of Messrs Miller and Adie, is 46°.23,† agreeing very nearly with the calculated results.

Hitherto we have considered only the temperature of the Old World, as determined under meridians passing through the west of Europe; and previous to actual observation, it was reasonable to infer, that under other meridians the heat would follow a similar law of distribution. The testimony of travellers, however, soon corrected this hasty inference; and as the condition of more distant climates was better known, the severity of a Canadian and a Siberian winter became proverbial. Notwithstanding this evidence, Mayer, and Kirwan, who adopted his formula, have considered it as universally applicable; and Mr Leslie has maintained, on the authority of a few insulated facts, that the average temperature of the Old and New World will be found the same.‡

These speculations, however, have been completely overturned by the researches of Humboldt. He has shown, that though the temperature of the Equatorial regions is nearly the same under all meridians, yet in higher latitudes it declines rapidly in the new world, and under the eastern meridians of Asia. In the three first columns of the following Table, he has given the differences of temperature for the eastern and western hemispheres, under the parallels of 30°, 40°, 50° and 60° of north latitude.

Lat.	Temp. Old World obs.	Temp. New World obs.	Diff. between O. and N. World.
30°	70°.52	66°.92	3°.60
40	63 .14	54 .50	8 .64
50	50 .90	37 .94	12 .96
60	40 .64	23 .72	16 .92

The difference of temperature of the Old and New World is

* See Note A, p. 319.

† These observations were made in Merchant Court, 230 feet above the level of the sea.

‡ *Edinburgh Encyclopædia*, Art. AMERICA, Sect. *Climate of America*, vol. i. p. 614.

nearly 4° in the parallel of 30°; 9° in the parallel of 40°; 13° in the parallel of 50°; and 17° in the parallel of 60°.

The determination of the temperature of North America, enables us to approximate with more certainty to the degree of cold which exists at the North Pole; and as this question must always possess considerable interest in relation to any attempt that is made to explore these icy regions, I would request the attention of the Society to the nature of the argument by which I conceive that we may ascertain the *maximum* limit of the Polar temperature.

In the Old Continent, the mean heat at 60° of latitude is 40°. In 78° of latitude, Mr Scoresby makes it 17°, and thence infers that it must be 10° at the Pole. Now, if Mr Scoresby had approached the Pole in a meridian passing through the New World, he would have encountered a cold of 24° in the latitude of 60°; and in the parallel of 78° this cold would have increased to 4°, as deduced from the formula. If we then subtract from this an anomaly calculated after Mr Scoresby's ingenious process, we shall find that the Polar temperature computed in this way is many degrees below the *zero* of Fahrenheit's scale. Or, to state the argument more popularly, since the cold at the Pole is 10°, as inferred from observations made in the *mildest* meridian, it must fall greatly below this, and even *below zero*, if inferred from observations made in the *coldest* meridian. The winds which blow from the continent of Greenland,—from the northern extremities of America,—and from the frozen coast of Siberia, must produce at the North Pole an influence which is scarcely felt in the Spitzbergen seas.

From all these considerations, we are entitled to infer that the formula, which represents the actual temperatures with such accuracy from the Equator, and through all the varieties of climate in the Temperate Zone, even to the parallel of 78°, where the fixed ice acts with its full influence, is not likely to fail in its accuracy when extended to its limit; and, therefore, that the temperature of the Pole itself is not far from 0° of Fahrenheit.*

* As this reasoning is founded on the assumption that the Pole is the

The Meteorological observations which have been recently made in Lancaster Sound by Captain Parry, confirm in a very remarkable degree the general formula, and the opinions respecting Polar temperature, which I have endeavoured to establish in the preceding pages.* Instead of giving too great a degree of cold to the Arctic latitudes, as every person supposed the formula to have done, it errs on the opposite side, and ascribes to the parallel of $74\frac{3}{4}$° a temperature of about 6°, whereas Captain Parry found it to be so low as 1°.33.

This intrepid and skilful navigator, whose important discoveries in the Arctic Regions do equal honour to the heroism of the men under his command, and to the liberality of the British Government, observed the temperature of the regions which he visited, with peculiar care, and by means of the finest instruments. The observations were made every two hours; and as the expedition continued nearly twelve months in the parallel of 74° 45′, and in the meridian of 110°, we may consider the mean annual temperature of that point of the globe as established by means of above 4300 observations.

The following abstract of this valuable Journal has been kindly communicated to me by Captain Parry, with the permission of the Lords of the Admiralty.

" *ABSTRACT of the* HECLA's *Meteorological Journal for Twelve Kalendar Months, during which Period she was within the Parallels of* 74° *and* 75° *of North Latitude.*

MONTHS.	Mean Temperature of Air in Shade.			REMARKS.
	Max.	*Min.*	MEAN.	" During the time that we were in Winter Harbour, it was always found that the
1819, Sept.	+37°	—1°	+22°.54	thermometer on board stood
Oct.	+17 .5	—28	— 3 .46	from 2° to 5° higher than the
Nov.	+ 6	—47	—20 .60	one on shore, from the
Dec.	+ 6	—43	—21 .79	warmth created by the fires,

coldest point of the globe, the results given above will admit of considerable modification, if that supposition shall be found improbable, as will be shown in the subsequent part of this paper.

* This part of the paper was read before the Royal Society on the 4th December 1820.

1820, Jan.	— 2	—47	—30 .09
Feb.	—17	—50	—32 .19
Mar.	+ 6	—40	—18 .10
Apr.	+32	—32	— 8 .37
May,	+47	—4	+16 .66
June,	+51	+28	+36 .24
July,	+60	+32	+42 .41
Augt.	+45	+22	+32 .68
Annual Temperature,			+ 1°.33

&c. The *minimum* temperature for February was —50° on board, but —55° on the ice. On the ice, 14th and 15th of February, the thermometer was at — 54° for seventeen hours.

The mean annual temperature may be fairly considered as 1° or 2° below zero."

The intense cold which is thus proved to exist in the latitude of $74\frac{3}{4}$°, indicates, when compared with that in 78° in the Spitzbergen Seas, a very singular state of the Isothermal lines at the Pole itself. The thermometric curve of 17°, which rises in the meridian of Spitzbergen to the 78th degree of north latitude, descends in the meridian of Melville Island to the 65th degree, and unless we suppose that the climate of these regions is subject to no law, we are forced to conclude that the Pole of the globe is not the coldest point of the Arctic hemisphere, and that there are *two points of greatest cold* not many degrees from the pole, and in meridians nearly at right angles to that which passes through the west of Europe.

Observations are still wanting to determine the exact positions of the Isothermal Poles; but they appear to be situated in about 80° of N. Lat., and in 95° of East and 100° of West Long.; the Transatlantic one being nearly 5° to the N. of Graham Moore's Bay in the Polar Sea; and the Asiatic one to the north of the Bay of Taimura, near the North-East Cape.

This view of the distribution of temperature within the Frigid Zone, suggests, or rather renders necessary, a New Law of the Progression of Climates. The gradation of heat on the Transatlantic Meridian is so essentially different from that in the west of Europe, that it is impossible to represent the two classes of phenomena by one Formula, in which the limiting temperatures are found at the Equator and the Pole. No attempt, indeed, has been made to include them in the same law, and still less to refer them to a principle which embraces all intermediate meridians.

As we are not acquainted with the cause of the anomalous distribution of heat in high latitudes, observation alone must guide us in determining the form of the Isothermal lines. From their general resemblance to the Isochromatic Curves, I tried to calculate the temperatures by the product of the sines of the distance of the place from the two Isothermal Poles; but this law did not represent the facts, and I found that they might be more accurately expressed by the formula

$$\text{Mean Temp.} = 82°.8 \text{ Sin. D.}$$

upon the supposition that the greatest cold is 0° of Fahrenheit, or

$$\text{Mean Temp.} = 86°.3 \text{ Sin. D} - 3\tfrac{1}{2}°$$

upon the more probable supposition, that the greatest cold is $-3\frac{1}{2}°$ of Fahrenheit, 82°.8 being the mean temperature of the Equator in the warmest meridian, and D the distance of the place from the nearest Isothermal Pole.*

By applying this last formula to the results obtained by Humboldt, and to the observations of Captain Scoresby and Captain Parry, we shall have the following observed and calculated temperatures. †

	Mean Temp. of Old World.			Mean temp. of New World.		
Lat.	Observ.	Calc.	Difference.	Observ.	Calc.	Difference.
30°	70°.52	71°.61	+1°.09	66°.92	62°.61	—4°.31
40	63 .14	63 .31	+0 .17	54 .50	51 .97	—2 .53
50	50 .90	53 .16	+2 .26	37 .94	39 .65	+1 .71
60	40 .64	41 .55	+0 .91	23 .72	26 .02	+2 .30
$74\frac{3}{4}$	Captain Parry,	-	-	1 .33	4 .37	+3 .06
73	17 .00	19 .66	+2 .66	Captain Scoresby.		

* The distance D from the Isothermal Pole is in the coldest meridian D = 80° — Lat.; and in the warmest meridian Cos. D = Cos. 10° × Sin. Lat. In all intermediate meridians we have $\text{Cos. D} = \frac{\text{Cos. L (Cos. } l - \theta)}{\text{Cos. } \theta}$, and Tang. θ = Cos. M. Tang. L, where M is the difference of longitude between the place and the Pole, L the co-latitude of the Isothermal Pole or 10°, and l the co-latitude of the place.

† The calculation for the Old World is founded on the supposition that the meridian to which the mean results of Humboldt belong is at right angles to the cold meridian in 100° west longitude. The greater number of places, however, from which the mean was taken, are nearer the Asiatic than the American Pole. Hence we see the reason why the differences are all positive.

The differences in the *fourth* and *seventh* columns are far from being considerable; and when we reflect upon the uncertainty in the position of the poles, and the range of the annual temperature at any given place, the coincidence of the observed and calculated results is greater than could have been expected.

In the preceding comparison, the places to which the mean results belong, are supposed to be situated either in the warm meridian which passes through the west of Europe, or in the cold meridian which passes through North America. In comparing, however, the theory with observation, we shall proceed to put it to the severe test of contrasting it with observations made in intermediate meridians, both in the Old and the New World; and in this comparison we shall begin with the Asiatic Pole, and suppose it to have a temperature of — 3½°, and to be placed in 80° N. Lat. and 95° of East Long. from Greenwich.

	Distance from the Asiatic Pole.	Mean Temperature. Observed.	Calculated.	Difference.
Enontekies,	20° 39′	31°.03*	26°.93	—4°.10
Uleo, -	23 16	33 .08	30 .59	—2 .49
Umeo, -	25 06	33 .26	33 .11	—0 .15
St Petersburg,	27 11	38 .84	35 .92	—2 .92
Stockholm,	29 44	42 .30	39 .30	—3 .00
Moscow, -	29 55	43 .16	39 .54	—3 .62
Warsaw, -	36 06	48 .56	47 .35	—1 .21
Astracan, -	37 25	49 .08	48 .94	—0 .14
Vienna, -	40 37	51 .76	52 .68	+0 .92
Pekin, -	40 56	54 .86	53 .04	—1 .82
Nangasaki, -	48 57	60 .80	61 .58	+0 .78
Seringapatam,	68 04	77 .00†	76 .55	—0 .45
Columbo, -	73 12	79 .50	79 .12	—0 .38

From these differences, which are almost all negative, it appears that we have assumed too great a degree of cold for the Asiatic Pole. If we make it 0° of Fahrenheit, we obtain the following results:

* Reduced to the level of the sea by Humboldt's rule.

† The mean temperature in 1814 was 78°.25, and in 1816, 75°75. No correction is applied for its elevation above the sea.

	Mean Temperature. Observed.	Calculated.	Difference.
Enontekies, -	31°.03	29.20	— 1.83
Uleo, -	33 .08	32.71	— 0.37
Umeo, -	33 .26	35.12	+ 1.86
St Petersburg,	38 .84	37.83	— 1.01
Stockholm, -	42 .30	41.07	— 1.23
Moscow, -	43 .16	41.30	— 1.86
Warsaw, -	48 .56	48.79	+ 0.23
Astracan, -	49 .08	50.31	+ 1.23
Vienna, -	51 .76	53.90	+ 2.14
Pekin, -	54 .86	54.25	— 0.61
Nangasaki,	60 .80	62.44	+ 1.64
Seringapatam,	77 .00	76.81	— 0.19
Columbo, -	79 .50	79.26	— 0.24

The differences in this Table (amounting at an average to $1^{\circ}\frac{1}{10}$) are far within the limits of the errors of observation; but being negative in general, they may be reduced still farther to an average of 1°, by supposing the Asiatic Pole to have the temperature of + 1° of Fahrenheit, which is $4\frac{1}{2}°$ warmer than the Transatlantic Pole. The formula in this case becomes

$$T = 81.8° \text{ Sin. } D + 1°,$$

from which we obtain the following results:

	Mean Temperature. Observed.	Calculated.	Difference.
Enontekies	31°.03	29.85	— 1.18
Uleo, -	33 .08	33.31	+ 0.23
Umeo, -	33 .26	35.70	+ 2.44
St Petersburg,	38 .84	38.37	— 0.47
Stockholm,	42 .30	41.57	— 0.73
Moscow, -	43 .16	41.80	— 1.36
Warsaw, -	48 .56	49.20	+ 0.64
Astracan, -	49 .08	50.70	+ 1.62
Vienna, -	51 .76	54.25	+ 2.49
Pekin, -	54 .86	54.59	— 0.27
Nangasaki, -	60 .80	62.69	+ 1.89
Seringapatam,	77 .00	76.92	— 0.08
Columbo, -	79 .50	79.33	— 0.17

In comparing the theory with observations made round the Transatlantic Pole, the results are equally satisfactory. The third column of the following table is calculated from the formula $T = (86°.3 \sin D) - 3\frac{1}{2}°$, and the Pole is supposed to be situated in 80° N. Lat. and 100° West Long.* and to have a temperature of — $3\frac{1}{2}°$.

	Distance from American Pole.	Mean Temperature. Observed.	Calculated.	Difference.
Melville Island, -	5° 15′	1°.33	4°.39	+ 3°.06
Upernavick, -	12 15	16 .34	14 .81	— 1 .53
Omenak, -	13 58	16 .60	17 .33	+ 0 .42
Godhavn, -	17 08	22 .04	21 .92	— 0 .12
Godthaab, -	20 19	26 .07	26 .46	+ 0 .39
Fort Churchhill, -	20 58	25 .34	27 .38	+ 2 .04
Julianæshaab, -	24 25	30 .33	32 .17	+ 1 .84
Eyafiord, -	24 08	32 .16	31 .78	— 0 .38
Nain, - -	25 16	30 .03 †	33 .34	+ 3 .31
Okkak, - -	24 47	31 .00 ‡	32 .68	+ 1 .68
Quebec, - -	34 44	41 .90	45 .67	+ 3 .77
Cambridge, -	39 04	50 .36	50 .89	+ 0 .53
New York, -	39 53	53 .78	51 .84	— 1 .94
Philadelphia, -	41 08	53 .42	53 .27	— 0 .15
Williamsburg, -	43 40	53 .10	56 .09	— 2 .01
Orotava, - -	60 00	70 .11	71 .24	+ 1 .13
EQUATOR, W. L. 100,	80 00	81 .50	81 .5	0 .0
EQUATOR, E. long. 95.			81 .56	+ 0 .06

In the reasoning from which Humboldt estimates the mean temperature of the Equator, he appears to me to have taken for granted a very material fact. Having found a coincidence between the mean temperature of equinoctial America and equinoctial Asia, he concludes that the mean temperature of the Equator is 81°.5, and is uniform in every point of that great

* If we suppose that the observations in West Greenland, and those about Hudson's Bay and Labrador, are best fitted to give the position of the Pole, it is obvious, that it should be removed a little from the former, and brought nearer the latter, so as to be placed a degree or so farther south. This change would also produce a greater coincidence with Captain Parry's observations.

† For 1779—80. See *Phil. Trans.*

‡ Ibid. Ib.

circle; but as these are the very regions under the line where the temperature should be the same, in consequence of being similarly situated with regard to Canada and Siberia, no conclusion can be drawn until a similar temperature has been found on the African coasts of Benin and Loango. The heat under the Equator being thus supposed to be uniform, Humboldt felt himself entitled to conclude, that the colds of Canada and Siberia did not extend their influence to the equatorial plains,* and that between the tropics, the Isothermal lines are parallel to the equinoctial.

The theory which I have explained above, requires a different distribution of heat at the Equator. The *maximum* mean temperature of that circle should be 82°.8 in Africa, in order to give 81°.5 as the equinoctial temperature in America and Asia; and the difference of these values, or 1°.3, must be regarded as a measure of the influence which the colds of Canada and Siberia extend to the equatorial plains. Nor is this a mere theoretical result. I consider it as fairly deducible from facts furnished by Humboldt himself; and this distinguished traveller seems to have drawn from these facts the same conclusion, before he had deduced the uniformity of the equatorial temperature from a comparison of Asiatic and American observations.

In his *Political Essay on New Spain*, he makes the following remarks: "On the eastern coast of New Spain, the great heats are occasionally interrupted by strata of cold air, brought by the winds from Hudson's Bay towards the parallels of the Havannah and Vera Cruz. These impetuous winds blow from October to March; they are announced by the extraordinary manner in which they disturb the regular recurrence of the small atmospherical tides, or horary variations of the barometer, and they frequently cool the air to such a degree, that at Havannah, the centigrade thermometer descends to 32° of Fahr., and at Vera Cruz to 60.8 Fahr.—*a prodigious fall* for countries in the torrid zone."—vol. i. p. 65, Eng. edit. "The great breadth of the New Continent,—the *proximity of Canada,—the winds which blow from the north, &c.* give the equinoctial regions of Mexico and the island of Cuba a particular cha-

* *Edinburgh Philosophical Journal,* vol. iii. p. 263.

racter. One would say, that in these regions the temperate zone,—the zone of variable climates,—*increases towards the south, and passes the tropic of Cancer*," &c.—vol. ii. p. 410. "On the east coast of Mexico," he elsewhere remarks, "the north winds cool the air, so that the thermometer falls to 62°.6 Fahr.; and at the end of the month of February, I have seen it remain for whole days under 69°.8; while, during the same period, the air being calm, at Acapulco, it is between 82°.4 and 86°. The latitude of Acapulco (16° 50′) is 3° farther south than that of Vera Cruz; and the high Cordilleras of Mexico shelter it *from the currents of cold air which rush in from Canada upon the coast of Tabasco*," (Lat. 18°)—vol. ii. p. 148.*

From these quotations, it appears, that the cold winds from Hudson's Bay produce a very striking effect upon the climate even of the tropical regions. Rising in the parallel of 60°, and sweeping over the whole continent of North America, through an extent of 2600 miles, they retain their gelid influence even in the latitude of 18°. Can we suppose, then, that such winds as these cease all at once to impart their cooling energies to more southern zones. Acting with such power in the parallel of 18°, will they not refresh the opposite shores of the Pacific Ocean, and temper even the burning heats of the equinoctial line? Whatever law of progression we may adopt to represent the decreasing influence of these northern currents, in their passage towards the line, there is none which allows them the influence described by Humboldt on the coast of Tabasco, that will not extend that influence to the Equator itself.

Although the preceding views, respecting the distribution of heat within the Polar Circle, make the temperature much lower than had previously been supposed, yet when taken in conjunction with the results of the expedition under Captain Parry, they rather encourage the hopes which have been so reasonably entertained, of reaching the Pole itself.

Upon the supposition that there are *two* Isothermal centres in 80° of latitude, and that their temperature is — 3½° of Fahrenheit, the mean temperature of the pole of the globe will be about 11°, incomparably warmer than the regions in which

* See also vol. i. p. 75.

Captain Parry spent the winter. If an expedition, therefore, were to set out for Spitzbergen, and remain there for one or more seasons, till an opening should be found through the icy barrier which stretches from that island to the Greenland coast, there is every reason to believe that it would ultimately succeed.

If the Pole is placed in an open sea, the difficulty of reaching it entirely ceases; and if it forms part of a frozen continent, those intrepid individuals who sustained the rigorous cold of Lancaster Sound, could experience no hardship under a comparatively milder climate.

Hitherto we have supposed the two Isothermal Poles to have the same temperature, and to be situated on nearly opposite meridians; but this supposition is not rendered necessary by any of the phenomena, and we may obtain a better expression of the temperatures, by placing the Poles at different distances from the Equator, and ascribing to them different intensities. The existence of a cold and a warm meridian, is a proof that there are causes which powerfully influence the annual temperature, independent of the position of the earth's axis with respect to the sun; so that the effects which they produce can have no symmetrical relation to the pole either in position or intensity.

The two northern Poles of the terrestrial magnet, for example, are situated, the one 4° and the other 20° from the Pole, and they have neither equal intensities, nor opposite positions. Imperfect as the analogy is between the Isothermal and the Magnetic centres, it is yet too important to be passed over without notice. Their local coincidence is sufficiently remarkable, and it would be to overstep the limits of philosophical caution, to maintain that they have no other connection but that of accidental locality. The revolution of the two magnetic foci round the pole, the one in 1740 years, and the other in 860, has been recently deduced by Hansteen from numerous observations, and if we had as many measures of the mean temperature, as we have of the variation of the needle, we might determine whether the Isothermal Poles were fixed or moveable.

The idea of such a motion suggests an explanation of some of the most remarkable revolutions on the surface of the globe. There is no fact in the Natural History of the Earth better ascertained, than that the climate of the west of Europe was much colder in ancient than in modern times. When we learn that the Tyber was often frozen;—that snow lay at Rome for forty days;—that grapes would not ripen to the north of the Cevennes;—that the Euxine Sea was frozen over every winter in the time of Ovid;—and that the ice of the Rhine and the Rhone sustained loaded waggons;—we cannot ascribe the amelioration of such climates to the influence of agricultural operations.

The cold meridian which now passes through Canada and Siberia, may then have passed through Italy; and if we transfer the present mean temperatures of these cold regions, to the corresponding parallels in Europe, we shall obtain a climate agreeing in a singular manner with that which is described in ancient authors.

It is not, however, in the altered condition of our atmosphere merely, that we are to seek for proofs of a periodical rotation of climate. The impressions of the plants of warm countries, and the fossil remains of land and sea animals, which could exist only under the genial influence of the Temperate Zone, are found dispersed over the frozen regions of Eastern Asia; and there is scarcely a spot on the solid covering of the globe, that does not contain indications of a revolution in its animal and vegetable productions.

This interchange of the productions of opposite climates, has been ascribed to some sudden alteration in the obliquity of the Ecliptic, and even to a violent displacement of the Earth's axis; but astronomy rejects such explanations, as irreconcileable with the present condition of the system, and as incompatible with the stability of the laws by which it is governed.

Having thus endeavoured to establish a new law of the distribution of heat over the surface of the globe, it might be no uninteresting inquiry to investigate the causes which have modified, in so remarkable a manner, the regular influence of the solar rays. The subject, however, is too comprehensive, and too hypothetical, to be discussed at present. How far the general form and position of the continents and seas of the northern hemisphere may disturb the natural parallelism of the isothermal lines to the Equator?—To what extent the current through Behring's Strait, transporting the waters of warmer climates across the Polar seas, may produce a warn meridian in the direction of its motion, and throw the coldest points of the globe to a distance from the pole ?—Whether or not the magnetic, or galvanic, or chemical poles of the globe, (as the important discoveries of M. Oersted entitle us to call them), may have their operations accompanied with the production of cold, one of the most ordinary effects of chemical action ?—Or whether the great metallic mass which crosses the globe, and on which its magnetic phenomena have been supposed to depend, may not occasion a greater radiation of heat from those points where it developes its magnetic influence ?—are a few points, which we may attempt to discuss, when the progress of science has accumulated a greater number of facts, and made us better acquainted with the superficial condition, as well as the internal organization, of the globe.

Note A.—As Mr Playfair's observations were made in Windmill Street and Buccleuch Place, where the thermometer must have been influenced by the heat reflected from the opposite sides of these streets, I consider the mean of his annual temperatures, viz. 47°.8, as erring in excess, and have therefore preferred 46°.23, the result of Messrs Miller and Adie's observations.

This opinion respecting the temperature of Edinburgh is strongly confirmed by the following valuable and accurate observations, made and communicated to me by my friend Mr James Jardine.

Temperature of the Crawley Spring, situated 564 feet above the level of the sea.

1811, 30th January,	46°.5	Fahrenheit.
21st March,	46 .0	
18th April,	46 .2	
19th August,	46. 7	
	46.35	
Add for 334 feet above Merchant Court,	1.00	
Mean Temp. at Edinburgh,	47.35	

Temperature of the Black Spring, situated 882 feet above the level of the sea.

1815, 12th January,	44°.8 Fahrenheit.
1811, 31st January,	45 .0
1818, 4th February,	44 .6
1811, 18th April,	44 .8
1810, 17th September,	45 .0
1819, 8th October,	44 .8
1810, 31st December,	45 .0
Mean,	44 .86
Add for 652 feet above Merchant Court,	1 .95
	46 .81
Mean Temperature at Edinburgh from Black Spring,	46 .81
Mean Temperature at Edinburgh from Crawley Spring	47 .35
General Mean Temperature of Springs at Edinburgh,	47 .08

Art. X.—*Experiments relating to the Reflective Powers of Crown, Plate, and Flint Glass, with theoretical considerations.*—(Continued from last Number, p. 67.) By R. Potter, Esq. Junior, Associate of the Society of Arts for Scotland. Communicated by the Author.

Having proceeded so far in determining the reflective powers of metals and solid transparent bodies, it becomes desirable to consult in the original *Bouguer's Traité d'Optique*, a work now become very scarce in this country, and also Mr Herschel's *Treatise on Light*, in the *Encyclopedia Metropolitana*, which latter not being sold separate, I have found almost equal difficulty in getting a sight of it as of the former.

Bouguer was the first who undertook any photometrical investigations with success; he examined almost every subject to which photometry could be applied, and the address with which he adapted his instruments to their particular objects must always rank him amongst the most ingenious of experimenters; but the very great variety of subjects he undertook must be considered a misfortune, in preventing him paying that critical attention to the more important ones which would have contributed more to his lasting fame. In his examination of the reflective powers of the transparent bodies, glass and water, the greatest fault in the photometer he used appears to have been that it gave him *considerably* less light as reflected at small angles of incidence than it ought to have done, whereas the error in the plan I have described lies the other way, until the extraneous light is measured and allowed for. But I was particularly surprised to find that the numbers in his tables of the reflections by glass and water were not the actual results of experiments, but calculated from assumed formulæ,—a fact of which few would be aware from the manner in which they are generally given in English works. He says that he determined the reflections at some incidence, and particularly at the very high ones and the very low ones, and then sought a formula to calculate the numbers in the tables. He says he verified the numbers for water at several different incidences, and found them sufficiently near, but he does not speak so clearly respecting those for glass; but it will be seen on comparison that the numbers in his table do not differ more at the *higher* incidences from mine for *both* surfaces, than we should have expected from the insufficiency of his photometer for obtaining very critical results. The formula he used for water was this, $A + B z^3 + C z^6$ where z is the versed sine of incidence, counted from the perpendicular, (with him it was the co-versed sine as he counted the angle of incidence from the surface) and that for glass was $A + B z^3 - C z^6$, where z is the same variable as in the other.

The art of polishing specula was not sufficiently advanced in his day to give him a fair chance of discovering, in his investigations with them, the law of the variation of the reflective power; and though he says he used a mirror of a "très beau poli," yet, from his other remarks, there is no doubt that its polish was quite insufficient for the experiment. In mercury he would not have the same difficulty; and, accordingly, it was to me highly amusing and instructive to learn from his own words the pains it cost him to deceive himself when his experiments must have shown him that the real law for metals was different essentially from that for transparent bodies. In my former paper on the reflection by metals, I expressed an opinion that mercury, being in the fluid state, might follow a different law from the solid metals; but after reading Bouguer's reasons to himself, for not always finding the result as he wished, I have now no doubt how the fact stands. But his success in deceiving himself was more complete than after a long lapse of eighty to ninety years it was with myself, when, unexpectedly, I again fell upon the same thing. On repetition of the experiments, I awoke to the full value of a discovery perhaps of equal importance in physical optics with any of late date, and of which I have just reason to be highly proud, and this on several accounts; first, that I believe it is the experiment to settle the question of the rival theories on the nature of light, as to whether it is an emitted matter, or only consists of undulations or vibrations in a subtile ether. I think those candid mathematicians who have adopted the undulatory theory will acknowledge that the *essential* difference in the laws for metal and glass cannot be accounted for on any supposed differences in the state of the assumed ether in those bodies. The refractive power, on their hypothesis, having no other foundation than the metaphysical principle of Maupertuis, must exist wherever the ether should be of different density; but reflection, on the same hypothesis, has no other basis; and hence reflection and refraction must exist together; and it has accordingly been the custom to deduce the supposed refractive powers of opaque bodies from their reflective properties. But if it can be proved, strictly speaking, that the metals have no *refractive* power, and that reflection does take place in bodies which do not possess it,—what becomes of the undulatory theory?—We must acknowledge that it is not founded on the law of nature. But we have no need to take more than a single view of the laws for metals and transparent bodies to convince ourselves of the essential differences between them; and the latter being known to be possessed of refractive power, we seek in vain for any analogous expression in the laws for metals to that in theirs. On the Newtonian theory, where the effects are referred to the action of attractive and repulsive forces on dynamical applications, this difference in the laws is no more than we should expect from the different physical characters of the bodies, and is capable of being accounted for in as satisfactory a manner as any other optical phenomenon. I have reason also to be proud, that the fact does not appear to have been at all anticipated by the most eminent mathematicians of the present day; and I respectfully regard it as a mark of unprejudiced philosophical spirit in Dr Brewster, to admit into his scientific journal a paper which went to controvert the opinions to which the most eminent opticians were committed in print.

The undulatory theory has been spiritedly supported by some of the first French mathematicians, and they have brought to the inquiry the highest analytical talent. Now, if the formulæ which they have deduced from the undulatory hypothesis are found to give results at variance with observed phenomena, we are justly entitled to draw an argument from it, against the hypothesis from which they emanated, as being also at variance with fact. And if the consequences of the undulatory theory, as they have been developed in the hands of Young, Poisson, Fresnel, Arago, and Biot, do not accord with experiments, to whom must the defence of the theory be intrusted? M. Poisson and Dr Young, from considerations on this theory, have given for the intensity of the reflected pencil, at a perpendicular incidence, the following expression $\left(\frac{\mu' - \mu}{\mu' + \mu}\right)^2$ where μ' is the refractive index of the reflecting body, and μ that of the medium which is in contact with it. Then, for the reflection at the first surface of flint glass in air, we have $\left(\frac{1.570 - 1.}{1.570 + 1.}\right)^2 = .491$, or of every 100 rays incident 4.91, whereas experiment gives only 3.61 as reflected, in crown glass refractive index 1.524, we should have .0431, or of every 100—4.31, and experiment gives only 3.45 as really reflected, in plate glass, refractive index 1.517, we should have .0421, or of every 100 rays 4.21 reflected, and it is only in fact 3.38, these differences amounting to $\frac{1}{4}$ of the whole reflection are sufficient to set at rest all opinion as to the sufficiency of the formula. M. Fresnel's formula which, when adapted to light in its natural state, becomes $\frac{1}{2}\left\{\frac{\sin^2(i-i')}{\sin^2(i+i')}+\frac{\tan^2(i-i')}{\tan^2(i+i')}\right\}$ i being the angle of incidence and i' that of refraction, gives also quantities quite at variance with experiment, as will be seen by only considering, that, at the greatest incidence possible, the squares of the sine and tangent of $(i-i',)$ become equal to the squares of the sine and tangent of $(i+i')$ and the formula becomes $\frac{1}{2}$ $(1+1)$ or unity, so that the whole light incident ought then to be reflected, whilst it has been known since Bouguer's time that no known substance reflects *at any incidence*, all the light falling upon it, but always, even at the most favourable one, absorbs or transmits a very large proportion, and never less than about $\frac{1}{4}$ in any substance yet tried. At a perpendicular incidence the formula falls into the one of M. Poisson's above, and so is there liable to the same objections. Thus we see that the deductions of the ablest advocates of the undulatory theory stand opposed, not to my experiments only, but to those of the father of photometry, made near a century ago; and when we read in the works of Fresnel and Young, the unbecoming expressions they have used towards Newton, we must not blame them too harshly, but remember in charity, that, when so large a beam was in their own eye, they could not possibly see clearly to draw out the mote from Newton's. Mr Herschel, who has said of the undulatory theory, that "It is in fact, in all its applications and details, one succession of *felicities*, insomuch that we may almost be induced to say, if it be not true, it deserves to be so;" and has since spoken even more decidedly, will now see that the Newtonian theory has a claim to an apology from him.

At the close of my last paper, I said that I intended to try again a few of the measurements of the reflections for both surfaces of glass, as there was more irregularity amongst those there given than was admissible, to enable us to form a pretty certain judgment, as to whether the reflections were equal, if the rays were incident indifferently on the surface from the denser or the rarer medium. I have since obtained the following,

Incidence.	Gross reflected.	Extraneous.	Not reflected by the glass.	At the 1st surface.	Difference.	Value of $\frac{1}{x}$	Or of every 100.	Loss to be ad.	1st reflection at 2d surface of every 100 incident.
10°	8.33	.48	7.85	3.82	4.03	$\frac{1}{22.86}$	4.37	.25	4.62
30	9.46	.71	8.75	4.57	4.18	$\frac{1}{21.83}$	4.58	.25	4.83
50	12.16	1.11	11.05	6.65	4.40	$\frac{1}{20.21}$	4.94	.29	5.23
70	27.54	.55	26.99	16.01	10.98	$\frac{1}{6.64}$	15.06	.31	15.37

which are still more irregular than were to be wished; but we must remember that the error in the whole light for both surfaces is concentrated in that for the second surface, and we may consider fairly, that they go to prove the correctness of the law I have before enunciated.

To trace whether the values of the constants in the formula for solid, singly refracting, transparent bodies $y = a + \frac{c^2}{r+b-x}$ had any connection with, and dependence on, their *known* physical properties, it was required to determine still the refractive indices and specific heats in the specimens made use of in the experiments. The results in the table below were obtained by many repeated trials, both in the refractive powers and the capacities for heat, and I believe they may be considered very near the truth. Though some of the specific heats differ in some degree from what are found in books on chemistry, I have so far proved these numbers, by making the trials in the reverse ways, that I believe I have got them very near correct. I make the specific heat of flint glass much less than Mr Dalton has done; and some of the older chemical writers have made it even half as much again as I do. It is an experiment that requires some precautions; and I may some time give to the public the means I have taken to secure correct results. The refractive indices of the crown and plate glass I determined with Dr Wollaston's instrument, but was obliged to polish a small prism of the flint. It will be seen that I formed an erroneous guess at the refractive power of the plate glass, in supposing it of that sort which has higher refractive power than crown.

	Specific gravity.	Refractive index.	Specific heat for equal bulk.	Reflect. at highest incid.
Flint glass,	3.225	1.570	.43	72.07
Plate, ——	2.511	1.517	.39	74.26
Do. ——	2.425	1.501	.41	
Crown, ——	2.541	1.524	.38	75.77
Speculum metal,	8.900		.67	63.91
Steel, (hard)	7.800		.88	53.60 ?

I remarked at the close of my last, that transparent bodies like metals only reflected a portion of the light incident on their surface at the highest possible incidence, and in that on the reflective powers of metals, I had traced a remarkable connection between the light that steel and speculum metal reflect, and their capacities for heat. It is highly desirable to ascertain if the law will hold with other metals, and I think silver, and perhaps some others, may, by those who work in them as trades, be brought to a sufficient polish for determining the reflective power at the *highest* incidence, which is all that is required to prove the question; but it holds so far, as we see, in solid transparent bodies, when the reflection is uninfluenced by any perpendicular velocity in the rays, that I venture to propose the following law: *That in all solid bodies capable of receiving a truly smooth* (*or polished*) *surface, the reflective power, when uninfluenced by any perpendicular velocity of the rays of light, is a function of their capacity for heat.* It is probable that a similar law will be found in liquids, but it is clear that we must not expect to compare them at once with solids, whose particles are in so different a state of aggregation.

If we take the formula for glass $y = a + \frac{c^2}{r + b - x}$ we may put it under this form, $y = a + \frac{c^2}{\frac{\cos^2 i}{r + \sin i} + b} =$

$a + \frac{c^2 (r + \sin i)}{\cos^2 i + b\,(r + \sin i)}$ by putting for $r - x$ its value $\frac{\cos^2 i}{r + \sin i}$ if we put y' = the quantity of light which passes the surfaces and enters the glass by virtue of the particular direction of the motion of the rays to the surface, or between the limits $y = a + \frac{c^2}{r+b}$, and $y = a + \frac{c^2}{b}$, which is equivalent to removing the origin of the co-ordinates in the figure of the hyperbola to the point in the axis of y, whose ordinate is equal to $a + \frac{c^2}{b}$ and counting y' negative to y, and if we put y'' equal to the quantity reflected, which depends on the different values which the variables in the formula may receive, we shall always have $y' + y'' = \frac{c^2}{b}$ and $y' = \frac{c^2}{b} - y'' = \frac{c^2}{b} - \frac{c^2}{r+b-x} = \frac{c^2\,(r-x)}{b\,(r+b-x)}$ and $y' : y'' :: \frac{c^2\,(r-x)}{b\,(r+b-x)} : \frac{c^2}{r+b-x} :: \frac{\cos^2 i}{b\,(r + \sin i)} : 1 :: \cos^2 i : b\,(r + \sin i)$ or the portion of light which enters the glass is to the portion which is reflected on this consideration, as the square of the perpendicular velocity of the ray, to b multiplied into the radius plus the sine of incidence; and this is conformable to what Sir Isaac Newton has deduced for the action of the attractive force which produces the effect *called in this case* refraction.

The formula for metals may be put under this form, $y = a'(r - x) + b = a'\left(\frac{\cos^2 i}{r + \sin i}\right) + b'$ by putting a' for $-a$ and b for $b + a\,r$ where, a being a negative quantity, a' becomes positive, we see that the variable part in this formula is the reciprocal of the principal variable part in that for glass, and so any analogy as to refractive power in metals must be imaginary. Upon what we may learn when the second surfaces of transparent bodies have been examined photometrically at incidences producing total reflection, it is almost too much to hazard a surmise, but the similarity of their effect in what is denominated circular polarization to that called elliptical polarization in metals, would encourage the idea, that we may some day be able to unravel the secret of the mode of action of the more subtile properties of matter, and arrive at satisfactory conclusions on points which we can now hardly safely undertake.

Respecting the values of c^2 and b, it is clear that they must be such as to agree with the phenomena at the perpendicular, as well as at the highest, incidence. In the former case we have the intensity of the reflected ray $y = a + \frac{c^2}{r + b}$, and in the latter $y = a + \frac{c^2}{b}$, now there are an infinity of numbers which will give the same value for y in the latter; but being required to agree at the same time with the former, this takes away the indetermination when we know the value of a. When a remains indeterminate, we can only give the quantities such values as will agree best with the experiments, until further investigation shall enable us to deduce them from other considerations. If farther research shall confirm the law that the value of $\frac{c^2}{b}$ depends on the chemical property of the capacity of bodies for heat, we shall be one step nearer, and perhaps experiments on the high refracting glasses may give a clue to the whole phenomena of the perpendicular reflection.

The undulationists will soon find, no doubt, that they have only to allow for the effect which I attribute to the capacity for heat, to bring their formula for the intensity of the reflected pencils at the perpendicular and the highest incidence, pretty near to the correct quantities; but they will find rather more exercise for their ingenuity to accommodate them to the intermediate incidences.

ART. XI.—*Memoir on Barometric Instruments acting by Compression, considered particularly in their application to the Measurement of Heights; including some new Trigonometrical Determinations.* By JAMES D. FORBES, Esq. F. R. S. Ed. Communicated by the Author.

PART II.—*Practical Inquiries connected with the Measurement of Heights.*

As the basis of most of the following deductions of absolute height, and as affording an estimate of the degree of confidence to be placed in the trigonometrical operations, the results of which are to be recorded in the following pages, I deem it proper to commence with some account of the determination of the height of Colinton House above the mean level of the sea.

Most of these deductions, and many of the comparisons of the barometric results for which they were made, were completed some years since; and I only discovered extremely recently that I might, as far as the immediate result was concerned, have dispensed with some of the most laborious of these operations. Through the kindness of Mr Jardine, civil engineer, whose elaborately accurate results by actual levelling of the heights of many positions of interest, (and most of which are yet unpublished,) must be of the highest importance to those engaged in such inquiries, I have obtained the elevation of a point not far distant from Colinton House, and easily connected with it by actual levelling. The near approximation of my results to those obtained by Mr Jardine in this laborious, but accurate manner, and freed too from all the uncertainties of terrestrial refraction, must, as will presently be shown, form the best guarantee for the extreme precision of the base line which I employed, and which formed the foundation of all my determinations of heights in the Pentland range.

This, and other verifications to be pointed out in the course of the present paper, and which effectually exclude all idea of accidental coincidence, enable me to speak with some confidence on the use of the theodolite, and to express my opinion, that instruments of even small size, made according to the present very improved state of the art of dividing, are capable, under judicious use, of giving results of far greater delicacy than is commonly imagined. The theodolite, for the most part, has either been used as little better than a toy, the unpractised or negligent observer reading off the indications in a much looser way than even the graduation directly afforded, or omitting the precautions of reversing the instrument and comparing the verniers,—or else it has been treated like the circle of a fixed observatory; made unwieldy in dimensions, and its indications examined and reduced with the aid of every optical resource, and every mathematical refinement. I am confirmed by Professor Leslie * in saying, that under the one form it has been used much too little and too carelessly; and under the other, a prodigality of analytical refinements have been lavished upon it, which the precision of the most splendid instruments in existence could scarcely warrant. Were ordinary and small surveys carried on more extensively by the use of the theodolite, and less by the chain, far more precision would be attained: it is always desirable to abridge the sources of multiplied error on the field, even at the expence of more complex labour in the closet. And where larger triangulations are to be made, considering the accumulation of sources of error which may be fallen into in the measurement of a long base line, where the ground is not highly favourable, it is perhaps best for those unfurnished with costly and laborious apparatus for the measurement of such bases, to content themselves with one repeatedly verified and of moderate length, from which, with every attention to accuracy, a *secondary* base may be obtained by triangulation, from the extremities of which the great angles may be taken. From experience, I am disposed to recommend this *where the ground is not particularly favourable;* in this other case, I shall also have an example to offer of an opposite mode of procedure.

The instrument with which the following trigonometrical observations were entirely made is a theodolite by Troughton. It is a portable one of the usual dimensions, the circle being

* *Geometry.* Notes 4th edit. Pp. 427, 448.

five inches in diameter, and divided on silver by double verniers to single minutes. But although the division is not carried farther, with such an excess of accuracy (as it may fairly be termed,) is this graduation performed, that those accustomed to ocular subdivision will readily carry it down to 30″, and even 15″; which last I have constantly been in the habit of doing in all my surveys. When due use is made of this privilege, and combined with the full application of the double verniers, and the reversing of the telescope, (a sharp achromatic,) in the Y's, which give *four* readings for every angle in azimuth, we are prepared to elicit results of very considerable delicacy. The particular instrument in question is certainly one of uncommon merit, and the same minuteness could not be expected from all of its size, even by the same celebrated maker. Mr Troughton considered it one of superior excellence.

My trigonometrical deductions have been confined to the counties of Edinburgh and Kincardine; but those with the sympiesometer have been extended to many other parts of Scotland, from which a selection will be given in due order.

§ 1. *Trigonometrical Determinations near Edinburgh.*

The short base line already alluded to was measured near Colinton House with a steel chain by Troughton, and was found to be 1334.1 feet in length, by a mean of two measurements which differed only by a small fraction. Nearly parallel to this was the principal base from which the observations were to be made, terminating in two commanding points, one on the top of Colinton House, the other being the summit of Craig-Lockhart Hill, a picturesque eminence about three miles west of Edinburgh. The horizontal length of this base line, deduced from the former, was 4393.6 feet. To find the height of that extremity which terminated on the roof of Colinton House above the medium level of the sea, a triangle was formed by measuring two angles which bounded this known side, and which were directed to a cliff at the western extremity of Inchkeith, an island in the Frith of Forth. This triangle required every precaution and attention to minutiæ which the instrument was capable of rendering sensible, as it lay rather obliquely to the base line, and one of the computed sides was no less than 50413 feet, or almost ten miles in length. I had also to struggle against the uncertainties of terrestrial refraction, as the angle on which the final height was to depend was one of depression. To eliminate as much as possible errors of this description, six angles of depression were taken on different days, at different times of the day, and in different states of the tide; to each of these proper corrections were applied, the mean refraction being taken at one-tenth of the included arc, or 50″, and the correction for curvature amounting to 60.3 feet; the average of all the results being taken, and the local reductions made, the height of the lowest door step of Colinton House proved to be 389.6 feet above the mean level of the sea.

Since I did not possess then, as now, accurate verifications of this deduction, I proceeded to confirm it by deducing the height of Arthur's Seat above Colinton House, the altitude of which being accurately known, that of the latter might be inferred. Abandoning, therefore, the base of 4393.6 feet, I deduced a new one from the fundamental measured base. The configuration of the ground was such as to induce me to extend an extremely oblique triangle to Arthur's Seat, so oblique that I should not have given much weight to the observation itself, had it not proved eminently confirmatory of the results already obtained. Here the angle was one of elevation, not of depression, and it measurement, like the last, was frequently repeated. The length of the secondary base in this case was 3419.3 feet, and the distance of Arthur's Seat from its western extremity 22097 feet. The deduced height of Arthur's Seat above the door-step, was 429.6 feet, and the former, being by the result of a double levelling of my friend Mr Jardine, 822 feet above the mean level of the sea, we have 392.4 feet for the height of Colinton House, differing only *two feet and eight-tenths* from that by direct observation; a surprising coincidence, considering the nature of the operations. This confirmation applied, of course, only to the trigonometrical part of the operation, and, as the fundamental base was the same in both, could furnish no criterion of the accuracy of the lineal measure of length. Of this I have now to furnish the confirmation afforded by Mr Jardine's level already alluded to, and which I have only had an opportunity of reducing by a connecting operation of levelling within a few days. I had the satisfaction of finding the height of the point already discussed, to come out by this process equal to 385.5 feet, (the uncertainty of the precise point to which Mr Jardine levelled may be stated safely at a foot,) which certainly approximates as nearly to my original result of 389.6 feet, derived from a triangle with one side of ten miles long, as could have been anticipated.

The base from Colinton House to Craig-Lockhart being thus authenticated, the results deduced from it on the neighbouring eminences are next to be concisely noticed. In the first place, by numerous observations of the elevation of Craig-Lockhart top, its height above the eye on Colinton House was determined as follows:

By altitudes,	140.0 feet.
By depressions,	143.6
Mean,	141.8
By other observations,	142.4
Mean,	142.1
By my mean of Colinton House, or 391 feet height of the eye on the roof,	437.6
	579.7

By the deduction from Mr Jardine's observation, it would be $5\frac{1}{2}$ feet lower, or 574. feet. Three principal points were observed from the extremities of this base, (which extremities we shall denote by the letters—C, for Colinton, and L, for Craig-Lockhart,) forming part of the Pentland range, bearing the names of Kirkyetton, Allermuir, and Warklaw Hills, which we shall denote by their initial letters. The following angles were ascertained:

K L C	76°.57′.48″	Alt. 4°.23′.37″	L C K	83°.16′.52″	Alt. 5°. 7′.30″
A L C	62 .22 .15	Alt. 4 .39 .37	L C A	97 .39 .22	Alt. 5 .56 .45
W L C	16 .25 .24	Alt. 3 .1 .7	L C W	155 .37 .33	Alt. 3 .4 .28

The two first triangles being solved, we find

L K = 12911 C K = 12663 L A = 12748 C A = 11395

And the proper corrections being made for refraction and curvature, we deduce for the heights above the sea,

Kirkyetton.		Allermuir.	
By Craig-Lockhart,	1576.3	By Craig-Lockhart,	1622.9
By Colinton,	1571.7	By Colinton,	1620.5
Mean,	1574	Mean,	1621.7

Or taking Mr Jardine's level for Colinton, 1569.5 and 1616.2

Kirkyetton was measured by General Roy half a century ago; * but though provided with so good an instrument as a quadrant by Sisson, of twelve inches radius, he does not seem to have obtained such precise results as I have been able to do with Mr Troughton's theodolite, of which the circle has a radius of only $2\frac{1}{2}$ inches. He places the Calton Hill at 344 feet above Leith Pier, and Arthur's Seat at 803; whence, from the height of the latter given above, we may compute his estimate of the Calton Hill above the medium level of the sea to be 363 feet, certainly considerably too great, as the foundation of the column of the present astronomical circle is only 346 feet. His result for Kirkyetton is 1544 feet above Leith Pier, or 1563 above the medium level of the sea, which comes within six feet of my measure, (taking Mr Jardine's constant of the height of Colinton); but, from the general agreement of my measures with the results of levelling, I confess I am disposed to give the latter the preference.

It is only extremely recently that Mr Jardine has furnished me with a direct determination, by levelling, of the height of Allermuir, which he found to be 1615.96 feet above the medium level of the sea, differing only *three inches* from that just given, his determination of the level near Colinton being adopted. With regard to the triangulation of Warklaw, still greater pains were taken, as it was meant to be in turn a new station. All the three angles of this triangle were measured, the last being from the summit.

$$
\begin{aligned}
W\,L\,C &= 16°.25'.24'' \\
L\,C\,W &= 155\,.37\,.33 \\
C\,W\,L &= 7\,.56\,.37 \\
\hline
\text{Sum,}\ & 179\,.59.34
\end{aligned}
$$

* *Philosophical Transactions* at large, for 1777, p. 718.

Leaving an error of less than 9″ to be added to each angle. From thence the following were computed:

C W = 8987.2 feet. L W = 13115.0 feet.

The altitude of Warklaw, as deduced by Craig-Lockhart, - - -	920.3
By Colinton,	919.7
Mean,	920.0
Or with Mr Jardine's constant,	914.5 feet.

One principal object in making Warklaw a new station was to obtain an accurate result of the height of the Bonally Reservoir, on which I had made many barometric experiments, and the precise determination of which was also of great consequence to some delicate researches in theoretical meteorology, yet unpublished. Here again I was anticipated by the more trustworthy, and most indefatigable operations of levelling, conducted by my friend Mr Jardine; but it was only to afford another near coincidence in the results of our two methods. The following angles were taken, denoting by B, the chimney top of the water-keeper's house at Bonally Reservoir.

B C W = 31°.21′.52″ Alt. 3°.50′.30″ C W B = 87°.13′.34″ Alt. 2°.8′.56″

Whence we have B C = 10225 feet, B W = 5328 feet. And, applying the equations for refraction and curvature, we find,

Height of B above C	686.2 feet, above W.	200.3 feet.
Reduction to level of sea,	437.6	929.0
	1123.8	1120.3
Mean,	1122.9	
Chimney top above embankment,	19.5	
Embankment above the sea, -	1102.5	
With Mr Jardine's constant,	1097.	

By a mean of two detailed systems of levelling which agreed with surprising precision, Mr Jardine found this point to be

1095.45 feet above the sea; agreeing within eighteen inches of my determination.

Another object of the base line C W, was to approximate to the height of the Dalmahoy hills, of which previous estimates seem to have been considerably erroneous. In Boué's *Essai Geologique sur l'Ecosse*, the West and East Dalmahoy's are respectively stated at 680 and 660 feet; in Knox's County Map at 866 and 826. My observations were, from more than one circumstance, not so perfectly trustworthy as the others I have cited. I shall therefore not enter into particulars, but only state, that every correction was applied with perfect precision, and that I have good reason to believe that the probable errors do not exceed five or six feet.

	West Dalmahoy Hill 849 feet.—East ditto,	796 feet.
With Mr Jardine's constant	844	791

§ 2. *Determinations of Heights near Edinburgh with the Sympiesometer.*

A complete detail of all the observations I possess would form a lengthened, and I believe fatiguing work. Of the great number which I have made, I shall select those localities which have had their altitudes most frequently measured, or such as are intrinsically interesting, and, instead of giving the details of the individual readings, the principles which have guided the selection of the most unexceptionable, the observations of the barometer for the hourly variation, and all the reductions and logarithmic computations which have been founded on these, I shall digest the most material data and final deductions into small tables, which may convey all the most important information at a glance.

I beg to premise a few words upon the mode of computation of the heights employed. Mr Adie's fathom, or logarithmic scale of the instrument, has been almost universally employed, giving the approximate height by simple subtraction. The sympiesometer is peculiarly well fitted to have a scale of this sort, because the height of the oil in the tube cannot be observed with the precision of mercury in the barometer, and, therefore, the accuracy of a vernier would be superfluous. In my instrument, single fathoms of altitude could be observed and in some cases half of these by estimation. Under such circumstances, any refinements in the correction of the approximate height would be but a waste of time, independent of the inherent defects of the instrument, such as I have already shown them to be. Since the instrument professes to require no correction for the attached thermometer, (except that performed by the sliding scale,) such as the barometer stands in need of, the only correction worth mentioning is that for the temperature of the air. Now, until the correction for the effect of humidity is brought more generally into use than has yet been done, even with the aid of the elegant researches of Anderson and the convenient tables of Galbraith, any refinement in this correction were quite uncalled for even with delicate instruments. The expansion of perfectly dry air is exactly .00208 parts of its volume at 32° for 1° Fahr. But in moist air this goes so high as .0025, or .0026. We need not wonder, therefore, that barometric formulæ differ materially where the effect of humidity is overlooked. Laplace employed $\frac{1}{250}$ for 1° Centigrade, which corresponds to .0022 for 1° Fahr., and was probably selected for convenience of application. General Roy fixed upon .00243, as is generally stated; but Pictet carried it so high as .00251.* Under this diversity, I have always employed a rule of extreme simplicity, and for the heights to which it has generally been employed abundantly accurate, even for instruments of greater precision than the sympiesometer. I have added $\frac{1}{100}$ of the approximate height for every 4° that the mean of the detached thermometers exceeded 32°. Nothing can be simpler than the correction under this form.

As I am anxious to give some specific idea of the ordinary limits of error of the sympiesometer, I should wish to separate them in some measure from the preponderating error of graduation in the instruments, as explained in the former part of this memoir. For that purpose, I shall give along with each result, its deviation from the *mean* of the results, not from the real or geometric height, in hundredth parts of that mean. We shall thus have a pretty correct view of the results of observations on a variety of heights, which, unless reduced to a

* See *Ramond sur la formule Barometrique de la Mecanique Celeste.*

common standard, would not be directly comparable. As by far the most numerous observations on one point which I possess are those of the height of the Bonally Reservoir, I shall commence with them. Having been already detailed, I shall merely assemble the results in the manner I have just proposed. I shall arrange them in two departments, of hot and cold weather, in order to show that, as the adequacy of the barometric corrections for temperature is well established, in its present form, the instrument contains proper sources of error, which are brought out by extremes of temperature.

BONALLY RESERVOIR.—Height by levelling, 686 feet.

WINTER SERIES.

Exp.	Date.	Barom. below.	Att. Ther. below.	Att. Ther. above.	No. of readings above.	Height deduced	Error from mean.	Remarks.
	1829.							
1.	Dec. 31.	30.30	57	30	2	679 ft.	+ .05	Snow.
	1830.							
2.	Jan. 1.	30.27	55	25	2	642	—.01	do.
3.	Jan. 9.	29.53	58	36	1	618	—.05	
4.	Jan. 23.	29.63	52	30	1	639	—.02	
5.	Feb. 12.	29.49	59	39	2	945	—.01	
6.	Mar. 13.	29.50	64	42	1	618	—.05	
7.	Apr. 17.	28.64	64	47	1	581	—.10	Highly unfavourable.
Mean,		29.62	58.4	35.6	1.4	631.7	.041	
Mean excluding Exp. 7.		29.79	67.5	34.3	1.5	649.2		

SUMMER SERIES.

Exp.	Date.	Barom. below.	Att. Ther. below.	Att. Ther. above.	No. of readings above.	Height deduced	Error from mean.	Remarks.
	1829.							
1.	May 20.	29.81	63	54	4	671	+.03	
2.	May 26.	30.21	62	53	4	696	+.07	
3.	May 30.	29.89	65	55	7	634	—.02	Rather diffict.
4.	July 6.	29.32	67	58	2	698	+.08	Brisk wind.
	1830.							
5.	June 5.	29.37	63	53	2	675	+.04	
6.	June 26.	29.02	64	61	4	676	+.04	
7.	July 24.	29.23	68	67	11	678	+.04	Very difficult.
8.	July 29.	29.55	74	62	8	587	—.10	Fog.
Mean.		29.55	65.7	57.9	5.2	664.4	.054	
Mean excluding Exp. 8.		29.55	64.6	57.3	4.9	675.4		

These results are not a little important. In order to show the conditions under which the instrument was placed, and in order to afford data for the computation of its native irregularities, I have given in the three first columns the pressure by the sympiesometer at Colinton, or at the base of the 686 feet, and the temperature by which it was affected at the two stations. We see the wide range of temperature to which it was exposed in winter, amounting at a mean to almost 24°, the lower station being an inhabited room. The fourth column shows the number of observations at the upper station, which the circumstances of the case required, to give an estimate of the time and attention necessary to observation in the open air. Here we have a striking difference between the summer and winter series, the former amounting at a mean to 5.2 observations, which, taking the intervals at 5′, gives nearly half an hour of patient attention as requisite, the latter only to 1.4. It must, however, be observed, that where only one or two observations were made, the stay I made was generally not less than 15′ or 20′, and it was only being otherwise occupied which prevented me from making more during the time; as in cold or dull weather the same incessant attention is not necessary as in hot weather to catch the passing affections, provided sufficient time be allowed, which is almost always considerable. The next column contains the heights deduced; and the one following, their deviations from the *general* mean of the whole observations, or 649 feet, in decimal parts. The very striking result is the great and constant error of the winter observations, whilst the summer ones, instead of erring beyond the geometrical height in excess, as the others do in defect, still come short of it by a few feet; and in spite of all the difficulties and uncertainties which have been fully shown to accompany observations in warm weather, are by far more accurate than those made easily and with small chance of error in the *use* of the instrument at a colder season; the source of this error is to be sought in the range of temperature. The mean deviation is taken without regard to sign. Now, it is obvious from these observations that the summer series, so nearly answering to the required condition, (the geometric height,) the mean results of the data which affect the performance of the sympiesometer, are nearly those under which it was formed to act with accuracy, or, in other words, under which it was originally graduated. But farther than this, I am prepared to show by an analytical process, which will hereafter be given, that in order to satisfy the mean results of these two series, the neutral points of the instrument must be 30.03 inches, and 60.°4 Fahr., that is, the scale of pressure was graduated at a temperature of 60.4, the scale of temperature, under a pressure of 30.03 inches, by the mean result of pressures of the instrument, which, in order to accommodate it to that corrected for the index error, may be increased about 0.1. This is a very interesting result, even supposing it not strictly applicable to *all* other cases; more especially from the nature of the upper station, which, being rather in a basin on the north slope of a range, the barometer is known by experience to stand higher than on insulated summits,* and therefore the height comes out smaller than it ought to do. This is illustrated by the following observations:

Allermuir Hill.

Height above Sympiesometer at Colinton House, by levelling, 1206.5 *feet; by Trigonometry,* 1206.7 *feet.*

Exp.	Date.	Barom. below	At. below.	Ther. above.	No. obs. ab.	Height deduc.	Error from mean.	Remarks.
	1829.							
1.	May 20,	29.81	63	56	4	1286	+ 05	Brisk E. wind.
2.	July 6,	29.32	67	52	2	1246	+ 02	Brisk W. wind.
	1830.							
3.	June 9,	29.74	61	50	4	1150	—07	Wind N. E. moderate.
4.	July 15,	29.00	66	58	4	1265	+ 03	S. W. violent clouds and showers.
5.	Aug. 3,	29.13	67	57	5	1196	— 03	
	Mean,	29.40	64.8	54.6	3.8	1229	.040	

Now, as I mentioned in § 1 of the last part of this memoir, that, the neutral points being given, the true pressure would be found by increasing its deviation from the standard pressure, whether + or —, by $\frac{1}{100}$ part for every 5° that the ther-

* *Ramond, Mémoires sur la formule Barometrique de la Mecanique Celeste*, p. 49.

mometer was *below* the standard temperature, or, diminishing it by the same quantity if *above* that point, could we once obtain the neutral point accurately, the correction of any other observation for this source of error would be easily performed. By comparing this series, however, with the summer one of the Bonally Pond, it is pretty obvious even to the eye that the same neutral points cannot satisfy both. I shall quote some other observations which may be classified with the last, and which will enable us to extend this generalization.

Kirkyetton Hill.

By Trigonometry, 1169 feet above Colinton House.

Exp.	Date.	Barom. below.	Att. Ther. below.	Att. Ther. above.	No. obs. ab.	Height deduc.	Remarks.
	1829.						
1.	May 7,	29.32	64	51	6	1156	Bright sun.
	1830.						
2.	July 15,	29.00	66	56	4	1208	Violent S. W. wind and showers.

Capelaw Hill.

By Knox's County Map, 1080 feet above Colinton House.

Exp.	Date.	Barom. below.	Att. Ther. below.	Att. Ther. above.	No. obs. ab.	Height.	Remarks.
	1829.						
1.	May 20,	29.81	63	54	4	1052	Wind E. brisk.
2.	July 6,	29.32	67	55	4	1087	Wind W. brisk.

Warklaw Hill.

By Trigonometry, 505 feet above Colinton House.

Exp.	Date.	Barom. below.	Att. Ther. below.	Att. Ther. above.	No. obs. ab.	Height.	Error from mean.	Remarks.
	1829.							
1.	May 26,	30.21	62	53	3	520	+ .03	Wind brisk.
2.	Dec. 31,	30.30	57	31	2	517	+ .02	Snow.
	1830.							
3.	May 7,	29.16	65	55	3	534	+ .06	High S. wind.
4.	June 23,	29.22	62		3	467	— .08	*Imperfect.* Bright sun
5.	July 22,	29.28	69	64	4	487	— 03	Sultry. Wind W., moderate.
		29.63	63.0	50.7	3.0	505		

By taking the whole of the observations on Allermuir, Kirkyetton, and Capelaw, on the one hand, and the whole of those on Warklaw on the other, and subjecting the means to analysis, we may show that their conditions are satisfied, by assigning the values of 29.93 inches of pressure, and 44°.6 of temperature for the neutral points. The former may be considered as a very striking coincidence, and that the latter should be so much lower than that deduced from the Bonally observations, is certainly not wonderful, both on the account already alluded to, and when we consider that in the one case the pairs of results differed much in temperature, and very little in pressure, and, in the other, the circumstances were precisely reversed. Those, too, who are aware of the mode of eliminating two unknown quantities, must also know that there are two scales of corresponding values of the two quantities sought, the respective variations of which nearly satisfy the conditions of the problem, whilst the determination of the *precise* value is one of great delicacy, and requires a large series of observations. Had we possessed appropriate corresponding winter observations on these insulated heights, we should probably have attained a truer value of the neutral points. I shall confine myself to the results already detailed, which are the fullest I can offer on the Pentland range; nor can I permit myself to dwell on the nature of particular results, or to quote single observations on elevations, even though intrinsically interesting, as I have already given in last number, a pretty full analysis of the Bonally series, with illustrations of particular errors to be guarded against. The want, too, of precise geometrical determinations, has prevented my extending the list, as I have not found the results in Knox's County Map, however interesting in a general point of view, sufficiently precise for such delicate comparisons; and, in particular, I suspect that he has overrated the heights of the southern Pentland range, viz. Turnhouse Hill, Carnethy, East Black Hill, East and West Kip,—on which, however, my observations are too meagre to stand alone.

§ 3. *Trigonometrical Determinations in Kincardineshire.*

Of no range of hills, perhaps, in Britain has a more vague estimate been formed with regard to height, than the north-east district of the Grampians. Many of their summits bore the character of being much higher than they had any title to, such as Mount Battock, formerly considered to be 3465 feet above the sea, * (which, by its irregular number, gave some ground of confidence in its accuracy,) but proved by the measurements of Dr Skene Keith to be no more than 2600. Of other elevations extending into the high land of Aberdeenshire, very little has till lately been known, and that alone by the aid of the barometer; the same observer has proved that there are eminences in that remote district, capable of at least throwing a doubt upon the claim of Ben-Nevis to be the highest land in Britain.

I was induced by an extremely favourably position for measuring a base line, to determine the height of an interesting pass named from two heaps of stones which mark its summit, the *Cairn o'Mount*, which is the only communication for carriages across this mountain range for a great district in that direction.

It so happens that the great road between Stonehaven and Brechin, in traversing that extensive flat, known under the provincial name of the "How of the Mearns," extends for a long distance in an almost mathematically straight line. Along a portion of it, in September 1829, I measured a base with the greatest care twice over. By the first, it proved to be 2663.07 feet; by the second, 2662.77. The mean, or 2662.93 feet, was employed in the following deductions. There were two points to which my observations were principally directed, the Cairn Hill already mentioned, which form-

* See Col. Imrie's paper on the Geology of the Eastern Grampians; *Edinburgh Transactions,* vol. vi. Had this diligent observer been in the habit of using the barometer in his geological excursions, even in the roughest way, he could not have failed of detecting the enormous errors of this assigned elevation. It has been a principal object in the inquiries which form the substance of this memoir, to endeavour to furnish geologists and travellers in general with an instrument at once commodiously portable, prompt in its action, and accurate in its results. Without something more effective in these respects than any barometer we at present possess, it is vain to look for any great extension of this interesting class of facts. I trust before this memoir is concluded, to be able to offer some definite proofs of advancement towards this great object.

ed the sky line of the northern horizon occupied by the primitive range of Grampians; the other was the summit of a flat-topped chain bounding the southern side of the great flat of Kincardineshire, composed of red sandstone, and virtually forming a continuation of the Sidlaw Hills, but having the particular appellation of Garvock. The Cairn Hill is the only one which I have yet compared with the sympiesometer. Denoting the east and west extremities of the base line by E. and W., the summit of the western heap of stones already mentioned by C, and the top of Garvock by G, the following angles were measured.

$$CEW = 105.^\circ\ 2'\ 45''$$
$$CWE = 68.\ 30\ .30$$

WC = 22907 feet	EC = 22071 feet.
Alt. 3° 5′ 15″	Alt. 3° 11′ 37″

The correction for refraction being taken at $\frac{1}{10}$th of the included arc amounts to 22″; which being applied, there will result.

Height of C above *W*. end,	1231.3	Above *east* end,	1227.2
Correction for curvature,	+ 12.5		+ 11.6
Reduction for each station to a common one, which we shall call (F.)	— 19.5		— 15.0
Height of the Cairn above (F)	1224.3		1223.8

Mean of both	1224.0
Estimated height of (F) above the mean level of the sea, - -	294.0
Height of the Cairn Hill, -	1518.

By a similar operation, Garvock was found to be 709.1 feet above station (F,) or 1003 above the mean level of the sea.

The absolute height of station (F) was derived from four observations with the sympiesometer, which not being all made under favourable circumstances, must be taken as merely an estimate. The distance was from twelve to fifteen miles from the sea shore, and only the two first observations had the benefit of being repeated on a return to the first station; the hourly variations in the other cases being taken from previous and succeeding observations, they have therefore only half the weight given to them that they would otherwise have had.

Date.	Height.	Divisors.	Value.
1829. Sept. 21,	297 feet.	2	594
1830. Apr. 27,	267	2	534
1830. Apr. 22,	351	1	351
1830. May 4,	283	1	283
		6)	1762
	Estimated height,		294 feet.

On no subject have such gross errors remained uncontradicted as that of levels; the elevations which we have now determined at 1518, 1003 and 294 feet, have usually passed for 2000, 900, and about 100 feet.

§ 4. *Selection of Measurements by the Sympiesometer in Kincardineshire and other parts of Scotland.*

I. *Kincardineshire.*—The only observations which I shall quote here at present, beside those just given for the determination of the absolute height of the station (F), which was the second floor of Fettercairn House, are a comparison of the height of the Cairn Hill by this method, with the geometrical one.

1829, September 22d.—Height of West Cairn above the second floor of Fettercairn House, or station (F), by going and returning observations with the sympiesometer, 1240 feet. By trigonometry, 1224 feet. Error $= + \frac{1}{76.5}$.

Pressure below 29.18. Att. ther. 63.5. Att. ther. upper station, 49.6.

I have already given the general results of the measurement of the height of station (F) above the sea, and I shall not enter here into particulars, nor into those of observations made with the sympiesometer in some neighbouring counties, of which, in the great paucity of known facts connected with this curious subject, I do not at present possess means of verification.

II. *Peebles-shire.*—Although nearly destitute of geometrical determinations of the heights now to be noticed, yet some of these are in sufficient number to be instructively comparable with one another, or sufficiently interesting to render even an approximate determination not without value. The fundamental station, which I shall call (H,) in the parish of Eddlestone, was no less than thirteen miles distant in a direct line from Colinton House, and two ranges of hills intervened. They had the benefit, however, of nearly contemporaneous observations with the barometer at the other station. The following are four determinations :

Date.	(H) above Colinton.	Deviat. from mean.
1829. June 4,	280 feet.	—.02
1829. June 22,	307	+.07
1829. Oct. 9,	294	+.02
1829. Oct. 15,	263	—.07
Mean,	286	.045

(H) above the sea 701 feet.

In Knox's County Map of Edinburgh, Eddlestone village (with which station (H) is nearly on a level,) is marked 750 feet above the sea ; but I have good reason for believing that estimate to be too great, especially from Mr Telford's level of the height of Peebles Bridge, in the course of a survey for a projected canal.

The first subsidiary point I shall notice was under cover, and therefore the instrument was not so liable as usual to the fluctuations of external temperature. I shall denominate it by the letter (G).

June 1829. (G) above (H).

Exp.	Barom. below.	Att. Ther. Below.	Att. Ther. Above.	No. of obs. above.	Height.	Var. from mean.
1	29.20	58	59	2	204	+.02
2	29.64	57	62	2	187	—.06
3	29.72	66	63	1	175	—.12
4	29.10	57	64	5	235	+.18
5	29.23	59	61	4	194	—.02
			Mean,	2.8	199	.08
			Above the sea,			900 feet.

The next station was a step higher; we shall denominate it by (S) and we place beside it a table containing two values of the height of a considerable piece of water, named Athelstane Loch, or West Loch.

(S) above (H.)
June 1829.

Exp.	Height.	Deviation from mean.
1	305 ft.	—.02
2	304	—.02
3	326	+.05
4	308	—.01
Mean,	311	.025

Above the sea, 1012 feet.

West Loch above (H.)

Exp.	Height.
1	293
2	278
Mean,	285

Above the sea, 986 feet.

Mr Knox places West Loch 1012 feet above the sea; but, by reducing his height of (H) to my estimate, it will be brought even lower than that I have just given, and the same applies to the reduction of his other levels in this district. For example, he places Jeffries' Cross, which forms the summit of Dundroich, or the Druids Hill, at 2044 feet above the mean level of the sea. By very careful observations with the sympiesometer on the 6th of June 1829, I made the highest summit, or Jeffries' Cross, 1269 feet above (H,) and the western summit 1112 feet, or 1970, and 1813 above the sea. The former, when increased 50 feet for the difference of Mr Knox's level, near (H,) and mine, gives a pretty near approximation. Dundroich is generally stated at 2100 feet, but, until better observations be obtained, I would substitute 1970. It is hardly necessary to observe, that, in order to obtain the results with Mr Jardine's level of Colinton House, we must deduct 5.5 feet from the preceding heights above the sea.

III. *Stirlingshire.*—The following measurement of the height of Benlomond is so instructive that I shall give it in detail.

Aug. 10, 1830.—Weather very fine; sultry; bright sunshine, with passing clouds; almost calm even at the summit.

The following observations were made at the level of Loch Lomond at Rowardinnan. The instrument was sheltered both by brushwood and an umbrella from direct radiation.

			Att. Ther.
12^h 0	29.675	= 191 Fath.	63.8
5′	29.42	= 226	63.1
10′	29.40	= 229	62.8
20′	29.31	= 242	61.4
30′	29.325	= 239½	61.6
33′	29.34	= 238	62.3
39′	29.30	= 245	62.4
43′	29.31	= 242	62.4

Thus, after three quarters of an hour of observation, some doubt yet remained as to the actual pressure indicated at the level of the loch: but even were a traveller's patience unlimited, his time is not; I was, therefore, obliged to set out, and selected 242 fathoms as the probable true pressure by the logarithmic scale.

On the summit of Ben Lomond circumstances were more favourable.

		Att. Ther.			Att. Ther.
3^h 45′	690½ Fath.	48.5	4^h 10	700 F.	50.5
50′	705	50.0	25	696	50.4
4 0′	695	56.2	30	696	50.4
5′	700	56.?			

Rowardinnan, 3 feet above the lake.

6^h 15′ 10′ open.	29.29	= 246 F.	62.9
20′	29.44	= 224½	60 6
23′	29.44	= 224½	60.4
26′	29.46	= 220	59.7
35′	29.46	= 220½	57.8

There can be very little doubt that one, if not both the series of observations of Rowardinnan failed of giving the true pressure, especially as I had reason to believe that the barometer was sinking during the day, and not rising, as above indicated. Be this as it may, we can only take the observations as they stand before us. By reducing them in the manner already explained, we shall find the height to be 2965 feet. The geometrical height by the trigonometrical survey is 3177 feet above the level of the sea, or 3145 above that of the lake. This result, therefore, errs 180 feet in defect.

IV. *Argyleshire.*—The last example which I shall select at present, is a determination of the point in the great military road through Glencroe, known under the name of " Rest and be thankful," from an inscription left there by the soldiers who made the road, and which forms its summit level. The height of this interesting pass may very probably be known, but I am not in possession of any previous determination of it. Observations with the sympiesometer were made, August 17, 1830, at Cairndow, on the bank of Loch Fyne, and at Tarbet, on Loch Lomond, both being reduced to the *mean* level of the sea; the variation of pressure was obtained for the interval about the middle of which the instrument had been observed at the summit level of the military road; and the proper reductions being made, the height of the latter proved to be 874 feet.

§ 5. *On the Mean Error of Measurements by the Sympiesometer.*

In several of the preceding observations, I have given the errors in decimal parts of the mean results, wishing to separate in some measure the predominant error arising from the graduation of the instrument. It is clear, however, that by this method, especially where the variation of circumstances is considerable, we can arrive at no fair result, and that each observation should be corrected for the known deviation of its conditions from the neutral points of the instrument; the deviation of these from their means would then represent the real uncertainties of the measurement, freed from errors admitting of numerical estimation. We might, then, by the aid of the doctrine of probabilities, compute the probable error attaching to a single observation, or to a series. In the present case, however, such a result would be little worth the labour, unless it were desired to show the amelioration produced on the instrument by putting it under a new form. But upon this I am not yet prepared to enter. We may, however, remark, that in all such estimations there are two distinct classes of error, which it would require extensive series of observations satisfactorily to separate. One is a *function* of the height measured, the other is not. The former is common to all species of barometric levelling, depending on the variable condition of the atmospheric strata;—the latter is due to particular sources of error in the instrument, and may be as great (and is frequently greater) in a very small elevation than in the highest. Hence we should do the instrument injustice by confining our experiments to a small scale. The possible error might then amount to a large fraction of the result. It is to this object that all our labours to improve the instruments themselves are directed, whilst the determination and correction of those errors which (generally speaking) are dependent upon, or functions of, the height itself, is one of the most interesting problems to which the attention of philosophers has been directed; and their researches upon this subject alone would have been sufficient to immortalize the names of Deluc and Shuckburgh, and Playfair, and Laplace, and Ramond.

§ 6. *On the application of the Sympiesometer to travelling observations, or continuous levelling.*

The very interesting results which we obtain from ideal sections of a district of country by barometric levelling, give us an estimate of their importance, equalled only by their rarity. The fine examples of Humboldt, executed in tropical regions, leave at a distance any thing we can hope to achieve in the mutable atmosphere of our northern climate. The barometer is very seldom stationary, and almost as rarely are its variations uniform, even for a few hours together. If, therefore, in travelling observations, we have no fixed register for comparison, (and a register at any considerable distance from the point of observation will hardly answer our purpose,) we must laboriously discover by actual observations, at a moderate interval of time, the variation of pressure even several times a day. Now, to attain this object, it is manifestly of paramount importance that the action of the instrument shall be prompt, and true to the smallest fraction which it is capable of indicating. In this respect, I found the sympiesometer extremely defective in some pretty extended experiments which I made on this application of it in August and September 1829. The period was certainly particularly unfortunate for such an essay, as it was exactly during the continuance of those memorable floods which Sir Thomas Lauder's interesting work has rendered familiar, as far as Morayshire is regarded, but which also extended with great violence over the whole eastern district of Scotland. Under such circumstances, many of the most interesting periods of observation were irremediably destroyed by the rapid fluctuations of the barometer; but in some cases, even under every disadvantage, I obtained pretty accurate results. Throughout a journey of several hundred miles, the instrument was most sedulously observed; and, partly from inconsistencies of observation, partly from the want of ascertained elevations for comparison, even the most careful reduction of the whole of these observations has not furnished me with results so *generally* correct as to warrant me in publishing them. Nor even under favourable circumstances could I expect results worthy of great confidence, without having an instrument by which we might from a single hour's observation obtain a very correct value for the actual change of pressure; and this is so far from being the case with the sympiesometer, that it would be often difficult even to ascertain the *direction* of the variation. But I cannot close these remarks without bearing testimony to the portability of the instrument, which went through the journey just alluded to, embracing many of the middle and eastern counties of Scotland, in an open gig, and during the most variable weather, without meeting with the slightest accident.

Postscript.—Since the first portion of this paper was printed off, I have ascertained more precisely, by the aid of a spirit level, the height of the chimney top of the waterkeeper's house at Bonally above the embankment, which (not looking for so close a coincidence with the result by levelling) was roughly stated at 19.5 feet in page 337. I find it, however, to be 21.2 feet, which reduces the actual height with Mr Jardine's constant to 1095.3 feet, agreeing within *two inches* of the result by levelling.

Art. XII.—*Observations on the Temperature of Springs made during a voyage to Mount Elbrouz in Caucasus.* By M. Kupffer, Member of the Academy of Sciences of St Petersburg.

The following interesting article forms the fifth section of M. Kupffer's valuable account of the voyage to Mount Elbrouz, undertaken by command of the Emperor of Russia. The general report on the voyage is drawn up by M. Kupffer, who has been so kind as to communicate it to the Editor. The other sections will be published in successive articles.

" In a former memoir, (See this *Journal*, No. iii. New Series, p. 134, and No. iv. p. 251,) I showed, from a great number of observations collected by different observers and by myself, that the distribution of heat in the interior of the globe, and at a small distance from its surface, was different from that which is observed at the surface, or rather in the air which surrounds it. I afterwards demonstrated that we might express, by a very simple formula, the decrease of heat from the Equator to the Poles in the stratum of the terrestrial crust where the oscillations of temperature vanished,—a heat which is carried to the surface by springs of a nearly constant temperature. This formula has, I have since been informed, been given in the *Mecanique Celeste*, where Laplace says that it represents with sufficient accuracy the decrease of the mean temperature of the air from the Equator to the Poles. It is easy to convincé ourselves, that it expresses much better still the observations hitherto collected relative to the temperature of the ground.

The facts which I have consigned in the memoir already quoted, were chiefly collected in the west, north, and east of Europe. Our journey to Caucasus gave me an opportunity of increasing the number of them, by observations collected in the south-east. I have at the same time been able to determine more exactly the decrease which the temperature of the ground experiences relative to its elevation, by observations made at points which differed greatly in level. I shall first give an exposition of these observations, and shall then connect them with those made in other parts of Europe; and I shall endeavour to unite these facts under a single point of view.

All our observations were made with thermometers carefully calibred after the method explained by M. Bessel in his collection of astronomical observations. We always chose copious springs, and we almost constantly observed several springs at the same point, and sometimes at two epochs sufficiently distant to ascertain their variations of temperature. The temperature of wells was observed only to show that their temperature depends on particular circumstances, and is often independent of that of the ground. Observations of this kind cannot be employed in the determination of the temperature of the ground. The following are the observations themselves :

1. *St Petersburg*. Lat. 59° 56½′, long. 27° 59½′ east of Paris. —An abundant and slightly ferruginous spring at the village of Okhta, in the park of M. Koucheleff-Besborodko, in the months of May and June, in degrees of Reaumur, - 4°.9

2. *Moscow*. Lat. 55° 45′, long. 35° 17′, elevation 600 feet nearly.—Spring of Pakrovsky, a small village, 18 versts from Moscow, on the left of the road to St Petersburg, 10th September, - - - - 5°.2

3. *Sadonsk*. Lat. 52° 20′, long. 36° 35′.—An abundant spring rising from a very porous calcareous rock, June, - 5°.9

4. *Moskovskaïa Krepost*. Same latitude as Stavropol, and thirty versts to the west of it.—Several springs which rise from a calcareous rock, the most copious of which and the coldest was at the end of June, - - 8°.5

5. *Stavropol*. Lat. 45° 3′, long. 39° 39½′, elevation 1800 feet nearly.—Several abundant springs had in August a temperature of from 8°.8 to - - - 8°.5

6. *Taganrog*, on the sea of Azoff. Lat. 47° 12′, long. 36° 37′. —Several very copious springs observed by M. Elsingk with a thermometer not verified, - - 10°

7. *Nicolaieff*, a town situated very little above the Black Sea, and distant from it only seventy versts. Lat. 46° 58′, long. 29° 40′.—A spring at Spasky near the observatory, temperature not constant, - - - 9°.8

8. *Hot Springs of the Caucasus*. Lat. 44° 2′, long. 40° 42′, elevation 1300 feet.—A very copious spring at the foot of Machouca in August, - - - 10°.6

9. *Stone Bridge on the Malka*. Lat. 43° 45′, elevation 2500 feet.—Copious spring, but exposed to the sun, July, 8°.5

10. Three small springs in a narrow close valley, between high mountains, at some distance from the stone bridge of Malka, where we were 5800 feet above the sea, 3°, 3¼°, and 4°

11. *In our Camp on the Upper Malka*, at the foot of Elbrouz. Lat. approx. 43½°, elevation 7700.—A spring of sweet water on the banks of the Malka, - - 3°.3

A spring of acidulous water, - - 3 .5

12. *On the Bermamac*. Elevation obtained 7500 feet.—A small spring observed during rain, - - 4°.2 5

The points 8 to 12 are situated under meridians very little different. At *Sadonsk*, No. 3, the temperature of a well was found to be No. 2. At *Isvali*, situated to the north of Voronege, in the parallel nearly of 52° 40′, the temperature of a well six metres deep was exactly the same. This temperature ought to be below the mean temperature of the country.

The observations, No. 8, 9, 10, 11, give us a new determination of the decrease of the temperature of springs in a vertical line. The four points at which these observations have been collected, being little distant from each other, we have only to compare the differences of their respective elevations. In this way we obtain the three following data:

	Diff. of Temp. Reaumur.	Diff. of Level.
No. 8 and 9	2°.1	1,200 feet
No. 8 and 10,	7 .3	4,500
No. 8 and 11,	7 .3	6,400

The two first data give a decrease of 1°, Reaumur in every 600 feet. The third differs too much from the two first to be admitted into the same calculation. We should observe that the springs, No. 8, 9, and 10, rise upon similar strata, that is upon a calcareous rock; while No. 11 issues from a trachytic soil. This last gives a decrease of 1° Reaum. for 877 feet. The mean of these numbers is 1° for 740 feet. The reduction of 1° for every 600 feet of height rests on too few observations to be adopted finally; but it is sufficiently exact for observations at small altitudes. After making this reduction, we obtain the following results:

	Lat.	Long.	Temp. ground at level of the sea.
St Petersburg, -	59°57′	28° 0′	4°.9 Reaum.
Moscow, -	55 45	35 17	6 .2
Taganrog, -	45 3	39 40	11 .0
Stavropol, -	47 12	39 37	10 .0
Nicolaieff, -	46 58	29 40	9 .8
Hot springs of Caucasus,	44 2	40 42	12 .7
Malka Bridge, -	43 45		12 .7
No. 10, -			12 .7 to 13.7
Camp on Malka,			16 .1

All these observations were collected in a season when the temperature of springs varied little from its mean value throughout the year. In the memoir already quoted, I have shown that, by uniting the observations made under the same meridian, the decrease in the temperature of the ground, in virtue of the latitude, is very well represented by the formula

$$a - b \text{ Sin.}^2 l = t,$$

t being the temperature of the ground at twenty-five metres depth, (the point where it begins to become constant,) l the latitude of the place of observation, and a and b constants which must be determined by observation.

We shall begin our calculations with the meridian of *St Petersburg;* but as we have made only two observations on this meridian, we shall use the observation at *Cairo* under the same meridian, where the temperature of the ground is 18°.0. In this way we have the three following equations:

$$a - b \text{ Sin.}^2 (30° \ 3') = 18°.0$$
$$a - b \text{ Sin.}^2 (46 \ 58) = 9.8$$
$$a - b \text{ Sin.}^2 (59 \ 57) = 4.9$$

The combination of which, by the method of least squares, gives

$$a = 24{,}40$$
$$b = 26{,}41$$

These values of a and b, substituted in the preceding equation, give the following values of t:

St Petersburg,	$t = 4°.7$	Calc.	4°.9 Obs.
Nicolaieff,	$t = 10.3$		9.8
Cairo,	$t = 17.8$		18.0

By combining the observations of *Moscow, Taganrog*, and *Stavropol*, we obtain in the same manner,

$$a = 24.20$$
$$b = 26.36$$

Whence we deduce the following results:

Moscow,	$t = 6.2$	Calc.	6°.2
Taganrog,	$t = 10.1$		10.0
Stavropol,	$t = 11.0$		11.0

The following table contains the values of a and b for *six* different meridians, the only ones from which I could collect a sufficient number of observations.

	a	b	$a-b$
Havanah, 84° 43′ W. of Paris,	24.0	33.7	— 9.7
Meridian of Paris, -	21.3	20.9	+ 0.4
Upsal, 15° E. of Paris, -	24.4	25.6	— 1.2
Petersburg, 28° E. of Paris,	24.4	26.4	— 2.0
Moscow, 35° E. of Paris,	24.2	26.4	— 2.2
Bogoslovsk, 60° E. of Paris,	22.9	27.5	— 4.6

These values of a and b enable us to calculate the different points under the above meridians, where the temperature of the ground is successively equal to 0°, 5°, 10°, &c. and the preceding formula gives

$$\text{Sin } l = \sqrt{\frac{a-t}{b}}, \text{ or } \cos 2l = 1 - 2.\frac{a-t}{b}.$$

The following table has been calculated by means of this formula.

Temp. of ground.	Corresponding heat under the meridians of 85° W.	0°	15°.	30	35	60
0	57°33		77°30′	74° 2′	73°13	65°52
5	48 40	62 1	60 31	59 1	58 31	53 47
10	40 8	47 20	48 36	47 37	47 10	43 14
15	31 7	33 18	37 18	36 38	36 11	32 25
20	20 9	14 27	24 30	24 6	23 30	18 57

Hence we may easily draw upon any map lines through the points of equal temperature. In this way, we shall obtain curves which I have called *Isogeothermal lines*, to distinguish them from the Isothermal lines, whose curvatures they in general follow, but from which they sometimes separate considerably at several points. In the north, for example, the Isogeothermal lines are more distant from the Equator than the corresponding Isothermal lines, or rather, which is the same thing, the temperature of the ground is higher in the north than that of the air. In the vicinity of the tropics, on the contrary, the mean temperature of the ground is lower than that of the air, as the observations of MM. Humboldt, Buch, and others have decidedly proved.

If we call the mean temperature of a surface unequally heated, the sum of the mean temperature of all its points divided by this number, it is evident that the mean temperature of the surface of the ground in our globe cannot be different from that of the atmosphere which touches it; but, nevertheless, the distribution of the heat of the ground may, at a certain depth, deviate considerably from that of the temperature of the air—the propagation of heat in a solid body, and a bad conductor, like the earth, not being submitted to the same laws as the propagation of heat in air, the theory of which has not yet been given. I abstain at present from any farther developement of these ideas, which I shall submit in another memoir to a new discussion and a more profound examination."

Art. XIII.—*Observations on the recent adjudication of the Wollaston Medal to Mr William Smith for his Geological discoveries.*

In the account given in the *Spectator*, February 26, 1831, of the proceedings at the anniversary meeting of the Geological Society, held on the 18th February, the First award of the Wollaston Medal is announced in the following terms:—

" The late Dr Wollaston having bequeathed to the Geological Society L. 1000, the interest to be employed annually in recompensing or encouraging geological inquiries, and the Council having directed a medal to be struck, bearing the impress of Dr Wollaston, the first of these, together with a sum of money, has been adjudicated to Mr W. Smith. Before the delivery of this medal, the President gave a chronological account of the discoveries of Mr W. Smith, by which he justified the terms of the following award, viz. 'That the first Wollaston Medal be given to Mr W. Smith, in consideration of his being a great original discoverer in English Geology, and especially for his having been *the first to discover and to teach the identification of strata, and their succession, by means of imbedded fossils.*' "

The scientific readers of this *Journal* may remember that when the Royal Society of London adjudged, for the first time, the two Royal Medals to Mr Dalton and Mr Ivory, we expressed our doubts of the propriety of the principle which the society seemed to have adopted. We were of opinion that the Royal Medals were founded for the purpose of promoting new discoveries, and not of rewarding old ones, and we ventured to mention the names of several distinguished individuals who would be entitled to receive the Royal Medals in future years if the society continued to act upon the principle with which they set out. (See this *Journal*, No. xii. p. 369, April 1827.) Whether the Royal Society were influenced by our statement, or discovered of their own accord the error into which they had fallen, we have no means of knowing; but it was gratifying to observe that they abandoned a principle of adjudication which had not one single argument to recommend it.

Three years afterwards Mr Babbage, in his able and useful work on the "*Decline of Science in England*," went a step farther than we had done, and showed that the adjudication of the medals to Mr Dalton and Mr Ivory for discoveries made long before his Majesty had founded them, was in direct violation of the rules which the Council had laid down, and had actually transmitted to the King through the Secretary of State for the Home Department.

Although in this breach of their own rules the Royal Society acted from the best intentions, and with the view of promoting the interests of science, yet the almost universal decision of the public against the principle which they followed, and the fact of their having themselves renounced that principle in their future adjudications, must be held as at once a proof and a confession of error.

Under these circumstances, we were surprised to observe that the *Geological Society* had committed the very same error, by adjudging the first Wollaston Medal to Mr Smith for discoveries made long before it was founded. As we do not know the terms of Dr Wollaston's bequest, or the rules which the Geological Society have framed for their guidance, we are unable to determine whether or not they have acted in conformity with them; but even if the terms on which Dr Wollaston bequeathed the money are sufficiently vague to authorize the principle of adjudication which has been adopted, we do not hesitate to condemn the principle, not only as highly injurious to the progress of science, by withdrawing a powerful stimulus from those who are engaged in geological pursuits, but as preventing the wealthy patrons of science from establishing other prizes in future.

When the principle of adjudicating medals for rewarding old discoveries is once admitted, the difficulty is to fix the retrospective limit at which we are to stop. The principle, indeed, does not admit of a limitation, for there is the same reason for rewarding a discovery *sixty* years old as there is for rewarding a discovery made only *ten* years ago.

The principle, however, is in itself untenable, and if it is followed out, the medal must be adjudged in successive years to Sir James Hall, Professor Buckland, Professor Sedgewick, Dr MacCulloch, Dr Hibbert, Mr Poulett Scrope, Mr Lyell, Mr Murchison, &c.

If foreigners are admitted to competition we might add a list of distinguished names, with Von Buch, Cuvier, Brongniart, Cordier, &c. at their head; and until all these veterans had been crowned by the Geological Society of London, every young aspirant after fame would be deprived of the just recompense of his labours.

It is, we think, peculiarly unfortunate that, in the present degraded and declining condition of English science, the few rewards which genius can command are not judiciously conferred. Some individuals who are actively engaged in scientific inquiries are not even aware of the existence of such rewards, while others are wholly ignorant of the conditions upon which they are founded, and the periods at which they are awarded. In place of its being left, as it now is, to the different societies to whom the prizes belong to find out the individuals who deserve them, these individuals should be encouraged to bring forward their own claims, and to transmit memoirs and discoveries in express competition for the prize, along with certificates or opinions of eminent scientific men respecting the merits of their discoveries. As the council of the society, which has the power of adjudication, cannot always be supposed to contain the individuals that are most distinguished in any branch of science, they would, by the method which we suggest, not only be enabled to find out the most meritorious individual, but they would enjoy the satisfaction of having their own opinions confirmed by those of the most eminent philosophers in the land.

We make no individual allusions when we state that the different prizes founded in Great Britain for the encouragement of science have not yielded those benefits which they were so well fitted to bestow. Of whatever blame has been thus incurred, the Royal Society of London must take to itself the greatest share, only because it has had the greatest number of prizes to confer. Even in the golden age of that institution, when it was adorned by Davy, and Young, and Wollaston, the council often found itself at fault in the adjudication of their prizes; and in place of consulting individual members of the society, who could have aided them with their opinions, we can state, on the highest authority, that they sought for an approval of their adjudications from the philosophers of Paris, and that, too, under circumstances where it was peculiarly improper to have done so.

In the year 1820, the council of the Royal Society ventured upon an innovation, by adjudging the Copley medal to Professor Oersted for his celebrated electro-magnetic discoveries. This medal had been hitherto appropriated to the best paper or series of papers *printed in the Philosophical Transactions*, and its history was identified with that of the Royal Society itself. To divert it, therefore, from its original purpose, and to give it to foreigners, however distinguished, was a stretch of power which cannot be too severely blamed. The Royal Society of London was not more called upon to do honour to the distinguished Danish philosopher than any other learned academy in Europe; and if they did feel it their duty to outdo other institutions in liberality, they might have done it *out of their own funds*, without breaking through a rule consecrated by long custom, if not guarded by the positive terms of the bequest, and thus depriving the author of the best paper in the *Transactions* for 1820 of his just reward.

The Society cannot defend itself by stating that there was no paper of sufficient merit in the Transactions for 1820. Mr

Herschel's memoir "*On the Action of Crystallized Bodies on Homogeneous Light*," justly merited the Copley medal of 1820; and though he did receive the same medal for 1821, yet this forms no justification of the rash innovation which we have mentioned. The same deviation from established usage has been made in subsequent years; but it is enough for us to have pointed out the first false step of the Society.

There is one other remark on these prizes which we think of some importance. None of the medals established in Great Britain have, so far as we know, been adjudged twice to the same person. This may perhaps be a very good arrangement; but we believe it is in direct opposition to the intentions of the founder, as well as to the express rules laid down for their adjudication. If a medal is founded for the best paper in the *Phil. Trans.* for each year, there can surely be no reason why one person may not receive that medal twice, thrice, or even four times; and if another medal is founded for the most important discovery in science made in any part of Europe during a period of either one or two years, it should be possible for the same person to receive it more than once.

If the Royal Society have laid it down as a rule, as we believe they have, that the same medal shall not be adjudged more than once to the same person, they must have done it on the false supposition, that the medal is merely a badge of honour, a duplicate of which no person would be ambitious of wearing. But these medals are not marks of honour: They are substantial rewards, or pecuniary prizes, given not only to honour the successful inquirer by their public adjudication, but to indemnify him as much as possible for the expences incurred by his scientific researches. In proof of this we have only to state the fact, that almost all the large medals adjudged by the Royal Society have been converted either into silver plate or money.

When science is pursued by men of fortune, the loss of time, and the expence of apparatus and materials does not enter into their calculation; and if it did so, it would perhaps not form a large item in their annual account, as it is not common for such persons to devote much of their time to the arduous labours of original research. When a philosopher, however, carries on his inquiries by the sacrifice of a half or even a third of his whole professional income, and when this loss is increased by the purchase of expensive apparatus; the acquisition of a pecuniary reward cannot be unwelcome, independent of the honour with which it is accompanied. Upon this principle, prizes should always be adjudged to the person who really deserves them, however frequently he may have been the successful competitor.

We would strongly recommend it, therefore, to the Royal Society to imitate the admirable example of the Academy of Sciences of Paris, in placing all its medals upon a distinct and intelligible footing; and to publish an annual programme, stating the terms, and time of adjudication, and the various particulars which competitors might be desirous of knowing. Whether this is done or not, we would suggest to the editors of that excellent work, The British Almanack, to publish annually in the Companion to the Almanack, a list of the various prizes, whether scientific or literary, which are annually adjudged by the different institutions in the country.

Art. XIV.—*On the nature of the Rings formed by the double refraction of Quartz.* By G. B. Airy, Esq. F. R. S. Plumian Professor of Experimental Philosophy, Cambridge.

At the meeting of the Philosophical Society of Cambridge held on the 21st of February 1831, Professor Airy read a paper on the double refraction of Quartz, of which the following is a correct abstract. We hope that Professor Airy has embarked seriously in this new investigation, and that his astronomical and professorial duties will not prevent him from devoting a portion of his time to a subject of such high importance.

"It is well known to those who have followed the recent discoveries respecting the properties of light, that the phenomena exhibited by quartz are very different from those in any other substance of similar crystalline character—as for instance calc spar. Thus when exposed to *plane polarized* light, a plate of *calc spar* exhibits a series of rings of which the colours commence from Newton's black at the centre; and these rings are intersected by a black cross: *quartz*, on the other hand, displays a series of rings, the central point of which exhibits a colour different according to the thickness of the plate: there is no cross, but at a distance from the centre rudiments of black brushes begin to appear. Again in the case of calc spar, on turning the analysing plate the rings change in colour, but are always circular, and of unchanged dimensions. On turning the analysing plate in the experiment with quartz, the rings become square figures, with a curious defect of symmetry, and dilate or contract continually. If we put together a plate of right-handed and a plate of left-handed quartz in the same apparatus, we obtain a most singular and beautiful appearance, consisting of four coloured spirals cutting a number of concentric circles.

"On exposing these substances respectively to light *circularly polarized*, the appearances are still more remarkable; calc spar exhibits rings dislocated at each quadrant, with a grey cross; while the colours in quartz are seen in the form of two spirals inwrapping each other, with no black or grey cross.

"Professor Airy, after describing these phenomena, the most striking of which are new, proceeded to state and develope the hypothesis which they have suggested to him; of which the main point is this: that the two rays in quartz are *elliptically polarized*, one to the right, the other to the left: the major axes of the ellipses being respectively in and perpendicular to the principal plane. Calculations founded on this supposition represent with a very close agreement, the very various and complex phenomena which have been noticed; and, what is more remarkable still, they not only coincide in the general facts, but lead also to deviations from symmetry such as are observed to exist in the figures.

"After the meeting, Professor Airy exhibited, 1st, a model to illustrate Fresnel's idea, that circularly-polarized light is formed from plane-polarized light (when the plane of polarization is inclined 45° to that of total internal reflexion) by retarding the undulations perpendicular to the plane of reflexion by one quarter of an undulation; and that double such a retardation shifts the plane of polarization 90°: which was also shown to be the fact with Fresnel's rhomb.

2d, A new polarizing machine: the advantages of which are;—that complete rings may be seen with a very small specimen: that by placing the specimen in another position, the macled structure may be very well seen: that circularly-polarized light may be used as well as plane: and that lamp-light may be used as well as daylight.

3d, An attempt to exhibit the coloured rings by the light of heated lime; which succeeded so far as to show the practicability of this application."

Art. XV.—ANALYSIS OF SCIENTIFIC BOOKS AND MEMOIRS.

I.—*A Rationale of the Laws of Cerebral Vision, comprising the laws of Single and Erect Vision, deduced upon the principles of Dioptrics.* By John Fearn, Esq. London. Pp. 176.

Many of our readers are no doubt acquainted with the pneumatological writings of Mr Fearn, and with the correspondence which they occasioned with the late Professor Dugald Stewart, and which has been published in the *Parriana*, or notices of Dr Parr. Our illustrious countryman did not view the speculations of Mr Fearn with a favourable eye, and to Mr Stewart's great influence over public opinion, Mr Fearn attributes the total indifference of his countrymen to his intellectual labours. He has therefore made a direct appeal to the philosophers of France, to whom he dedicates his present work; and if it should merit their unqualified censure, he says he "shall be content to have it supposed that his previous writings are of no better complexion." Mr Fearn then makes a second appeal to the Lord Chancellor Brougham, and in a subsequent part of the work, he calls upon the Editor of this Journal by name, to avow his assent to the "laws of cerebral vision."

The strictures on Mr Stewart's conduct, in giving his opinion of the pneumatological labours of Mr Fearn, have but little tendency to encourage others to undertake the same ungracious task; but as Mr Fearn admits the principles of dioptrics to be well understood, and perfectly established, and asserts that his speculations are in no case contradictory to them, we shall enjoy the advantage denied to Mr Stewart, of being able to demonstrate the truth of our opinions.

Mr Fearn's work contains the following subjects:

Sect. I. Initiatory reasoning upon Data and Method.

II. Of Single Vision from two ocular impressions.

III. Of Erect Vision with two Eyes, involving the crossing and re-forming of images behind both eyes.

Sub-section. Of the principle of the visual direction of objects.

IV. Of erect vision with a single eye, involving the crossing and re-forming of images behind both eyes.

V. Of Vision without external objects.

As the views of our author on all these subjects are original, and stand in direct opposition to the opinions of the most distinguished philosophers and metaphysicians, it would require a volume as long as his own to make our readers acquainted with them, and another volume of equal length to examine them in detail. Mr Fearn will therefore, we hope, be satisfied with an examination of his *fourth mode of vision*, which he characterizes as "a clear field of unoccupied ground, there not being the least evidence of its having ever been noticed, and far less discussed in any extant treatise on optics, that has fallen in my way. On the contrary, the total neglect or oversight of this mode; *or rather the avowed denial of it in the case of human vision;* is plainly implied in a variety of ways in the extant treatises on the subject. With regard to the reality of this mode, I do not anticipate the smallest possibility of an objection, when it comes to be fully described."

The fourth mode of vision is announced in the following formal proposition, which we print in exact imitation of the original.

PROP. 14.

"*When we see an external object one-half of it with one eye, and the other half with the other; it is certain from the laws of dioptrics, that an impression from* ONLY ONE-HALF *of this object is inverted in* ONE *eye, and an impression from the* OTHER HALF *of it is inverted in the* OTHER; *and the consequence of this is, that we ought to see,* NOT THE WHOLE OBJECT IN THE NATURAL *arrangement of its features; but this object in* TWO UNNATURAL HALVES, TURNED PREPOSTEROUSLY BACK TO BACK. *But any such preposterous phenomenon as this we* NEVER WITNESS; *and, therefore, we do* NOT SEE IMMEDIATELY FROM THE INVERTED IMPRESSIONS IN THE EYES; *but these inverted impressions are* RE-FORMED AND RECTIFIED TO A NATURAL ARRANGEMENT BY SOME CEREBRAL MECHANISM WITHIN THE CRANIUM."

In order to understand this, let us suppose that we are looking at the words COACH HORSES, which we may consider as representing a coach, and horses yoked to it. Let us then place the edge of a sheet of paper between H and H, the opposite edge touching the nose, so that when we close the *right* eye, we shall see only the COACH, and when we close the *left* eye, we shall see only the HORSES. Now, since an inverted picture of the COACH, and also of the HORSES, is formed in each eye, a person stationed behind the two eyes will see these inverted pictures thus, HƆAOƆ SƎSЯOH. The coach and horses are now no longer in their natural arrangement, as Mr Fearn expresses it; but in two unnatural halves, turned preposterously back to back; and as we never witness any such preposterous phenomenon, he concludes that we do not see immediately from the inverted impressions in the eyes. Hence he is led to presume the existence of "some cerebral mechanism by which the inverted impressions are reformed and rectified."

We shall now proceed to show how the two *unnatural halves* are made to form a *natural arrangement* without recourse to any such mechanism.

It is a law of vision deduced from observation, and universally and demonstrably true, that when a ray of light, issuing from any point of an external object, falls upon the retina, the point of the object from which the ray issues is seen in the direction of a line drawn perpendicular to the retina, from the point at which the ray falls upon it. Now, if from every point of every letter in the inverted words HƆVOƆ SƎSЯOH, as delineated in the retina, we draw lines perpendicular to the retina till they meet the paper before the eye, to which its two axes are directed, their terminations will actually depict the words COACH HORSES. Hence it follows, that the preposterous position of the inverted images is absolutely necessary to their being re-formed in virtue of the law of vision already mentioned.

The phenomena of vision, such as single vision with two eyes, and erect vision from inverted images, are as well explained as any physical phenomenon, and present no difficulties whatever to those who are willing to study the subject with diligence and patience. From the general tone and character of Mr Fearn's work, we cannot hope to convince him of the mistakes which he has committed. He considers, indeed, his views as beyond the reach of criticism, when he says, " I hope I may be allowed to affirm, that there is no fact in dioptrics, or in any department of optical science, that is more rigorously demonstrated than that of the recrossing and rectifying of images behind the single eye." If this be true, philosophers are fools, and philosophy folly, and Mr Fearn may hope to establish a new school on the ruins of that of Boyle and Newton.

Thus disappointed by the perusal of the first sections of Mr Fearn's book, we hoped to find something deserving of praise in his fifth and last section, "on Vision without external Objects," a subject very little studied, and one on which it would be difficult to make numerous experiments, without stumbling upon some useful or important fact.

The optical readers of this Journal will recollect that we have had occasion to discuss the analogous subject of the vision of impressions on the retina. These impressions, however, were made with strong light on the retina, whereas Mr Fearn has occupied himself principally with the luminous circles produced by pressure on the eye-ball, and he treats only of the direction in which they are seen. He regards it as a most extraordinary phenomenon, that the luminous image is always on the opposite side of the eye-ball to that where the pressure is applied; whereas this is the necessary consequence of the law of vision, that when any point of the retina is acted upon, either by light falling on its inner surface, or by a pressure either on its inner or on its outer surface, light is seen in the direction of a line perpendicular to the retina, at the point of action or pressure. Hence we explain all the phenomena which he has described, and many more which have escaped his notice. Had he acted upon his eye-ball with a greater pressure, an experiment not very safe, he would have found that the pressure was propagated across the eye-ball to the opposite point of the retina, and that, in consequence of two opposite points of the retina being acted upon simultaneously by pressure on one point, two diametrically opposite luminous images are produced. This fact is very important, as it proves that pressure on any part of the retina, either from within or without it, produces a luminous image, which is seen in the same direction as if the same point of the retina had been acted upon by direct light.

We regret very much that we are obliged to give so unfavourable an account of Mr Fearn's optical labours. If he will only leave the field of speculation, and, with some feelings of respect for the researches of his predecessors, will devote himself to the hard labour of experiment and observation, we have no doubt that he will do something which will gain him credit and reputation.

II.—*An Experimental Inquiry into the Number and Properties of the Primary Colours, and the source of Colour in the Prism.* By WALTER CRUM, Esq. Glasgow, 1830. Pp. 47.

IT is much to be regretted that so excellent a chemist as Mr Crum should have left his own science to speculate upon the subject of prismatic colours, and undertake the hazardous task of overturning the splendid discoveries of Sir Isaac Newton. After Mr Crum had, in 1822, discovered his principal fact, and drawn from it his most important conclusions, he learned that Dr Joseph Reade had, in 1816, published a volume entitled, "*Experimental Outlines for a New Theory of Colours*," in which this fact and these conclusions were distinctly contained; but though thus deprived of all originality, he is so convinced of the importance of the discovery, that he conceives he cannot better serve the cause of science than by rendering more obvious the truth of Dr J. Reade's theory.

The *principal fact*, the discovery of which is thus given to Dr Reade, has been well known for more than a hundred years, and has been observed, studied, and explained by every philosopher who has repeated the experiments of Newton. Mr Crum announces his deduction from it in the following proposition:

Blackness or Darkness consists of three colours, and these may be produced from it by the Prism.

To prove this, he places a slip of black cloth $\frac{3}{8}$ths of an inch broad and 4 or 5 inches long upon a sheet of white paper laid upon the ground. He then views it through a triangular prism held near the eye, having its axis parallel to the black object, and at the distance of four feet from it. In this case the black object entirely disappears, and instead of it three objects will be observed of the three simple colours *blue, red*, and *yellow*. This effect, which Mr Crum has represented in beautiful coloured drawings, undoubtedly takes place, and the only fact to be determined is, whence come the three colours. To suppose that they came from the black cloth, seems to us a most extraordinary perversion of intellect. If darkness could produce all the splendid colours of the external world, why did the Almighty create light?

Mr Crum has not told us whether or not the *brilliancy* of the colours increases with the *deepness* of the black. The fact is, that the brightness of the colours increases with the brightness of the sheet of white paper on which the *black* cloth is laid, so that if the white paper is rendered 10, 20, or 30 times more luminous, the *blue, red*, and *yellow* colours are made 10, 20, or 30 times more luminous. Now, if the colours were produced from the black cloth, how does it happen that their brightness depends on the white paper? The question cannot be answered, and the conclusion is inevitable that the colours proceed from the light which is emitted from the white paper on each side of the black cloth. The *red* and *yellow* are produced from the light on one side of the black cloth, and the blue from the light on the other side. In this case these three colours, in place of being primary are all compound, and their composition has been explained by Dr Young in his *Elements of Natural Philosophy*, vol. i. p. 439, and illustrated in fig. 422 of his 19th plate.

The whole of Mr Crum's speculations on colours are of the same description,—they form one mass of error; and will be eagerly laid hold of by certain foreign journalists to depreciate the character of British science. We did not expect that Scotland was to furnish such a weapon for the use of her enemies.

III.—*Sections and Views illustrative of Geological Phenomena.* By HENRY T. DE LA BECHE, F. R. S., F. G. S. Pp. 71. 4to.

THIS useful volume contains an interesting collection of geological facts addressed to the eye through the medium of *forty* lithographic plates, most of which are finely coloured. These sketches are collected from a great variety of works, some of which are expensive and beyond the reach of ordinary geologists; and the letter-press of the volume is occupied with succinct and judicious descriptions of the phenomena represented in the plates.

In pointing out the vast importance of facts in the present state of geological speculation, M. de la Beche makes the following admirable remarks, which every geological student ought to engrave upon his memory.

"It would be well if the geologist would, before he begins to generalize, place himself before a globe or map of the world, and honestly ask himself how much is really known of the structure of the surface of that world. The answer might be, that, if *the whole known with exactitude were placed on the desert of Great Sahara, the area representing that desert would not be covered.* Even of the countries which have been considered the best explored, Great Britain, France, and Germany, how much remains to be examined! Yet in the face of this confessedly limited information, we are told how the whole surface of the world has been formed. Why not content ourselves for the present with an honest deduction from the facts before us? The advance so made is no doubt slow, but it is certain, and the step gained is firm.

"The work of pioneers is certainly laborious, and little suits minds which desire to advance rapidly and grasp all at once; but as a large accumulation of facts must precede any just conclusions respecting the general laws which have governed the formation of the world, we of the present day must, I am afraid, be compelled to perform the office of geological pioneers, however laborious, and comparatively inglorious that office may be.

"One of the principal objects of the following work is to induce geologists to present us with sections more conformable to nature than is usually done. Sections and views are, or ought to be, miniature représentations of nature, and to them we look, perhaps, more than to memoirs, for a right understanding of an author's labours.

"Among the sections here presented, there are doubtless many that are only approximations to the truth, but, as approximations, they may be valuable, and add to our stock of knowledge."

Art. XVI.—SCIENTIFIC INTELLIGENCE.

I. NATURAL PHILOSOPHY.

ELECTRICITY.

1. *On the Laws of Electrical Accumulation.* By Mr Snow Harris, Plymouth.—In the first volume of the *Transactions* of the Plymouth Institution just published, Mr Harris has inserted an elaborate paper on the "Laws of Electrical Accumulation." The following is a recapitulation of the facts which he considers to be established by his experiments.

1. An electrical accumulation may be supposed to proceed by equal increments.

A coated surface charging in any degree short of saturation, receives equal quantities in equal times, all other things remaining the same.

The quantity passing from the outer coating is always proportional to the quantity added to the inner.

2. The quantity of matter accumulated may be estimated by the revolutions of the plate of the electrical machine, supposing it in a state of uniform excitation; or it may be measured by the explosions of a jar connected with the outer coatings.

It is as the surface multiplied by the interval which the accumulation can pass.

When the surface is constant it is as the interval.

When the interval is constant it is as the surface.

It is also as the surface multiplied by the square root of the free action.

When the surface is constant, it is therefore as the square root of the attractive force of free action.

3. The interval which the accumulation can pass is directly proportional to the quantity of matter, and inversely proportional to the surface.

It is as the quantity divided by the surface.

If the matter and surface be either increased or decreased in the same proportion, the interval remains the same.

If, as the matter be increased, the surface be decreased, the interval will be as the square of the quantity of matter.

4. The force of electrical attraction varies in the inverse ratio of the square of the distance between the points of contact of the opposed conductors, supposing the surfaces to be plain and parallel, or otherwise between two points which fall within the respective hemispheres at a distance equal to one-fifth of the radius, supposing the opposed surfaces to be spherical.

5. The free action is in a direct proportion to the square of the quantity of matter, and in an inverse proportion to the square of the surface.

It is directly as the effect of the explosion on a metallic wire, all other things remaining the same.

If the matter and the surface increase or decrease together, and in the same proportion, the attractive force of free action remains the same.

If, as the matter be increased, the surface be decreased, the attractive force of free action is as the fourth power of the quantity of matter.

6. The effect of an electrical explosion on a metallic wire depends exclusively on the quantity of matter, and is not influenced by the intensity or free action.

It is diminished by accumulating the matter on a divided surface.

It is as the square of the quantity of the matter.

It is as the square of the interval which the accumulation can pass.

It is directly as the attractive force of the free action, all other things remaining in each case the same.

It is as the *momentum* with which the explosion pervades the metal.

II. CHEMISTRY.

2. *Existence of Copper in Vegetables and Blood.*—M. Sarzeau has confirmed the discovery of Meissner, that copper exists in vegetables, and he has obtained the following results:

	Milligrammes of Copper.
1 Kilogramme of grey quinquine contains,	5
Madder, -	5
Coffee, green Martinique,	8
Coffee, Bourbon, -	8
Common, -	8
Wheat, -	4.7
Farina, -	0.7
Fæcula of potatoes, -	0.0
Blood, -	1.1

M. Sarzeau has found that 1 milligramme of copper may be detected by the cyano-ferruret of potassium in 1 kilogramme of water.—*Journ. Pharm.* xvi. 505.

3. *On the Inflammation of Phosphorus in a partial Vacuum.* By A. D. Bache, M. D. Prof. of Nat. Phil. and Chem. Col. Depart. Univ. Pennsylvania.—In the last number of the *American Journal of Science and Arts*, (page 147,) I observed an extract from one of the foreign journals, in relation to the experiment of Van Bemmelen, with phosphorus in the rarefied air of the receiver of an air pump. The article from which that extract is taken reached us in the *Bulletin des Sciences Physiques, &c.* about the same time with the first volume of the French translation of *Berzelius' Treatise on Chemistry*, of which the article in the *Bulletin* is a notice. Having referred to the account of Van Bemmelen's experiments, given by

Berzelius, it appears that the cause assigned by their author to explain his results, was objected to, and that an explanation was still wanting; in search of this I engaged in a series of experiments still in progress. For the present I would call your attention to a portion of the facts exhibited by these experiments, which seem to me interesting.

Van Bemmelen found that a stick of phosphorus powdered with resin or with sulphur, and placed on cotton under the receiver of an air-pump, or exhausting the receiver was inflamed; and that the same effect was produced by wrapping a stick of phosphorus in cotton; then placing it under the receiver and exhausting the latter.

Its inflammation occurs when phosphorus alone is placed under the receiver and the air within is rarefied. These experiments I have repeated many times. The inflammation produced by resin is remarkably different from that which takes place when the sulphur is used.

In addition to the substances just mentioned as producing the inflammation of phosphorus under the partially exhausted receiver of an air-pump, I find, that the same effect is produced by powdering with finely divided

Charcoal,		
Spongy platinum,	Hydrate of potassa,	Carbonate of lime,
Antimony,	Lime,	Nitrate of potassa,
Arsenic,	Magnesia,	Nitrate of lead,
	Hydrate of baryta,	Sil. fluate of lime (fluor spar,)
Per sulphuret of mercury,		
Sulphuret of antimony,	Silica,	Muriate of platinum and ammonia,
Per-oxide of mercury,	Chloride of sodium,	Boracic acid.
Per-oxide of lead,	Muriate of ammonia,	
Per-oxide of manganese,	Chloride of lime,	

The temperature being about 60° Fah. or above that point.

Proceeding to an extension of the experiments to air of the natural density at different temperatures, I found, that at about 60° F., *Carbon,* in the form of animal charcoal, or of lamp-black, *causes the inflammation of a stick of phosphorus powdered with it:* this takes place either in the open air or in a close receiver of a moderate size. The fusion of phosphorus is produced at about the same temperature by (among other substances,) finely divided platinum sponge, antimony, potassa, lime, silica, carbonate of lime, &c. These actions are, as was to be expected, aided by an elevation of temperature above 60° F.

These results, I am led to believe from a partial trial, will find useful application in eudiometry by meams of phosphorus.—*American Journal,* No. 38, p. 372.

III. NATURAL HISTORY.

MINERALOGY.

4. *On Xanthite and its crystalline form.* By Lt. W. W. Mather, Assistant Prof. of Chem. and Min. U. S. M. A.—Xanthite has been de-

scribed as a new mineral species by Dr Thomson, from its chemical and some of its physical characters.* I have now the pleasure to state, that it also differs in its crystallographical characters, from any mineral species hitherto described. Dr Thomson describes it as a mineral of "a light grayish yellow colour, consisting of a congeries of very small rounded grains, easily separable from each other, and not larger than small grains of sand. These grains are translucent, and some of them indeed transparent. The lustre of the transparent grains is splendent; that of the translucent grains shining. The lustre is inclining to resinous. The grains are rounded, but when examined with the microscope, they seem to consist of imperfect crystals. The texture before a powerful magnifier seems foliated; but the grains are so small, that it is not easy to make out its true texture with accuracy. Specific gravity, 3.201.

"Easily crushed to powder by the nail of the finger. It is therefore soft. It does not scratch calcareous spar. Infusible before the blowpipe *per se*. Nor did it fuse along with carbonate of soda."

Dr Thomson found the constituents to be

Silica, - - - -	32.708
Lime, - - - - -	36.308
Alumina, - - - -	12.280
Peroxide of iron, - - -	12.000
Protoxide of manganese, - - -	3.680
Water, - - - - -	0.600
	97.576

and he considers it as essentially composed of 2 atoms of silicate of lime, and 1 atom of silicate of alumina.

I have found the Xanthite at Amity, Orange County, New York, in laminated masses in the same rock in which it is disseminated in grains. These masses are very frangible, crumbling readily into grains, some of which can be cleaved into prisms of perhaps 1-20th of an inch in their lineal dimensions. The laminated masses when held to the light exhibit very plainly by reflection, the directions of the cleavage planes. It exhibits double refraction when a candle is viewed through a thin plate of it, by placing it over a fine hole pierced in a card. It can be fused in small particles on a fine slip of platinum foil by the common blowpipe. When in fusion it intumesces, and gives a greenish translucent bead, slightly attractable by the magnet. With borax it gives a glass yellow when hot, but colourless when cold.

The cleavages are parallel to the sides of a doubly oblique prism, which is probably its primary form, as no other system of cleavage planes could be obtained. The reflective goniometer gave for the angles

P on M - -	97° 30′
P on T - - -	94 00
M on T - -	107 30

* *Ann. of the Lyc. of Nat. Hist. of New York*, for April 1828.

The planes M and T were not sufficiently brilliant to give the angles exactly; but it is presumed that the variation is not very great.—*American Journal*, No. 38, p. 359.

ZOOLOGY.

5. *Notice regarding the Salamandra atra.* By Mr STARK.—The two specimens of this reptile presented to the Royal Society of Edinburgh by George Fairholme, Esq. "were found very high on the Alps in the canton of Berne. They are perfectly black, frequent dry grounds, and have a slow crawling motion. I have not (says Mr Fairholme in a note which accompanied the specimens) seen this kind in any museum, even at Berne; and it is not much known, probably from its only appearing a few weeks of the year. The Chamois hunters call it *Raggimulli*, and consider it venomous; but it appeared perfectly harmless when alive."

Mr Fairholme is not without reason in supposing that the present species is not much known; for it is not alluded to by Cuvier in the first edition of the *Règne Animal*, probably from that celebrated naturalist never having seen the animal. In the second edition of this work, however, the species is noticed at the close of the description of the *Salamandra terrestris*, on the authority of Laurenti, in these words: "There is found in the Alps a salamander similar to the common one, but entirely black, and without spots. *Sal. atra*, Laurenti, pl. 1. fig. 2."

The other French naturalists who have mentioned the *Salamandra atra*, do not appear to have been able, by the possession of specimens, to indentify them with the description and figure of Laurenti. Sonnini and Daudin, long before the publication of the *Règne Animal*, had described the Black Salamander of the Alps as a separate species; but their statements rested solely on the authority of the original describer. Daudin, in particular, thus speaks of the *Salamandra atra*: "Laurenti has described and figured this Salamander, which appears not to differ from the preceding species (the *Salamandra terrestris*) but in its colour, which is deep black, without any yellow spot, and in its being one-half smaller. This author informs us that the Austrians name it *Lattermandl*, and that it is found in holes or clefts in the mountains of Etscher, where the Salamander with yellow spots has never been observed. We ought, then, with Laurenti and Sonnini, to regard this Salamander as a particular species, and not a simple variety, as Gmelin, Lacepede, Latreille, Schneider, and other learned naturalists have believed."—Daud. *Hist. Rept.* viii. 225.

I have not been able to procure a sight of Laurenti's work containing the description and figure of the *Salamandra atra*; but there can be little doubt, that the specimens which Mr Fairholme presented to the Society, are those of the animal which Laurenti has described. The locality of this species, at a certain elevation, joined to the total want of the coloured spots, and its diminutive size, distinguish the *Salamandra atra* from the common *Sal. terrestris*. The doubts of the French naturalists seem to have arisen from not having seen this apparently rare species. J. S.

6. *Vision of the Mole.* By Geoffroy St-Hilaire.—Does the mole see? Aristotle, and all the Greek philosophers, thought it blind. Galen, on the other hand, maintained that the mole saw. He affirmed that it has all the known means of sight. The question has been resumed in modern times. Naturalists have found the eye of the animal. It is very small—not larger than a millet seed; its colour is an ebony black; it is hard to the touch; and can scarcely be depressed by squeezing it between the fingers. Besides the eyelid which covers it, it is protected by long hairs, which crossing each other, form a thick and strong bandage. Such an eye ought to be destined to see. But anatomists do not find the optic nerve. What use could an eye be of, deprived of a nerve, which in other animals transmits the visual sensations to the brain. This consideration naturally tends to restore the opinion of Aristotle and the Greeks, and to induce the belief that the mole does not see, and that its eye is only a rudimental point, without use.

Direct experiments, however, made at the request of G. St-Hilaire, show most incontestibly that the mole makes use of its eyes, since it turns to avoid obstacles placed in its way. But if the mole sees, how is this accomplished without an optic nerve. M. Serres was of opinion that the place of this nerve was supplied by a superior branch of the fifth pair, analogous to the ophthalmic branch of Willis.

According to Geoffroy St-Hilaire, this change of function in a nerve, which it is not naturally destined to perform, does not exist. The mole sees by aid of a particular nerve, being unable, on account of the too great extension of the olfactory apparatus, to follow the direction which it takes in other animals, towards the tubercula quadrigemina, takes another direction, and anastomoses, in the nearest point, (au plus pres,) with the nerve of the fifth pair.—*Ann. des Sciences d'Observation*, i. 144.

IV. GENERAL SCIENCE.

7. *Great Scientific Meeting to be held at York.*—Arrangements are now making for holding at York, in July next, a meeting of the cultivators of science from every part of the British Islands. The object of the association is similar to that of the German Society, so fully described in this number. The sittings will continue for a week. The Lord Mayor and the authorities at York have, as might have been expected, entered heartily into this plan, and the Philosophical Society of that city have kindly offered to charge themselves with any preliminary arrangements which may be necessary.

Scientific individuals who propose to attend or to become members of the association are requested to communicate their intention to John Robison, Esq. Secretary to the Royal Society of Edinburgh, who has undertaken to act as Secretary till the association be constituted. Such communications will of course be post paid.

8. *Observations on the influence of Cold on New-born Children.*—Dr Trevisan has been making researches in Italy, principally at Castle Franco, analogous to those of MM. Villermi, and Milne Edwards, in France. The

conclusions at which he arrives, are:—In Italy, of one hundred infants born in December, January, and February, sixty-six died in the first month, fifteen more in the course of the year, and nineteen survived; of one hundred born in spring, forty-eight survive the first year; of one hundred born in summer, eighty-three survive the first year; of one hundred born in autumn, fifty-eight survive the first twelve months. He attributes this mortality of the infants solely to the practice of exposing them to cold air a few days after their birth, for the purpose of having them baptized at the church. As well as MM. Milne Edwards and Villermi, Dr Trevisan calls the attention of the ecclesiastical authority to measures suited to put a stop to such disasters, without violating the precepts or practices of religion.—*Ann. des Sciences d'Observation*, i. 144.

9. *Ossification of the Vitreous Humor.*—M. Krekn has lately met with that rare case, the ossification of the vitreous humor of the eye. It occurred in a man seventy years old, who died of gastritis; the preparation is placed in the Strasburg Museum. The left eye was healthy, but the right presented the following appearance:—The globe was diminished in size, had lost its spheroidal figure, and presented the appearance of four wrinkles or furrows, corresponding with the insertion of the recti muscles. It was heavy and hard. When a horizontal section was made from behind forward, the sclerotic was found to be very thick, particularly at its posterior part, near the entrance of the optic nerve; the instrument was soon arrested by a hard body, filling the whole space of the eyeball behind the crystalline lens, and consequently occupying the place of the vitreous humor. Immediately within the sclerotic was the choroid membrane, distinct, and rather thicker than natural. The retina was unchanged; the solid body within was marked by the same depression which had been observed externally. It was of a pale white colour, and was internally of a cellular texture, like the cancelli of the long bones. The crystalline was indurated and of a yellowish white colour; the optic nerve was wasted.—*Idem.*

10. *Zoological Weather Glass.* (*Mag of Nat. Hist.* iv. 479.)—At Schwitzengen, in the post-house, we witnessed for the first time, what we have since seen frequently, an amusing application of zoological knowledge, for the purpose of prognosticating the weather. Two frogs, of the species *Rana arborea*, are kept in a glass jar about eighteen inches in height, and six inches in diameter, with the depth of three or four inches of water at the bottom, and a small ladder reaching to the top of the jar. On the approach of dry weather the frogs mount the ladder, but when wet weather is expected, they descend into the water. These animals are of a bright green, and in their wild state, here climb the trees in search of insects, and make a peculiar singing noise before rain. In the jar they get no other food than now and then a fly, one of which we were assured, would serve a frog for a week, though it will eat from six to twelve in a day, if it can get them. In catching the flies put alive into the jars, the frogs display great adroitness.—*Idem.*

ART. XVII.—REGISTER OF THE BAROMETER, THERMOMETER, AND RAIN-GAGE, *kept at Canaan Cottage.* By ALEX. ADIE, Esq. F. R. S. Edin.

THE Observations contained in the following Register were made at Canaan Cottage, the residence of Mr Adie, by means of very nice instruments, constructed by himself. Canaan Cottage is situated about $1\frac{1}{2}$ mile to the south of Edinburgh Castle, about 3 miles from the sea at Leith, and about $\frac{1}{4}$ of a mile N. of the west end of Blackford Hill. The ridge of Braid Hills is about 1 mile to the south, and the Pentland Hills about 4 miles to the west of south. The height of the instruments is 300 feet above high water-mark at Leith. The morning and evening observations were made about 10 A.M. and 10 P.M.

DECEMBER 1830.

D. of Week.	Day of Month.	Thermometer. Morn.	Thermometer. Even.	Thermometer. Mean.	Register Therm. Min.	Register Therm. Max.	Register Therm. Mean.	Barometer. Morn.	Barometer. Even.	Rain.
W.	1	40	38	39	37	42	39.5	29.87	29.78	.05
T.	2	38	37	37.5	32	39	35.5	29.70	29.55	
F.	3	35	38	36.5	36	40	38	29.52	29.55	
S.	4	38	38	58	35	40	37.5	29.56	29.47	
S.	5	40	38	39	38	40	39	29.40	29.23	.03
M.	6	40	42	41	38	43	40.5	29.10	29.15	.06
T.	7	41	40	40.5	40	43	41.5	29.30	29.28	.05
W.	8	42	42	42	39	43	41	29.22	29.03	.27
T.	9	40	39	39.5	40	42	41	29.00	28.96	.15
F.	10	38	36	37	31	41	36	28.92	28.92	
S.	11	37	35	36	34	40	37	28.88	28.95	.11
S.	12	35	30	32.5	31	35	33	29.55	29.86	
M.	13	30	38	34	25	38	31.5	30.03	29.85	.04
T.	14	45	45	45	29	46	37.5	30.07	30.07	.18
W.	15	45	46	45.5	39	48	43.5	30.06	30.10	.07
T.	16	45	45	45	43	48	45.5	30.18	29.86	.05
F.	17	46	34	40	40	48	44	30.11	30.13	
S.	18	33	32	32.5	27	38	32.5	30.15	29.98	
S.	19	40	42	41	29	47	38	29.55	29.20	
M.	20	40	36	38	35	45	40	29.48	29.62	
T.	21	45	43	44	32	48	40	29.35	29.17	.07
W.	22	38	34	36	35	43	39	28.91	29.08	
T.	23	27	19	23	26	30	28	29.35	29.32	
F.	24	22	20	21	17	27	22	29.20	29.07	
S.	25	25	22	23.5	15	30	22.5	29.10	29.18	
S.	26	22	28	25	15	30	22.5	29.06	28.82	
M.	27	28	28	28	23	31	27	28.82	28.88	
T.	28	30	31	30.5	26	32	29	28.95	29.10	
W.	29	38	32	35	23	39	31	29.15	29.30	.04
T.	30	34	33	33.5	28	36	32	29.30	28.93	.90
F.	51	33	35	34	32	36	34	28.83	29.17	
	Sum,	1130	1096	1113	970	1228	1099	911.65	910.50	2.35
	Mean.	36.45	35.35	35.9	31.29	39.61	35.45	29.408	29.403	

JANUARY 1831.

D. of Week.	D. of Mon.	Thermometer. Morn.	Thermometer. Even.	Thermometer. Mean.	Register Therm. Min.	Register Therm. Max.	Register Therm. Mean.	Barometer. Morn.	Barometer. Even.	Rain.
S.	1	36	42	39	32	42	37	29.20	29.25	.08
S.	2	45	44	44.5	38	46	42	29.32	29.31	
M.	3	46	39	42.5	41	47	44	29.48	29.65	
T.	4	40	39	39.5	33	46	39.5	29.78	29.76	
W.	5	40	35	37.5	38	40	39	29.85	29.98	
T.	6	35	26	30.5	29	36	32.5	30.20	30.31	
F.	7	35	39	37	24	39	31.5	30.30	30.18	
S.	8	38	40	39	35	42	38.5	30.23	30.11	
S.	9	45	42	43.5	37	45	41	29.96	29.85	
M.	10	39	37	38	36	40	38	29.94	30.00	
T.	11	35	29	32	33	35	34	29.95	29.94	.05
W.	12	30	29	29.5	26	35	30.5	29.88	29.84	
T.	13	29	33	31	26	33	29.5	29.95	29.96	
F.	14	32	32	32	31	33	32	29.96	29.82	
S.	15	34	32	33	31	35	33	29.70	29.69	
S.	16	28	32	30	27	32	29.5	29.59	29.48	
M.	17	36	38	37	32	38	35	29.37	29.58	
T.	18	42	39	40.5	37	43	40	29.34	29.34	
W.	19	42	40	41	39	44	41.5	29.42	29.39	.23
T.	20	42	38	40	40	42	41	29.35	29.21	
F.	21	37	38	37.5	36	38	37	29.15	29.18	
S.	22	37	36	36.5	36	38	37	29.28	29.55	.10
S.	23	37	32	34.5	35	39	37	29.41	29.62	.20
M.	24	29	26	27.5	25	36	30.5	29.40	29.59	
T.	25	30	28	29	20	33	26.5	29.73	29.87	
W.	26	27	29	28	20	33	26.5	30.05	29.83	
T.	27	37	30	33.5	26	40	33	29.20	29.40	
F.	28	30	30	30	25	35	30	29.55	29.73	
S.	29	30	29	29.5	26	35	30.5	29.68	29.59	
S.	30	28	29	28.5	25	34	29.5	29.63	29.45	
M.	31	28	25	26.5	26	32	29	29.25	29.07	
		1099	1057	1078	965	1186	1075.5	919.04	919.11	.66
		35.45	34.10	34.77	31.13	38.26	34.69	29.646	29.649	

FEBRUARY 1831.

D. of Week.	D. of Mon.	Thermometer. Morn.	Thermometer. Even.	Thermometer. Mean.	Register Therm. Min.	Register Therm. Max.	Register Therm. Mean.	Barometer. Morn.	Barometer. Even.	Rain.
T.	1	28	30	29	24	30	27	29.05	29.02	
W.	2	33	33	33	28	34	31	28.96	29.08	
T.	3	34	24	29	30	37	33.5	29.17	29.10	
F.	4	33	32	32.5	19	33	26	28.98	29.27	3.05
S.	5	33	28	30.5	29	35	32	29.51	29.65	
S.	6	28	29	28.5	23	36	29.5	29.63	29.26	
M.	7	41	42	41.5	26	43	34.5	28.85	29.15	.30
T.	8	48	49	48.5	41	49	45	29.23	29.25	
W.	9	51	51	51	40	54	47	29.30	29.50	
T.	10	53	51	52	48	55	51.5	29.58	29.53	
F.	11	50	43	46.5	46	53	49.5	29.73	29.88	.04
S.	12	45	40	42.5	40	49	44.5	29.86	29.86	
S.	13	47	45	46	39	47	43	29.78	29.72	
M.	14	46	44	45	44	48	46	29.70	29.78	
T.	15	45	43	44	34	48	41	29.67	29.35	
W.	16	47	40	43.5	40	47	43.5	29.43	29.48	
T.	17	40	37	38.5	35	44	39.5	29.57	29.69	
F.	18	44	41	42.5	35	47	41	29.71	29.62	.13
S.	19	44	39	41.5	38	49	43.5	29.71	29.71	
S.	20	36	34	35	35	47	41	29.78	29.90	
M.	21	34	40	37	24	40	32	29.80	29.72	
T.	22	44	30	37	34	47	40.5	30.08	30.13	.10
W.	23	39	41	40	26	44	35	30.05	29.92	
T.	24	42	36	39	39	47	43	29.72	29.36	
F.	25	34	35	34.5	31	38	34.5	29.20	28.90	.16
S.	26	35	35	35	30	39	34.5	28.92	28.93	.10
S.	27	38	33	35.5	33	40	36.5	28.72	28.91	
M	28	40	32	36	31	44	37.5	29.00	29.27	
		1132	1057	1094.5	942	1224	1083	824.67	825.04	3.88
		40.43	37.75	39.09	33.64	43.71	38.68	29.452	29.469	

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NEW SERIES.

NOTICES TO CORRESPONDENTS.

The Articles of Mr Potter, Mr Forbes, Mr J. Veitch, Mr Laidlaw, and Mr Matheson, and also several Notices of Books, have been Postponed till next Number.—Mr Marshall's Meteorological Observations, which came too late for insertion in this Number, will appear in the next.

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