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ART. I.—*Biographical Notice of the late Sir J. E. SMITH, President of the Linnæan Society, with an estimate of the character and influence of his botanical labours.* By the Rev. E. B. RAMSAY, A. B. F. R. S. E. F. S. A. Scot. * Communicated by the Author.

THE characters of the illustrious dead, their virtues, their diligence and attainments, are interesting and profitable subjects of contemplation. An opportunity is given us to take a view of the periods in which they lived,—to notice the progress or decline of sound philosophy,—and to form an estimate of the influence of their labours on the advancement of knowledge in general, and of that department of it particularly in which they excelled. From general views we often return with advantage to a study of detail. Such discussions have often a favourable effect in stimulating the zeal, and directing the energies of young students in those fields of inquiry where master minds have been successful; and at any rate those who cannot attain their excellence, may at least avoid their errors.

In this article we propose to give a short biographical notice of the late Sir J. E. Smith, President of the Linnæan Society,

* Read before the Royal Society of Edinburgh, January 5, 1829.

and to make a few remarks upon the character of his botanical labours, and their general influence in promoting the study of botany in this country.

To enlarge upon the *private* character and history of the late president is not our present object; I may remark, however, of one whom I am proud to have called my friend, that kindness and benevolence were his distinguishing characteristics. He seemed to imbibe a pure and amiable spirit from the lovely objects to which he had devoted his study. In cases of misfortune or affliction amongst his friends and relatives, he acted a part of the most unwearied kindness and benevolent sympathy. The same feelings extended to all capable of being their object. I have a very interesting letter from him on the subject of cruelty to animals, and the influence a clergyman might possess with his parishioners in alleviating animal sufferings. Those who shared his acquaintance will join me in bearing testimony to his kindness of heart, his benevolence of disposition, and urbanity of manners. To scientific men he ever evinced the greatest liberality in acknowledging their merit, in communicating knowledge, and in affording the use of his valuable library and herbarium.

Sir James was born in 1759, December 2; he studied in Edinburgh, where, in 1780, he gained the gold medal given to the best proficient in botany; in 1784 he became an author by translating the preface to the "*Museum Regis Adolphi Frederici*" of Linnæus; in 1786 he travelled on the Continent, and graduated at Leyden; on his return he published his tour. In 1788 the Linnæan Society was founded by Sir J. Banks, the late Bishop of Carlisle, and other botanists, partly I believe to remove some jealousy of members of the Royal Society, who thought too much attention was given to natural history. In 1810, when the society was incorporated by charter, the president, Dr Smith, received the honour of knighthood. From 1789 to 1793 he undertook some works with plates, but which were discontinued for want of encouragement. One great work, however, of this kind was completed, the *English Botany*, which gives coloured representations of all the plants of the country then known. It extended from 1790 to 1814, and contained above 2000 figures; the plates by the elder Sowerby, the descriptions by Sir James. From 1800 to 1804, appeared the *Flora Britannica*, in 3 vols., which comprised all the Phenogamous plants, the Filices and Musci. This work was translated word for word into German. The *Compendium Floræ Britannicæ* is a pocket abridgement of this, and is in the 5th edition. He was selected by the executors of the late Professor Sibthorpe of Oxford, to edit a very splendid work on the Flora of Greece, and he had to write the letter-press from very scanty materials. Three volumes folio have been published, and it is supposed that it will extend to ten. The professor left an estate to defray the expence, which, when the work is completed, is to afford a salary for professor of rural economy in Oxford. His *Introduction to Physiological and Systematic Botany* has passed through five English editions, besides several American ones. In 1812 he published his *Grammar of Botany*, which gives a sketch of the natural orders. During a large portion of his literary life he was engaged in contributing botanical articles to *Rees's Encyclopædia*, which in fact include a complete system, and are constantly referred to as of the highest authority. In the 2d volume of the *Supplement to the Encyclopædia Britannica*, he wrote an article which includes a review of the modern state of botany. He occasionally contributed very elaborate articles to the *Linnæan Transactions*. His last and greatest work is the *English Flora*, the 4th volume of which was published in London the day its author died, March 15, 1828, at Norwich; it includes the Phenogamous plants and ferns, and is not a translation of the *Flora Britannica*, but an entirely new work. When we consider all these works, and the very elaborate character of many of them, we cannot but admire his diligence and application, especially as he was of a very feeble constitution; and how many may say of him as the younger Pliny wrote to his friend of his uncle, the elder Pliny. "Erat incredibile studium, summâ vigilantiâ. Itaque soleo ridere, cum *me* quidam studiosum vocant; qui si comparer illi, sum desidiosissimus." * Sir James, as we have seen, evinced a very early

* Plin. Epist. 3. 5.

partiality for natural history, and especially for botany; this partiality was confirmed, and the decided bias given to his future studies, by the very singular circumstance of his becoming the possessor of the MSS., books, and Herbarium of Linnæus himself. The younger Linnæus, a botanist of considerable eminence, succeeded to them at his father's death. In 1783, as he died without issue, they became the property of his mother and sisters. Sir Joseph Banks was then known throughout Europe as the patron of science, and more especially as the enthusiastic cultivator of *botanical* science. The collection was accordingly offered to him for the large sum, as it was supposed, of 1000 guineas. He declined the purchase, but advised the parents of his young friend Smith to procure it for him. They hesitated, wishing him, I believe, to follow some profession to which they thought his botanical studies would be detrimental: however the offer was accepted through Professor Acrel, the friend of the family of Linnæus. In the meantime they had begun to suspect that they had been precipitate, as an offer of purchase was made from Russia. Dr Sibthorpe of Oxford was also anxious to procure it for that university. Professor Acrel, however, insisted that he to whom the offer had been made at first should have it. A small collection was taken from it on account of a debt, which made a deduction in the purchase-money of 100 guineas. It had still another danger to encounter before it reached this country. The king of Sweden, Gustavus III. had been absent on a visit to France, and on his return wishing to retain the collection for Upsal, he sent a messenger to the Sound, who arrived after the vessel had passed; and in October 1784 the collection arrived safe in twenty-six large boxes, having cost, freight and every expence included, L. 1029. Of this collection and its late possessor Decandolle thus speaks after he had visited England. "Dignissimus Floræ Britannicæ auctor cl. J. E. Smith, non tantum herbarii sui ditissimi me participem comiter fecit, et circa species Sipthorpianas dubitationes aliquot solvit, sed adhuc aditum libere concessit ad herbarium Linnæanum nunc suum, præ aliis in synonimiâ momentosum, *nec sine veneratione summâ adeundum*." * Sir James'

* Preface to the *Systema*.

whole collection and library have been purchased, I believe, by the Linnæan Society for L. 5000.

The first point of view under which we naturally look to the botanical character of the president, is as the disciple of Linnæus and the expounder of his system; and we do so as well from his enthusiastic admiration of the Swedish naturalist, as from possessing in his herbarium the very plants from which Linnæus made his descriptions, thereby being enabled to correct his errors and inadvertencies as well as to explain his principles.

There can be little doubt, I think, that what Newton was in mathematical science, Linnæus was in natural history, because both advanced their favourite studies in the same manner, viz. by laying down correct *principles* of examination,—principles which have been found applicable to the further and advanced state of science. I do not mean to put botany on a level with astronomy, or the study of natural history in competition with a study of mathematical science, nor have I any wish to place Linnæus in competition with Newton. For who shall place any bust in the temple of fame except far below the bust of Newton. But every man may be called great, who displays original powers of mind in the investigation of nature, and this was Linnæus's greatness. From his time, natural history assumed a new character, in classification, in the formation of genera and species, upon principles which time only affects by showing to be correct. In nomenclature, in definitions, in precision of language, and in accuracy of examination, he has shown himself to be the greatest naturalist, because on these principles the first. In botany these qualities were more particularly displayed, and the advantages of his principles of study were soon apparent. Botany, from being the most perplexed and repulsive of all studies, confined to the studious and laborious, became exalted in the scale of the exact sciences, inviting even to the careless, the indolent, and the fashionable. Viewing botany, as we do at the present moment, in all its correctness of language, its precision and arrangement, we are apt to forget the merits of those master spirits who cleared away the darkness and confusion which hung around the subject when they commenced their labours.

The mere Linnæan nomenclature is a gigantic effort, and of itself a wonderful instrument of order and perspicuity. Nothing can be so repulsive, so vague, and wearisome, as the nomenclature of the older botanists; and it excites our surprise, how they could ever have had patience to work with instruments so clumsy and ineffective. In chemistry, where there is not a tenth part of the individual objects to be specified that there are in botany, the advantages of nomenclature have been most remarkable in promoting facility of investigation and perspicuity of description. The deficiencies of the ancients in studying natural history are very striking, if we compare their attempts in this department with their glorious productions in poetry, eloquence, history, and morals. It is surprising what little progress they made in their investigations into nature, and it is the more remarkable, that they should not have made more progress in botany, if we consider their extreme partiality and almost reverence for flowers. * Unlike the artificial wreaths which now mingle with the locks of youth and beauty, or with which the wearers vainly think to supply the place of both, their chaplets were the living and breathing flowers of nature. The secret which explains the whole is their want of system. That has been the great engine of advancement in modern times; for, as we understand the term, the ancients had no system in their study of nature. The three great names amongst the ancients as professed naturalists are, Theophrastus, Dioscorides, and Pliny. But in none is there the smallest attempt at what we now understand by classification. Theophrastus describes about 500 species, Dioscorides about 700. But the contentions amongst commentators to ascertain the plants alluded to are endless and irreconcilable. Pliny's work is valuable, as collecting all that had been done by Greek authors before his time; but the descriptions are so vague, taken from such uncertain marks, and from comparison with other plants of which we know nothing, that, as a system of plants, it is perfectly useless. Thus botany went on till Lobel in 1570 adopted something like a system of classes. This was improved by the two Bauhines, who published their works, the *Pinax* and *Hist. Plant. Univ.* in 1623 and 1650. But the first really systema-

* "Ροδα και Θεοισι τερπνα." ANACREON.

tic form given to botany was by Ray, the great English botanist, the second edition of whose *Synopsis*, his great work, was published in 1677, and is strictly speaking a systematic work, having an arrangement into classes, genera, and species, though in this respect still very imperfect. Ray was unquestionably a great naturalist, and he who would depreciate his character detracts from the glory of his country, who may well be proud of him, both as a man and a naturalist. Amongst the fathers of natural history, he ranks only second to the illustrious Swede, and such was the universal opinion expressed at the dinner given lately in London, to celebrate the 200th anniversary of his birth, at which many of our most eminent naturalists bore testimony to his genius and his merit. There could not have been a more enthusiastic student of nature, a more acute observer, or one more learned in all that had been done before his time, and yet his classification is liable to radical objections. As formed from his own resources and observations it is wonderful, but quite inefficient for the accuracy necessary in botanical description, and quite unable to keep pace with the immense discoveries which were made immediately after his time. The classes are often formed upon such vague distinctions as Trees and Shrubs; the genera formed upon such characters as shape of leaf, colour, taste, smell, and even size. The nomenclature is of itself sufficient to prevent the study assuming that accurate and attractive form which so eminently distinguishes modern botany. The expression of a plant by two words, its generic and specific appellation, though not perhaps to be considered as an actual discovery of Linnæus, still was first adopted by him as a canon of botanical science, and without which botany must have been prevented from assuming any rank amongst the exact sciences. It seems incredible to a young botanist, accustomed to the concise precision of this nomenclature, to learn that a pupil of Ray, when he mentioned a plant, was obliged to repeat many words, and often a line and half of Latin description. Thus the *Lolium perenne* of Linnæus is named in Ray: "Gramen loliaceum, angustiore folio et spicâ;" in which there is no *generic* distinction, as *Gramen* applies to *all* his grasses; and the Linnæan *Festuca elatior* in Ray stands thus: "Gramen arundinaceum aquaticum panicula avenacea;" and such is the usual number of words by which his plants are named. In many cases two words are used, but often the descriptions are much longer than those I have quoted at random. It is recorded, that Sir W. Watson, an eminent physician and botanist, and a pupil of Ray's school, had a memory so tenacious, that he could refer to any plant in Ray's works, by its lengthened appellation; and was looked on as such a prodigy, as to be termed the "living lexicon of plants;" but to those who were not blessed with such a memory, the reference to plants must have been most discouraging; and we can imagine the overwhelming astonishment with which the vulgar and genteel ignorant must have listened, when he was pouring forth these "*sesquipedalia verba*" to designate a grass, a weed, or a moss. Sir W. lived to see the introduction of the Linnæan nomenclature; and though he may have lost some distinction he enjoyed for his powers of memory, yet how much must he have admired the precision, simplicity, and elegance which that nomenclature introduced. In fact, without this, the science would have been soon lost in a chaos of words.

The Linnæan system was early, though not immediately, adopted in this country. When Linnæus visited England, he found Dillenius, the botanical professor of Oxford, too much involved in Ray's system, upon which he had constructed his own works, and Sir H. Sloane, the great patron of natural history, too old and too much prejudiced, to adopt the bold opinions of the young Swede. The adoption of the system in the public instructions of Cambridge and Edinburgh, in the former by Professor Martyn, and in the latter by the late eminent Dr J. Hope, is the æra of the establishment of the Linnæan system in Britain,—a system which, we may safely say with Dr Pulteney, "gave the author of it a literary dominion over the vegetable kingdom, which, in the rapidity of its extension, and the strength of its influence, had not, perhaps, been paralleled in the annals of science." Hudson, Lightfoot, and Withering, wrote Floras on this system; but there can be no doubt that Sir J. E. Smith was the most accomplished disciple of this school, and the best expounder of its principles. The *Flora Britannica* is perhaps the most perfect specimen existing,—a work of which it has been said, by no mean authority, that it is worth studying, as well from its logical precision, as for its botanical information.

One quality for which Sir James was *eminently* distinguished, was that of patient investigation; and to a naturalist what more important requisite can be mentioned? In the field of his immediate inquiry, he laboured with indefatigable application; and that these labours did not extend to every part of botany, is only to say that the faculties and powers of man are limited. That his studies were chiefly directed to Phenogamous botany, arose from the feeling, that his qualifications and advantages fitted him chiefly for this department. He knew the able and indefatigable labourers who were employing themselves particularly in the field of Cryptogamic botany, and, occupied as he was, neither his time or strength admitted of his extending researches into that wonderful and daily opening field. I would humbly beg leave to suggest the importance of this subject to our young students, and especially to those engaged in natural history. Extended as the bounds of human science now are, it is utterly impossible for any one to obtain deep and sound knowledge in *many* of its departments. General views of all may be obtained; but no one can expect eminence for profound knowledge, except he select one department on which to concentrate his attention and his application,—one to which he feels his own inclinations and his qualifications to be the best adapted. Then the student may look forward to the hope of being known as an original inquirer, and may look to the distinction of discovery; and many whose powers would have been well adapted to such condensed application of them, weaken and dissipate those powers by embracing too wide a field. I have heard Sir James say, that he never wrote a single description, every part of which he had not verified by his own observation, where it was possible to do so. A remarkable instance of this accuracy was evinced by his description of the difficult genus *Salix*. He had all the known species collected in Mr Crowe's garden at Norwich, where he studied them for nine years, under all their different appearances and stages of growth, before he ventured to print. But in nothing is this industry more conspicuous than in his synonyms from the older writers. The labour of this part of his work must have been, from the vagueness of their descriptions, and the character of their plates, extremely laborious; indeed, I know that sometimes in the class *Syngenesia* a whole day would be occupied in ascertaining the synonyms of a species. Such industry may be despised, or its value overlooked, by those who are anxious to grasp at results without patient investigation; or, as Dr Hooker has remarked, "many will avail themselves of his labours without acknowledgment." But it is an important department of science; because adopting descriptions of authors without an accurate knowledge of synonyms, has been perhaps the most fruitful source of error that could be named. De Candolle has borne testimony both to the importance and difficulty of this department of labour:—"Synonimia seu variorum cujusque plantæ nominum genuina correlatio, suscepti a me laboris pars *utilior* est forsan sed certe periculosior. Botanici omnes qui nullius addicti magistri in verba jurarunt, et proprio quasi marte laboraverunt, probe sciunt quam difficilis sit synominia ex meris descriptionibus petità." *

Excepting De Candolle himself, Sir James was perhaps the most *learned* botanist of his time.

Another point in the late president's character as a botanist is to be noticed; his attachment (as it has been frequently represented I mean) to the principles of his master. Of this attachment and admiration he has given uniform proofs, especially on one occasion, where he has said "For my own part, I profess to retain not only the *plan* but the very *words* of Linnæus, unless I find them erroneous, copying nothing without examining, but altering with a very sparing hand." From expressions such as these, and which, from his enthusiastic admiration of Linnæus, sometimes go beyond his real sentiments, he has been, I fear, rather unfairly represented as a bigotted follower of Linnæus; as one who was anxious to retard the advancement of science where it did not proceed in

* Preface to the *Systema*.

the exact path which he and his master trod. May I be allowed a few remarks upon this point, in reference to the scientific character of Sir J. and to the subject in its general application. In the science of which we are now more immediately speaking, as well as in every department of human knowledge, it is an evil of no small danger to form a bigotted attachment to the scheme of any man, or so blindly to adopt his opinion as to shut the eyes to any views, merely because they oppose the opinions of an individual, however able. All are liable to error, and no one ought to suppose that any department of human science has been advanced so far by any individual, as to admit of no further discoveries by subsequent inquirers. That Sir J. had not formed this bigotted attachment to the works of Linnæus is proved by the spirit of his own writings. The motto he prefixed to his *Flora Britannica* was a proof of his intentions. " Nullius addictus jurare in verba magistri." Take the arrangement of genera in his system of *British Botany*, especially of those which Linnæus had laboured the least successfully, those of the natural orders *Gynandriæ*, *Cruciferæ*, *Umbelliferæ*, and we shall find Sir J. departing from his guide, and adopting the results of his own observation and the suggestions of Richard, Brown, De Candolle and other eminent modern botanists. In particular he followed the masterly formation of genera in the *Cruciferæ* by Brown, and his own arrangement of the genera in the order *Umbelliferæ*, evinces very high original powers of botanical combination. He has made use of such additional lights as authors subsequent to Linnæus's time have thrown upon the subject—he has advanced, but he advanced with caution; and surely if it be proper to go along with alterations and improvements, it is no less the part of an accurate student to retain such principles as are established, to pause before he adopts changes which will supersede principles which he sees no reason to think are incorrect. If, in a science like botany, almost overwhelmed as it is with its own weight, it is allowable to make alterations, change names, and abolish long established characters, the whole science must soon fall back into its primitive chaos and confusion, and every succeeding botanist's labours tend only to increase the confusion and perplexity. Sir James considered, therefore, an adherence to those rules and principles which had been found efficacious in the study of natural objects, should be preserved, except when there was notorious and manifest advantages in departing from them, for terms and arrangements cannot be made to which *no possible* objection can be urged. Botany is to be esteemed amongst the less exact sciences, inasmuch as, that after the knowledge of the individuals as they exist separately has been attained, something is left to opinion for their arrangement in classes, and notwithstanding all the pains that have been taken with them, it must sometimes be difficult to say to which of the adjoining classes the individuals on the confines of each ought to belong; and hence I think we may adopt a rule upon this subject laid down by Malthus in regard to political economy,—a science which we may remark, by the way, exemplifies in a very great degree the evils of departing from the rules, and definitions, and use of terms and arrangements used by those who were the principal founders of it; and it is hardly to be questioned that such differences, introduced by authors subsequent to Adam Smith, have been the cause of much prejudice against the science, from the idea of its vagueness, its fluctuating and uncertain character. Mr M.'s rule is this, "That the alteration proposed should not only remove the immediate objections which have been made, but should be shown to be free from other equal, or greater objections; and, on the whole, be obviously more useful in facilitating the improvement of the science. A change, which is *itself* always an evil, can only be warranted by superior utility, taken in its most enlarged sense." *

A most important consideration arises from this, with regard to the President's supposed aversion to the new and more enlarged view of the vegetable world, called the Natural System, and his general preference to the Linnæan or artificial. The answer to this objection is contained in his works, because in his "*Grammar of Botany*" he has been at the greatest pains

* Malthus, *Def. Pol. Econ.* p. 6.

to point out the natural relations of plants to the British student. He has retained the Linnæan arrangement in his Flora, for this obvious reason, that the Flora of a country must always represent so small a portion of the whole vegetable world, that even to those who are acquainted with all its individuals, it offers an imperfect idea of the natural arrangement. How much more so then must it be so to a student, who is only partially acquainted with it. There is no point on which young botanists are more mistaken than in their ideas of natural classification: they often imagine they have only to commence the study of natural arrangement, and become at once profound philosophical botanists. This is one of the signs of the times; a desire to grasp at general results and conclusions, without a previous study in detail. The error in this case is, putting the natural and artificial methods in *opposition* to each other, whereas it appears to be the object of the artificial system to collect materials to form a natural one; but it has been of late spoken of rather as something quite superseded,—as something to give way to a new and a nobler structure, built upon a foundation entirely different. We cannot venture to prolong these remarks, already so far extended, further, than to remark upon the injustice of setting the Linnæan system in opposition to, or as hostile, to a natural arrangement. It has been said on high authority, (an article in the *Edinburgh Review*,) that though Linnæus was so great in advancing botany during its early stages, yet that his system has greatly contributed to retard its ultimate perfection. But, that this accusation is unjust, we may appeal to his own words in his "*Philosophia Botanica*," the work in which he professes to lay down the principles of the science. He says, "Methodi naturalis fragmenta studiose inquirenda sunt, primum et ultimum hoc in Botanicis desideratum est."* He then proposes his *Fragmenta*, consisting of 68 families. In another place he speaks thus upon the same subject: "Summorum botanicorum hodiernus labor, in his sudat et desudare decet. Methodus naturalis hinc ultimus finis botanices est et erit."*

* *Phil. Bot.* pp. 27—137. edit. Vienn. 1755.

But to attain that "ultimus finis," Linnæus considered the study of plants by an artificial method, in order to gain a knowledge of individuals, the best preparation. Every botanist will look to the natural system, as the point to which his labours tend, and a knowledge of which will be his ultimate reward. Such was the opinion of the late president, and such his object in the "*English Flora*;" and let it be remembered that he wrote for those who were commencing the study of botany in the Flora of their country, and to commence with the study of natural relations of plants before plants are known, is obviously absurd, or, as he judiciously remarks, "the knowledge of natural classification, being the summit of botanical science, cannot be the first step towards the acquirement of that science," and to arrange the Flora of any country, except by the artificial method, is to show the "membra disjecta," rather than the symmetry of a perfect and complete body. Surely it is the most philosophical mode of study to ascend to system from a knowledge of facts, and in a close observation of nature in detail by an easy and artificial arrangement, is the best exercise for those who seek to discover her general analogies and extended principles of arrangement; and if the Linnæan botany be, (as I humbly apprehend it will be admitted by the candid and the studious to be) the best introduction to a study of the great combinations and universal analogies of nature in the formation of plants, we cannot hesitate to give Sir J. Smith's botanical writings a very high rank amongst those works which have contributed to extend an accurate knowledge of nature.

The last point to which I shall advert is the great influence which the late president's writings have had in promoting the study of botany in general, and diffusing through society the pleasure it is capable of giving. This is a very important feature in the character of the man of science, whose object, let it be remembered, is twofold: 1*st*, to advance the progress of science by extending its boundaries into new fields of inquiry, or by perfecting that which is already known; and 2*dly*, to disseminate a love of science generally, and encourage the study of it as a branch of a liberal education. In the first instance, he addresses men occupied like himself in deep research and profound investigations; but, in the second instance, he addresses those who know little or nothing of science, and who seek only such knowledge as may give them general notions, and as may be consistent with other avocations and other studies. The latter is an important part of the duty of a scientific man; and he who has a talent for it, should feel as much pleasure in alluring the ignorant to know something, as he feels in aiding the profound inquirer. Whatever may be the truth of the adage, "a little learning is a dangerous thing," in other cases, and we are inclined to doubt its correctness in all, every acquisition in the knowledge of nature is desirable. The contemplation of flowers has in every age afforded to mankind the purest pleasure. Their delicacy of form, their sweetness of fragrance, their brilliancy of colour, and the poetical associations connected with them, are never failing sources of interest and delight. But a degree of botanical knowledge greatly enhances this pleasure, and in the study of their mutual relations and varied construction, the student finds an agreeable and a never ceasing occupation. But I think that we may go farther than this, and say, that the study of botany is by no means unimportant in an intellectual point of view; for we cannot think that arrangements so beautiful, definitions so correct, discrimination so accurate, and distinctions so precise, should be studied without advantage to the mental culture; for of how much importance in every study are the habits which give precision to language, and distinctness to definition, and these advantages may be acquired, and these pleasures enjoyed, without a very profound course of botanical study, which indeed, can only be followed up by a few.

It is therefore of great importance that there should be works on these subjects, suited for general purposes, works which may be popular yet accurate, interesting yet scientific, which shall, in short, combine the essential qualities of sound botanical science, without the repulsive characters which frequently accompany scientific writings. It is in this view that Sir James has had so much influence in promoting the study of botany, and I am happy to add the testimony borne to this merit by his friend, the eminent professor of Botany in the university of Glasgow. "To his extensive botanical aquirements, he added the high attainments of an elegant scholar, and a talent of composition which has rendered his writings universally popular, and has been the means of throwing a charm over his botanical writings scarcely known to the science before." *

If much of the secret of human happiness consist, as Paley observes, in the formation of habits of observation, a knowledge of botany largely contributes to that happiness; for in a solitary walk, in a journey, or in the absence of those with whom we can converse, objects are constantly occurring to interest and amuse. Thus botany has sources of enjoyment similar to those so well described by Cicero, when speaking of the happiness arising from the study of letters. "Hæc studia adolescentiam agunt, senectutem oblectant, secundas res ornant, adversis perfugium ac solatium præbent, delectant domi, *peregrinantur, rusticantur.*" It is a noble and delightful office of the man of science, to spread around him the happiness of knowledge, and to put into the power of others the gratifications which science can so liberally afford. But this is a power not granted to all who are in possession of knowledge; for the communication and the possession of wisdom are by no means always united, and surely the value of any man's knowledge is to be estimated very much according to the happiness he diffuses around him. The character of the late illustrious President of the Linnean Society, will thus live in connection with science and its pleasures, and his name be repeated with gratitude by thousands who will consider him as a benefactor,—for having spread before them means of interest and gratification,—for having given them habits of observation and attention to the natural objects around them, by which their sources of enjoyment were multiplied, and pleasures made to spring up at every step.

* *Edinburgh Journal of Science*, No. v. p. 161.

ART. II.—*Theory of the Action of Caloric in producing the Expansion of Fluids and Solids, with a Formula for the Modulus of Gravity.* By W. S. SANKEY, Esq. A. M., of the University of Dublin, and Extraordinary Member of the Royal Medical Society of Edinburgh. Communicated by the Author.

IN estimating the relative effects of caloric on the expansion of fluids, as manifested by the increase of their altitudes in thermometers, &c. it appears to me that some circumstances connected with the essential character of the fluid state, as distinguished from every other state of bodies, have been very generally overlooked. This increased altitude or rise of the fluid is, I think, usually referred almost exclusively to the expansion of the fluid in a direction perpendicular to its base, and that solely in consequence of the distance between each parallel stratum of the fluid being increased by the fresh introduction of caloric. In this view the relative heights of the fluid at different temperatures will be considered as proportional to the expansive power exerted by the caloric upon the fluid, and, therefore, as such will be taken as a fair estimate of that power. It is obvious, however, to any one who will give the subject a moment's reflection, and take into consideration the nature of fluids, that, although this may hold true as to solids, it is not equally the case in respect to bodies in a state of fluidity. For it is clear that the same power which the caloric exerts in increasing the distance between the minute particles in the *perpendicular* direction will operate no less forcibly in increasing the distance between the particles in a direction *parallel to the base.* Now, considering the fluid as consisting of a number of parallel strata, it is obvious that the effect of this increased distance between the minute particles in each stratum will be to drive off and eject, for want of room, one or more atoms from each stratum, or else to diminish the bulk of each atom. This latter does not appear very consonant to the idea which we form of an ultimate atom, nor to the effects of caloric itself, considered as a general expansive power. At all events, we would not expect this to take place in fluids, where the freest motion being allowed to the minute particles, it does not seem necessary to suppose such an alteration of their form and bulk. Supposing, then, the ultimate atoms to remain unaltered, it is manifest that the effect of the addition of caloric must be to drive a number of atoms upwards to the top of the fluid, there being no other way of escape for them from each stratum. These atoms consequently will arrange themselves on the surface of the fluid in one or more strata, according to the number of atoms that had been driven up. Hence it follows that the addition of caloric increases the altitude of the fluid in *two* ways; *first*, by increasing the distance between the strata into which the particles of the fluid were arranged prior to this addition of caloric; and, *secondly*, by increasing the number of strata with the corresponding interstices or distances between them. If now the form of the vessel containing the fluid be such that sections parallel to the base shall at all altitudes be equal and similar, it is obvious that the entire number of atoms contained in the additional strata will be equal to the number expelled from each of the former strata multiplied by the number of such strata. Calling, therefore, the number of atoms driven off from each stratum z, and the number of such strata s, and the entire number of atoms expelled r, then $r = z\,s$. Further, if the temperature of the fluid be equable throughout, it is obvious that the number of atoms in *each* of the *new* strata will equal the number of atoms that *were* in *each* of the *old* strata, *minus* the number of atoms *expelled* from *each*, or, calling the number that were in each of the former strata x, then $x - z =$ number in each of the additional strata. Consequently the number of additional strata, (being equal to the entire number of atoms driven up, divided by the number in each of these additional strata,) will equal $\frac{r}{x-z} = \frac{z\,s}{x-z}$, where $\frac{z\,s}{x-z}$ must be an integer, as of course the surface of the fluid will be level.

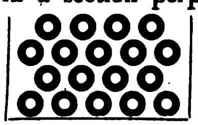
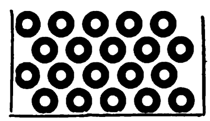
Calling now the altitude of the fluid for the original given temperature h, and the interval between the strata, supposing the particles arranged directly one above another, t; also the distance between the lowest stratum and the bottom of the vessel v, and the diameter of a minute particle p; then $h = s(p + t) - t + v$.

If now we designate the increased altitude h'; the increased interval between the strata t'; and that between the lowest strata and the bottom of the vessel v', also the number of strata s'; then in like manner, as above, we find that $h' = s'(p + t') - t' + v$, consequently $h' - h = s'(p + t') - t' + v' - s(p + t) + t - v$. But we have seen above that the additional number of strata, or $s' - s = \frac{zs}{x-z}$; substituting therefore for s' its value $\frac{xs}{x-z}$, $h' - h = \frac{s}{x-z}(xt' - xt + zt + zp) - t' + t + v' - v$, or, because $t' - t$ and $v' - v$ may be supposed very nearly equal, $h' - h = \frac{s}{x-z}(xt' - xt + zt + zp)$. But we have seen above that $h = s(p + t) - t + v$; therefore $s = \frac{h + t - v}{p + t}$, or, $t - v$ being indefinitely small in respect of h, $s = \frac{h}{p + t}$, therefore, substituting this value for s, $h' - h = \frac{hx}{x-z} \times \frac{t' - t}{p + t} + \frac{hz}{x-z}$. If now we consider $h' - h$ as indefinitely small, it will become the fluxion or differential of the altitude or equal to d h; also the number of atoms driven off from each stratum will become the differential of the number in each stratum, or $z = \mathrm{d}\,x$, and $t' - t$ will be the differential of the distance between the strata or equal to d t $\therefore$ $\mathrm{d}\,h = \frac{hx}{x - \mathrm{d}\,x} \times \frac{\mathrm{d}\,t}{p + t} + \frac{h\,\mathrm{d}\,x}{x - \mathrm{d}\,x}$, or d x being indefinitely small in respect of x, putting x for $x - \mathrm{d}\,x$, and dividing by h, $\frac{\mathrm{d}\,h}{h} = \frac{\mathrm{d}\,t}{(p+t)} + \frac{\mathrm{d}\,x}{x}$, therefore hyperbolic log. h = hyp. log. $p + t$ + hyp. log. x + cor.

But further we may observe, that, as the section of the vessel parallel to the base is constant, as also the size and form of the particles themselves, consequently, if we suppose these particles always to preserve their *relative* positions, which indeed appears necessary to the homogeneity of the fluid, the number of particles in each stratum must depend on the distance between the particles, or x must be a function of t.

To apply this to a particular case, let the vessel be tetrahedral, its base being a square; also let us suppose the minute particles of the fluid to be arranged directly one above another. It is obvious that the number of atoms in each stratum will be equal to the square of the number in each row of that stratum; so that, calling the number in each row y, $x = y^2$. Therefore, $y^2 s =$ the entire number of atoms in the fluid, which being a constant quantity, its differential $2 s y \, dy + y^2 ds = 0$; therefore $2 s \, dy = -y \, ds$. But we have seen above that $h' - h$ or $dh = s'(p+t') - s(p+t) = ds(p+t) + ds.dt + dts$. substituting therefore for ds its value $-\frac{2 s dy}{y}$ $dh = -\frac{2 dy s}{y}(p+t) - \frac{2 dy s}{y} dt + dts$. But $s = \frac{h}{p+t} \therefore dh = -\frac{2 dy h}{y} - \frac{2 dy h}{y(p+t)} dt + \frac{dth}{p+t}$. Therefore, $\frac{dh}{h} = -\frac{2dy}{y} - \frac{2dy.dt}{y(p+t)} + \frac{dt}{p+t}$. Now, calling any side of the square base of the vessel a, $a = y(p+t) - t + v$; or, $-t+v$ being indefinitely small, $a = y(p+t)$; hence a and p being constant, $dy(p+t) + dty = 0$. Therefore, substituting for dy its value $\frac{-dty}{p+t}$; $\frac{dh}{h} = \frac{3dt}{p+t} + \frac{2(dt)^2}{(p+t)^2}$ or, $\frac{2(dt)^2}{(p+t)^2}$ being indefinitely small, $\frac{dh}{h} = \frac{3dt}{p+t}$; therefore, hyp log. $h = 3$ hyp. log. $p + t +$ cor. When, however, $t = 0$, then $h = \frac{p^3 \times \text{entire number of atoms in the fluid}}{a^2}$, and $p + t = p$. Therefore, calling entire number of atoms n, cor. $=$ hyp. log. $\frac{p^3 n}{a^2} - 3$ hyp. log. p; therefore hyp. log. $h = 3$ hyp. log. $p + t +$ hyp. log. $\frac{p^3 n}{a^2} - 3$ hyp. log. p.

If now we suppose equal quantities of caloric to be added to the same fluid at different degrees of temperature, and to exert equal energies in expelling the same number of particles from each stratum, it is clear that the manifestation of these energies, as estimated by the increased height of the fluid, would be greater for the higher than for the lower temperature. We are not, however, authorized to assume that these equal additions of caloric will exert equal energies. For whilst, on the one hand, the elastic force of the caloric itself is greater at the higher temperature, on the other hand, its exercise is probably somewhat modified by the increased capacity of the fluid for caloric, in consequence of the absolute space having been increased by the previous expansion of the fluid.

We have hitherto supposed the particles of the fluid to be arranged one directly over the other. We are, however, by no means certain that such is the case. Let us now suppose them to be arranged triangularly, so that any four particles shall form an equilateral triangular pyramid. It is evident that they will be arranged in the containing vessel in alternate rows, presenting in a section perpendicular to the base either this appearance,  or this, . In both cases we perceive that the perpendicular distance between each stratum is less than the distance between the atoms. The distance between each stratum is, however, easily obtained, in terms of the distance between the atoms and their diameter. For let p be the diameter of the atom, t the distance between the atoms, then the perpendicular distance between each stratum $= \pm \left(\frac{3^{\frac{1}{2}}(p+t)}{2} - p\right)$, as is evident from the particles being arranged so as to form in the section equilateral triangles.

Hence $-t+v$ being indefinitely small, $h = s\left(\frac{3^{\frac{1}{2}}(p+t)}{2}\right)$

We may further observe, that, if the particles are arranged as in the first case, and the vessel of an hexahedral form, then the number of atoms expelled from the lowest stratum must be a multiple of six, and the number expelled from the stratum above it must be the same multiple of twelve, and so on through the alternate strata, as this is necessary in order to maintain the equidistance of the particles, and consequently the homogeneity of the fluid. Hence the number of atoms driven off from the lowest stratum being $m6$, the entire number driven off will $= \frac{s}{2} m18 = 9sm$. It is obvious, however, that this will be modified according as the number of strata is even or odd, or as the one or the other of the alternate strata is the lowest. From the preceding observations, it is evident that the arrangement of the minute particles must considerably influence the effects of caloric, as manifested in the rise of fluids. A knowledge of these arrangements of the atoms will probably be much facilitated by an attention to the forms of the crystals, and the mode of the transmission of light through the fluid. The figure of the atom has here been assumed to be a sphere, which will answer sufficiently for calculations made in regard to such minute bodies.

The size of these particles is also an interesting subject of research, the investigation of which might perhaps be aided by the comparison of different substances, in respect of the expansive power which caloric is found to exert upon them at the same and different temperatures. In conducting such inquiries, however, we should take into consideration that the specific gravities of bodies do not give us the specific weights of the ultimate atoms, but only of apparently equal bulks of matter. For instance, if we have two equal cubes of two substances, the weight of either is equal to the weight of its minute atom multiplied into the number of these atoms, consequently, calling the absolute weights of these equal bulks, S, Σ, the weights of the ultimate atoms W, w, the number of atoms in each row of each stratum N, n, then $S = W N^3$, and $\Sigma = w n^3$. Now, calling the diameter of the ultimate atoms of the two bodies P, p, and m the modulus of gravity, then $W = m P^3$, and $w = m p^3$, therefore $S = m P^3 N^3$, and $\Sigma = m p^3 n^3$, $\because$

$$S^{\frac{1}{3}} = m^{\frac{1}{3}} P N, \text{ and } \Sigma^{\frac{1}{3}} = m^{\frac{1}{3}} p n, \text{ and } \frac{S^{\frac{1}{3}}}{m^{\frac{1}{3}}} = P N, \text{ also } \frac{\Sigma^{\frac{1}{3}}}{m^{\frac{1}{3}}} = p n.$$

Calling now any side of any square surface of these equal cubes a, and T, t, the distance between the particles, then $a = N (P + T) - T$; also $a = n (p + t) - t \therefore a - N P = (N - 1) T$, and $a - n p, = (n - 1) t \therefore$ substituting for $N P$ and np their values as above, $a - \frac{S^{\frac{1}{3}}}{m^{\frac{1}{3}}} = (N-1)\ T$, and

$$a - \frac{\Sigma^{\frac{1}{3}}}{m^{\frac{1}{3}}} = (n-1)\, t \;\therefore\; T = \frac{a - \frac{S^{\frac{1}{3}}}{m^{\frac{1}{3}}}}{N-1} \text{ and } t = \frac{a - \frac{\Sigma^{\frac{1}{3}}}{m^{\frac{1}{3}}}}{n-1}$$

Taking these bodies to be now both of the same temperature, we will suppose further quantities of caloric to be added to each of them, such, however, that their temperatures shall still continue equal. In this case it appears probable that the distance between the minute particles at these different temperatures will be proportional, as being proportionate to the expansive forces of the caloric, or, calling the distances for the higher temperature T', T'', then $t : t' :: T : T'$. Hence we see, that, if this view be correct, the manifest expansions of bodies of different specific gravities will not be equal. For calling the expanded sides of any square surface of these cubes Q, q, it is evident,

$$\text{if } t : t' :: T : T' \therefore \frac{a - \frac{\Sigma^{\frac{1}{3}}}{m^{\frac{1}{3}}}}{n-1} : \frac{q - \frac{\Sigma^{\frac{1}{3}}}{m^{\frac{1}{3}}}}{n-1} :: \frac{a - \frac{S^{\frac{1}{3}}}{m^{\frac{1}{3}}}}{N-1} : \frac{Q - \frac{S^{\frac{1}{3}}}{m^{\frac{1}{3}}}}{N-1} \therefore$$

$a - \frac{\Sigma^{\frac{1}{3}}}{m^{\frac{1}{3}}} : q - \frac{\Sigma^{\frac{1}{3}}}{m^{\frac{1}{3}}} :: a - \frac{S^{\frac{1}{3}}}{m^{\frac{1}{3}}} : Q - \frac{S^{\frac{1}{3}}}{m^{\frac{1}{3}}}$; and multiplying by $m^{\frac{1}{3}}$; $a\,m^{\frac{1}{3}} - \Sigma^{\frac{1}{3}} : q\,m^{\frac{1}{3}} - \Sigma^{\frac{1}{3}} :: a\,m^{\frac{1}{3}} - S^{\frac{1}{3}} : Q m^{\frac{1}{3}} - S^{\frac{1}{3}}$, where we clearly see that q and Q cannot be equal.

Further, if we multiply the extremes and means, we have $a\,Q\,m^{\frac{2}{3}} - a\,m^{\frac{1}{3}} S^{\frac{1}{3}} - Q\,m^{\frac{1}{3}}\, \Sigma^{\frac{1}{3}} + \Sigma^{\frac{1}{3}} S^{\frac{1}{3}} = a\,q m^{\frac{2}{3}} - a\,m^{\frac{1}{3}} \Sigma^{\frac{1}{3}} - q m^{\frac{1}{3}}\, S^{\frac{1}{3}} + \Sigma^{\frac{1}{3}} S^{\frac{1}{3}}$; therefore, subtracting from both sides $\Sigma^{\frac{1}{3}} S^{\frac{1}{3}}$, and dividing by $m^{\frac{1}{3}}$, we have $a\,Q m^{\frac{1}{3}} - a\,S^{\frac{1}{3}} - Q\,\Sigma^{\frac{1}{3}} = a\,q\,m^{\frac{1}{3}} - a\,\Sigma^{\frac{1}{3}} - q\,S^{\frac{1}{3}} \therefore a\,Q\,m^{\frac{1}{3}} - a\,q\,m^{\frac{1}{3}} = a\,S^{\frac{1}{3}} + Q\,\Sigma^{\frac{1}{3}} - a\Sigma^{\frac{1}{3}} - q\,S^{\frac{1}{3}} \therefore m^{\frac{1}{3}} = \frac{a\,S^{\frac{1}{3}} + Q\,\Sigma^{\frac{1}{3}} - a\,\Sigma^{\frac{1}{3}} - q\,S^{\frac{1}{3}}}{a\,Q - a\,q}$

$\therefore m = \frac{(a\,S^{\frac{1}{3}} + Q\,\Sigma^{\frac{1}{3}} - a\,\Sigma^{\frac{1}{3}} - q\,S^{\frac{1}{3}})^3}{a^3\,(Q-q)^3}$ which gives us the *modulus* m in terms of quantities known by observation, and from this modulus thus known we can again calculate the expansions for new temperatures for either body, that of the other being known.

Thus knowing the modulus, and obtaining by observation a, S, Σ, q, we can find Q for the same temperature that gives q in the other body. In this way we may verify the calculations by experiment.

We may also observe, that, if this view be correct, we shall arrive at the same value for m from the comparison of the cubes of any two solid bodies in respect to their specific gravity and expansion, since the *modulus* of gravity must be constant. Therefore, finding the law of expansion for given temperatures in any one solid, we can calculate the law for all other solids.

Further, we have seen above, that $a = N(P+T) - T$, and also that $a = n(p+t) - t \therefore N(P+T) - T = n(p+t) - t$; therefore, putting for N, n their values $\frac{S^{\frac{1}{3}}}{m^{\frac{1}{3}}P}, \frac{\Sigma^{\frac{1}{3}}}{m^{\frac{1}{3}}p}$; $\frac{S^{\frac{1}{3}}}{m^{\frac{1}{3}}} + \frac{S^{\frac{1}{3}}}{m^{\frac{1}{3}}P} T - T = \frac{\Sigma^{\frac{1}{3}}}{m^{\frac{1}{3}}} + \frac{\Sigma^{\frac{1}{3}}}{m^{\frac{1}{3}}p} t - t$, or multiplying by $m^{\frac{1}{3}}$, $S^{\frac{1}{3}} + \frac{S^{\frac{1}{3}}}{P} T - m^{\frac{1}{3}}T = \Sigma^{\frac{1}{3}} + \frac{\Sigma^{\frac{1}{3}}}{p} t - m^{\frac{1}{3}} t$. We have seen also that $t = \frac{a - \frac{\Sigma^{\frac{1}{3}}}{m^{\frac{1}{3}}}}{n-1}$, therefore, putting for n its value $\frac{\Sigma^{\frac{1}{3}}}{m^{\frac{1}{3}}p}$, $t = \frac{m^{\frac{1}{3}}p\,a - \Sigma^{\frac{1}{3}}p}{\Sigma^{\frac{1}{3}} - m^{\frac{1}{3}}p}$, which, taking t as general for the distance between the particles, and p as general for the diameter of the minute particles, &c. will give us a general formula for the *distance between the minute particles*, in terms of the side of the square of the cube, the weight of the given body, the modulus of gravity, and the diameter of the minute particles. Applying it, however, in the present instance to the comparison of two solids, we find that, as $t = \frac{m^{\frac{1}{3}}p\,a - \Sigma^{\frac{1}{3}}p}{\Sigma^{\frac{1}{3}} - m^{\frac{1}{3}}p}$, so $T = \frac{m^{\frac{1}{3}}P\,a - S^{\frac{1}{3}}P}{S^{\frac{1}{3}} - m^{\frac{1}{3}}P}$. Further, then, as the elastic force of caloric may be considered as equal in all solids at the same temperature, therefore it is probable that the attraction between the minute particles, which is the counter-agent to this elastic force, is also equal. Consequently this attraction, being greater as the weight of the atom is greater, and as the distance between the atoms is less, therefore in general $m p^3 t^\varphi$ is equal for any given temperature throughout all bodies. To apply this in the present instance $m P^3 T^\varphi = m p^3 t^\varphi$, and dividing by m; $P^3 T^\varphi = p^3 t^\varphi$ $\therefore$ $P^{\frac{3}{\varphi}} T = p^{\frac{3}{\varphi}} t$, and substituting for T and t their values as before, $\frac{P^{1+\frac{3}{\varphi}}(m^{\frac{1}{3}} a - S^{\frac{1}{3}})}{S^{\frac{1}{3}} - m^{\frac{1}{3}} P} = \frac{p^{1+\frac{3}{\varphi}}(m^{\frac{1}{3}} a - \Sigma^{\frac{1}{3}})}{\Sigma^{\frac{1}{3}} - m^{\frac{1}{3}} p}$. Now, supposing the attraction between the minute particles to follow the law of gravity, or $m P^3 T^2 = m p^3 t^2$, then $\varphi = 2$, therefore

$\frac{P^{\frac{5}{2}}(m^{\frac{1}{3}} a - S^{\frac{1}{3}})}{S^{\frac{1}{3}} - m^{\frac{1}{3}} P} = \frac{p^{\frac{5}{2}}(m^{\frac{1}{3}} a - \Sigma^{\frac{1}{3}})}{\Sigma^{\frac{1}{3}} - m^{\frac{1}{3}} p}$. Hence we arrive at a value of P in terms of p, a, S, Σ, and m, which four latter being known by observation and calculation, we thus obtain the relative proportion of P to p. It is obvious, however, that we can again compare in like manner two other equal cubic bulks of the same matter, so that calling now the equal side of their square surfaces q, the absolute weights of these equal bulks U, u, we thence obtain a new equation for the values of P and p, or

$$\frac{P^{\frac{5}{2}}(m^{\frac{1}{3}} q - U^{\frac{1}{3}})}{U^{\frac{1}{3}} - m^{\frac{1}{3}} P} = \frac{p^{\frac{5}{2}}(m^{\frac{1}{3}} q - u^{\frac{1}{3}})}{u^{\frac{1}{3}} - m^{\frac{1}{3}} p},$$

in which equation, substituting the value of P derived above in terms of p, a, S, Σ, and m, we find a value of p in terms of a, q, S, Σ, U, u, and m, all quantities already supposed to be known. It is manifest also, that, if we compare the solid to which p belongs with different solids, we should still find always the same value for p. The same result also ought to be obtained from comparing this solid with any one other solid at *different* temperatures. In this way, then, we can put the calculations to the test of experiment. With this view, therefore, I submit them, as presenting, it appears to me, a somewhat new mode of considering the expansions of solids and fluids by the force of caloric.

ART. III.—*Case of extraordinary Physical Developement in a Boy six years of age.* By THOMAS SMITH, Esq. Surgeon, Kingussie. With a Plate. Communicated by the Author.

INSTANCES of remarkable deviation from the ordinary course of nature in her more recondite operations are, if properly viewed, objects of legitimate curiosity. For while what is common passes by without notice, or, if noticed, presents the facts too much under the same unchanging point of view for profitable or productive observation, what is extraordinary may be supposed to be attended with such new or well marked combinations of circumstances, to be seen under such new bearings and connections, as, if duly determined, may not only give insight into the causes of the deviation, but also throw additional light on the general principle or *law* of nature which regulates that class of phenonema.

Amongst the hidden things of nature may certainly be ranked the causes that produce the physical developement of man. For, though much labour and attention have been bestowed on that subject, and many causes have been assigned, yet it cannot be concealed, that nothing like a certain or satisfactory knowledge of the true causes has hitherto been attained. In this state of the subject, the following extraordinary case of early organic developement may perhaps be found not unworthy of notice, particularly as a faithful statement of facts, and, as far as the writer can discover, of concomitant circumstances, is attempted to be given; and as the same channels of observation are still open to supply any deficiences, and correct any mistakes that may appear in the narrative.

J—— M———— the subject of the present case, was born at Kingussie, Inverness-shire, in the month of October 1822. He is a natural son, and, from circumstances unnecessary to be mentioned, fell entirely under the care of his grandmother when he was about nine months old. He was nursed with his mother's milk eight months and a-half only, and, during the whole of that time, was fed also with spoon-meat, viz. porridge and milk or small beer, twice a-day. At the time of his birth he was rather a puny child, and showed no signs whatever of extraordinary growth, till he was at the age of six months, when his grandmother first observed his sexual organs to be unusually large. This she remembers well, because, afraid of this being made the subject of remark by the gossips in her neighbourhood, she warned her daughter not to expose or undress the child before them. The first time the attention of the writer of this paper was attracted to this boy was in the summer of 1826, when he accidentally saw the child naked, and was very much struck with the appearance of the sexual organs, which were certainly more developed, though he was not then quite four years old, than those of most young men at fourteen or fifteen years of age. The pubès or rather the root of the penis at the pubes, was covered on the sides with long light coloured hair. No measurements were taken at that time.

At present, he is six years and two months old. His height 4 feet $2\frac{1}{10}$ inches. He weighs 74 pounds avoirdupois, with his clothes on. The length of his body is remarkable, being 20 inches from the collar bone to the pubes; the length of the head, neck, and lower extremities being consequently 30 inches, 11 of which are occupied by the head and neck: so that the length of his lower extremities are only 19 inches, which is less than that of his body by an inch, a proportion entirely infantile. Round the lower part of his neck, he measures $14\frac{1}{2}$ inches; round the head immediately above the ears and eyebrows $22\frac{1}{2}$ inches, the height of his forehead is 2 inches; the length of his face, including forehead, $6\frac{1}{2}$ inches. An extraordinary ridge runs up the middle of his forehead, in the line where the frontal bone is divided in the fœtus into two equal parts, and which, in ordinary cases, is marked by a slight depression. The temporal ridge of the frontal bone also presents a peculiarity, having a hollow, not only on the side next the temple as usual, but also on the frontal side. The perpendicular height of the head from the meatus externus of the ear to the top of the head is 5 inches. The developement of the fleshy parts of the thighs and legs, arms and fore-arms, particularly towards the upper part of each, give a singular appearance to this boy, and suggests to the writer of this the idea of the muscles having grown without a corresponding elongation of the bones. Hence the vasti externi, the deltoid, the biceps, and supinator muscles, appear like huge lumps towards the upper end of the bones. The penis and testes are as large as those of most men, if not larger. The pubis is covered with black curly hair. He has also short dark coloured mustachios, but no hair on his chin. A sort of down, of the same light brown colour of the hair of his head, appears in the place of whiskers. His eyes are uncommonly sunk, and appear dull, and somewhat inanimate.

It is impossible, in a verbal description, to convey a just notion of the appearance of this extraordinary boy. I therefore send a correct sketch, see Plate I. which an eminent artist did me the favour to make of him at my request. This sketch presents a striking likeness, and gives a faithful representation of the appearance and proportions of every part as seen in a front view.

To render my observations in respect to the organic developements as complete as possible I measured the *facial angle*, and found it to be 83°. It is obvious that this angle must be much affected by the state of the frontal sinuses. In this boy, the uncommon projection of the upper parts of the orbits of the eyes, as well as of the lower part of the ridge running up the middle of the forehead, suggest the idea of uncommon largeness of the whole frontal sinuses; and this suggestion will be still farther confirmed by the deep hollow tone of voice which this boy has, if, as is commonly thought, the enlargement of these sinuses is attended with that effect. If the quantity of brain in the upper and anterior part of the cavity of the cranium has any thing to do with the intellectual functions, as some appear to think, there is another angle which it may be of still more importance to measure than the facial angle of Camper. The angle I mean is that which is formed by the meeting of a line drawn along the base of the brain, with another line drawn along the forehead, parallel to the inner table of the scull. This may be called the *basi-frontal* angle, and is found to vary considerably in different persons. In persons of undoubted great capacity, this angle has been found as high as 110° or 114°, while in some of an opposite nature, it has been found as low as 90° to 99°. In J—— M———— the basi-frontal angle is 90°.

To complete this class of observations, we wish it were in our power to add a phrenological cast of this boy's head. But for this task we confess ourselves unfit, not having had the advantage to have our tact improved under the tuition of a master, and having been generally unfortunate in our phrenological observations on the heads of our acquaintances. However, with a phrenological head before us to compare with the head of the subject in question, we venture to say, that the developement of the cerebral organs as a whole is in pretty just proportion to that of the body; and that Nos. 2, 8, or 16, (we are uncertain which) 13, 18, 19, and 30, are the most prominent bumps in J—— M————'s head.

Having stated the principal organic developements of this extraordinary boy, we come now to what, in a philosophical point of view, is the most interesting part of the subject, namely to inquire, whether or how far, these are accompanied by corresponding functional developements. On this head, we have endeavoured to collect every possible information—by our own personal observations—by reference to the teacher under whose tuition he has been for upwards of three months—by interrogating his grandmother—and by application to the neighbours who have seen him almost daily, from the time he began to walk. The results of these inquiries are,

1st, He has enjoyed almost uninterrupted good health from the time of his birth up to the present day. He sleeps soundly about nine hours in the twenty-four in summer, and eleven or twelve hours in winter. His natural functions arequite regular.

2d, He began to walk at or before the age of nine months. His strength is extraordinary for his age, though not disproportionate to his muscularity. I saw him lately lift from the ground an anvil, weighing 146 pounds avoirdupois. A year ago, if not earlier, he could carry two stoups full of water for a considerable distance. He runs swiftly, though awkwardly. Though conscious, and even boastful of his strength, he shows no disposition to quarrel with or hurt children of the same age;

on the contrary, he rather shuns than seeks contention. But when provoked, he beats with ease boys twice his own age.

3d, His grandmother reports his temper to be exceedingly violent when he is opposed in his wishes; but says, that he is easily awed into submission by the rod. He has never exhibited any of that gaiety or playfulness of disposition that is common to children of his own time of life; nor does he join other children in their diversions, which may be partly owing to his own disinclination; partly to this, that he has never been looked upon as a fit associate by children of any age. From the circles of the younger he has been excluded, by reason of his disproportionate bulk and strength; and from that of the older, by his want of the necessary advances in intelligence, for, though his strength is immense, he certainly shows a decided want of skill to direct it.

4th, Till lately, he showed a great disposition to pilfering, and this without any apparent object, since he would frequently hide what he stole, and make no use of it afterwards. Sometimes he was tempted to steal by being bribed to it by other children. But this fault appears to have arisen from ignorance, as he has now, I am told, abandoned it entirely, since he has been made aware that stealing is a crime.

5th, I have been solemnly assured by his grandmother, and her report appears to be confirmed by all that I can learn from the neighbours, that he has never exhibited the slightest inclination towards the other sex.

6th, In regard to the progress of his intellectual faculties, he is, and ever has been, decidedly behind other children of the same age. He was two years old before he could speak the two easiest words in his mother tongue, (Gaelic) and he has not yet acquired almost one word of English, though that is the language commonly spoken by the children about him. From these circumstances, and from the dulness of his look and evident inactivity of mind, he was long considered to be what is called a *born idiot*. He was three years old before he acquired the common use of words. About a month or two ago, on trial, I found that he did not, after three months *anxious* attendance at school, know more than two or three letters of the alphabet. Since that time his progress has been more decided. He has now acquired all the letters.

7th, He is regular, if I may depend upon his grandmother's account, in his devotional exercises. He says his prayers night and morning,—is fond of going to church,—and proves that he is attentive there, by repeating such parts of the clergyman's discourse as a child might be expected to notice.

It only remains to mention, that this boy has ever been in a state of the most extreme poverty. He has been indebted to the inhabitants of the village, for every morsel of bread he has eaten, and for the rags that barely suffice to cover his nakedness. He has never, as far as I know, worn shoes or stockings, and is seen in winter, as well as summer, going bare-footed and bare-legged, without appearing to suffer from the inclemency of the weather. *

This case, though extraordinary, is not altogether singular. Many like it are on record. The celebrated Baron Haller, in his great physiological work, cites from different authors upwards of twenty such cases, some of which are even more extraordinary than the present. (*Elementa Physiologiæ*, vol. x. l. 30, s. 1. § 15.) Not having an opportunity of consulting the works he refers to, I am unable to borrow any assistance from these cases, in the inductive reasoning which the circumstances attending this case might suggest; as Haller merely states the fact of the extraordinary growth, without in general mentioning any concomitant circumstances. But, as he observes immediately after, that a sufficiently full history of such premature growth is not to be had, it may be inferred that the circumstances in those he cites have not been fully given by their authors. One important fact, however, he does mention on the authority of Pliny and others, viz. that the *mind*, in these cases, usually remains in the infantile state. This, it is true, is somewhat indefinite; and, applied to J—— M————'s case, would very ill express the state of his intellectual functions from the time of his birth till very lately. I say till very lately, for within the last two months his mind appears to

* Any charitable contribution for promoting the comfort, or advancing the education of this remarkable boy, may be transmitted to Mr Smith, either directly, or through the Editor.—Ed.

have received a new impulse, which has evidently roused it to a state of greater activity. But before that time, that is, during the whole period of his extraordinary growth, his intellectual functions were evidently in a state of *extraordinary inactivity*, as unlike the usual state of them in children of the same age as their growth was unlike his.

The physical causes found in food, or climate, or lax constitution of the fibre, are totally inadequate to explain the extraordinary growth in this case. This is sufficiently proved by the facts mentioned in the history. Some other causes, therefore, must be sought for, and these, if I am not greatly mistaken, will be found in a *principle* which my observations lead me to suspect pervades the whole functional department of the human system; but which, as it appears not to have been attended to by others, it becomes me with due deference, and in the humblest manner, to submit to your consideration. To enter into a full consideration of this principle would occupy too much your time; allow me, therefore, to bespeak your patience, while I endeavour briefly to give an outline of facts, the consideration of which have forced this *principle* upon my notice.

The functions, including those of the mind as well as body, are numerous, but may without difficulty be reduced to three leading ones: the *constructive*, the *intellectual*, and the *reproductive*. 1*st*, The constructive functions are those by which the growth, which is nothing less than the successive formation and reparation of the body, is effected. Subservient to, and even part of these, are digestion, respiration, circulation, absorption and excretion, also perception and volition, in so far as these are necessary to the accomplishment of the general function. 2*d*, The intellectual functions have subservient to them perception and volition, and, as far as is necessary to their exercise, the constructive functions. 3*d*, The reproductive functions are well known, and have subservient to them not only the *constructive*, but also volition, perception, and even the intellectual functions each in their several places.

Now the *principle* to which I have alluded above, and which appears to me to be *universal* is this, that *any intention of one of these three functions is attended by a corresponding remission of one or both of the other two.* In other words, if any one of the functions is employed in excess, a corresponding deficiency will, I think, be found in the usual exercise of the others. A few examples of this principle will at present suffice.

To begin with the case of J—— M———— above related, the only circumstances in which he obviously differs from children of the same age, are his *precocity* of organic developement, attended with a decided *deficiency* of intellect, or activity of mind in the intellectual department. As the attentions that have of late been paid to him on all hands have evidently excited his ambition, and as he appears to have acquired a greater activity of mind in consequence, it will be highly interesting to observe the future reciprocal effects, if any, which his intellectual may have on the physical progress of further developement.

In all cases there is evidently *in utero* a very great activity of the constructive functions. This activity generally diminishes after birth in a degree, which, setting disease aside, bears an evident ratio to the increasing exercise of the intellectual functions. The *remission* or temporary suspension of the intellectual functions which occurs during sleep, is attended with an evident *intention* of the constructive functions, by which, in the time of healthy repose, the wearied or impaired organs are put into a state fit for renewed action. Great precocity of intellect, I have certainly seen attended with a marked decrease of the constructive functions. It is common for young persons of either sex to acquire about the time of puberty a sudden and extraordinary activity of the constructive functions, and I have long observed that the intellect then, except in matters that regard the *final cause of that activity*, becomes uncommonly sluggish and inactive. The reproductive functions succeed to the completion of the constructive, and it is well known that too great exercise of them is incompatible with an intense application of the mind to study. On the other hand, excessive intellectual exercise is sometimes destructive of health, (which depends upon a due performance of the constructive functions) and also of the reproductive powers or inclinations. Sir Isaac Newton, whose intellectual powers were never perhaps exceeded, is said to have exhibited this inactivity or deficiency of the reproductive.

These are a few of the facts from which I have ventured to deduce this *principle*. My attention having been drawn to it many years ago, I have taken every opportunity that has occurred since, to put it to the test of accurate and impartial observation, and can say with the greatest sincerity, that I have not met with a single fact which leads me to entertain any settled doubt of its *universality*. Sir Isaac Newton proposes as a *rule of reasoning*, that " in experimental philosophy we are to look upon propositions collected by general induction from phenomena, as accurately or very nearly true, notwithstanding any contrary hypotheses that may be imagined, till such time as other phenomena occur, by which they may either be made more accurate, or liable to exceptions." Under the protection of this rule, the principle just announced appears to me to stand. And, as it will be of the highest importance both to the moralist and physician, *if true*, I humbly hope that the obscurity of the individual who happens first to propose it to your notice, will be no obstacle in the way of your due examination of it.

ART. IV.—*Abstract of a Meteorological Journal kept at Funchal in the Island of Madeira, from January 1st to December 31st*, 1828. By C. HEINEKEN, M. D. Communicated by the Author.

JANUARY.

Pressure.		Corrected for Temp.	Temperature.	
Max.	30.48	66° = 30.394	Max.	70°
Min.	30.02	66 = 29.935	Min.	52
Mean	30.22	65 = 30.135	Mean	62.1

Diurnal range of thermometer, max. 18°; min. 7°; mean 12.5°

Rain, 4.08 in. No. 1.; *Dew Point*, max. 65 : min. 50; *Dryness*, max. 16, min. 1.

Winds, N. 2; N. E. 9; E. 1; S. E. 2; S. 2; S. W. 5; W. 10; = 31.

The thick hazy weather with heavy surf from the south, and the continuance of south winds *without rain*, were very unusual concomitants.

February.

Pressure.		Cor. for Temp.	Temperature.	
Max.	30. 43	64° = 30.344	Max.	69
Min.	29. 77	65 = 29.685	Min.	51
Mean.	30.172	64 = 30.087	Mean.	60.4

Diurnal range of thermometer, max. 16 ; min. 10 ; mean. 13.

Rain, 1.64 in. No. 1 ; *Dew Point*, max. 56 ; min. 50 ; *Dryness*, max. 14 ; min. 9.

Winds, N. 1 ; N. E. 17 ; E. 2 ; W. 5 ; N. W. 4 ; = 29.

A fine seasonable month.

March.

Pressure.		Cor. for Temp.	Temperature.	
Max.	30. 28	64 = 30.194	Max.	74
Min.	29. 85	64 = 29.765	Min.	53
Mean	30.092	65 = 30.007	Mean	61.8

Diurnal range of thermometer, max. 16 ; min. 8 ; mean 12.

Rain, 1.68 in. mean ; *Dew Point*, max. 65 ; min. 48 ; *Dryness*, max. 17 ; min. 1.

Winds, N. 9 ; N. E. 2 ; E. 7 ; S. E. 1 ; W. 8 ; N. W. 4 ; = 31.

A remarkably warm fine month.

April.

Pressure.		Cor. for Temp.	Temperature.	
Max.	30. 26	65 = 30.174	Max.	77
Min.	29. 34	65 = 29.257	Min.	52
Mean	30.056	66 = 29.971	Mean	61.9

Diurnal range of thermometer, max. 17 ; min. 8 ; mean 12.5.

Rain, 3.35 in. mean ; *Dew Point*, max. 63 ; min. 45 ; *Dryness*, max. 28 ; min. 1.

Winds, N. 1. ; N. E. 14 ; E. 5 ; S. E. 2 ; S. W. 1 ; W. 4 ; N. W. 3 ; = 30.

The hygrometer showed 4° of dryness during heavy rain, and with snow on the mountains on the 5th.

The sirocco continued much longer than usual, but it was irregular and imperfect. A perfect and regular sirocco I have never known to blow more than three days.

On the 3d, at 5 P. M., the barometer was at 29.26, the lowest I have observed during four years. The hygrometer was *unusually* high for rain. On the 4th the wind went round to N. W., and the barometer rose, but between the 4th and 5th (night) snow fell. On the 5th the barometer felt it, but rose a little during the day, although there were heavy showers.— *N. B.* Whenever the barometer rises during rain here, which it often does, the weather almost invariably soon mends and continues fair. A sudden fall, such as that of the 3d, seldom indicates so much or such continued bad weather as a gradually day by day lowering of the mercury. In rising it is the reverse. On the whole a cold month.

May.

Pressure.		Cor. for Temp.	Temperature.	
Max.	30.29	72 = 30.188	Max.	80
Min.	29.94	69 = 29.842	Min.	57
Mean	30.03	69 = 29.932	Mean	62.2

Diurnal range of thermometer, max. 16; min. 6; mean 11.

Rain, 2.14 in. mean; *Dew Point,* max. 69; min. 51; *Dryness,* max. 21; min. 1.

Winds, N. 5; N. E. 11; E. 3; S. W. 2; W. 6; N. W. 4 = 31.

The former part of the month remarkably warm; the latter more wet than usual. No observations made during the three last days of this month, and those noted of several subsequent months, in consequence of absence from home.

June.

Pressure.		Cor. for Temp.	Temperature.	
Max.	30. 27	72 = 30.167	Max.	76
Min.	30. 03	69 = 29.932	Min.	57
Mean	30.144	71 = 30.044	Mean	67.2

Diurnal range of thermometer, max. 15°; min. 4°; mean 9°

Rain, 0.21 in. mean; *Dew Point,* max. 70; min. 54; *Dryness,* max. 16; min. 3.

Winds, N. 2; N. E. 14 E. 3; W. 5; N. W. 6; = 30.

A cold backward month for the season. Observations omitted on two days.

JULY.

Pressure.		Cor. for Temp.	Temperature.	
Max.	30. 20	72 = 30.089	Max.	77
Min.	30. 02	74 = 29.909	Min.	64
Mean	30.091	73 = 29.980	Mean	70.8

Diurnal range of thermometer, max. 11 ; min. 7; mean 9.

Rain, 0.10 in. mean ; *Dew Point*, max. 72 ; min. 61 ; *Dryness*, max. 11 ; min. 2.

Winds, N. 3 ; N. E. 16 ; W. 12 ; = 31.

A cloudy damp month for the season, and more variable than usual. Observations omitted on eight days.

AUGUST.

Pressure.		Cor. for Temp.	Temperature.	
Max.	30. 17	73 = 30.059	Max.	80
Min.	30. 00	74 = 29.889	Min.	63
Mean.	30.107	74 = 29.996	Mean	71.3

Diurnal range of thermometer, max. 15 ; min. 10 ; mean 12.

Rain, none ; *Dew Point*, max. 73 ; min. 63 ; *Dryness*, max. 11 ; min. 1.

Winds, N. E. 31.

A fine summer month. The wind at N. E. or thereabouts, without shifting for many hours during the whole month. Slighter and less frequent siroccos during the summer than I ever remember. On the whole, it has hitherto been a cool season. Observations made on seventeen days only.

SEPTEMBER.

Pressure.		Cor. for Temp.	Temperature.	
Max.	30.15	75 = 30.039	Max.	81
Min.	29.93	75 = 29.819	Min.	65
Mean	30.04	75 = 29.929	Mean	71.8

Diurnal range of thermometer, max. 12 ; min. 9 ; mean 10.5.

Rain, 1.99 mean ; *Dew Point*, max. 75 ; min. 69 ; *Dryness*, max. 6 ; min. 0.

Winds, N. 1; N. E. 5; E. 2; S. E. 4; W. 18; = 30.

A damp cloudy wet month for September. Observations made on eighteen days only.

October.

Pressure.		Cor. for Temp.	Temperature.	
Max.	30.360	75 = 30.246	Max.	82
Min.	30.100	75 = 29.989	Min.	60
Mean	30.184	75 = 30.073	Mean,	70.3

Diurnal range of thermometer, max. 19; min. 10; mean 14.5.

Rain, none; *Dew Point*, max. 74; min. 56; *Dryness*, max. 18; min. 2.

Winds, N. 4; N. E. 12; E. 8; S. E. 3; W. 3; N. W. 1; = 31.

A very hot and unusually fine dry month, more like August than October. Barometer higher than it had been during the whole summer. So great a fall of the mercury with a N. E. wind as that on the 28th, and so little rain following, I never before observed. Observations omitted on two days.

November.

Pressure.		Cor. for Temp.	Temperature.	
Max.	30. 23	70 = 30.132	Max.	74
Min.	29. 80	68 = 29.715	Min.	55
Mean	30.046	70 = 29.948	Mean	64.8

Diurnal range of thermometer, max. 17; min. 8; mean 12.5.

Rain, 2.56 in. No. 1; *Dew Point*, max. 72.5; min. 54; *Dryness*, max. 15; min. 0.5.

Winds, N. 5; N. E. 9; W. 6; N. W. 10; = 30.

A very fine open month.

December.

Pressure.		Cor. for Temp.	Temperature.	
Max.	30. 44	67 = 30.354	Max	72
Min.	29. 98	68 = 29.882	Min.	52
Mean,	30.264	68 = 30.166	Mean,	62.7

Diurnal range of thermometer, max. 18; min. 9; mean, 13.5.

Rain, 0.52 in. No. 1 ; *Dew Point,* max 67 ; min. 50 ; *Dryness,* max. 15 ; min. 2.

Winds, N. 24 ; N. E. 1 ; W. 4 ; N. W. 2 ; = 31.

A remarkably fine warm dry mouth.

ANNUAL RESULTS.

Pressure.		Cor. for Temp.	Temperature.	
Max.	30.480	66 = 30.394	Max.	82
Min.	29.340	65 = 29.257	Min.	51
Mean,	30.120	69 = 30.022	Mean,	65.6

Diurnal range of thermometer, max. 19° ; min. 4° ; mean, 12°.

Rain, 17.67 in. ; *Dew Point,* max. 75 ; min. 45 ; *Dryness* max. 28 ; min. 0.

Winds, N. 57 ; N. E. 141 ; E. 31 ; S. E. 12 ; S. 2 ; S. W. 8 ; W. 81 ; N. W. 34 ; = 366.

Rain for Four Years : 1825, 20.43 in. ; 1826, 43.35 in. ; 1827, 18.17 in. ; 1828, 17.67 in. = 99.62. Mean, 24.90.

Pressure for Four Years : max. 30.620 ; min. 29.340 ; range, 1.280 in.

Temperature for Four Years : Max. 84 ; min. 50 ; range, 34°

Do. for Three Years means, viz. 1826, 64.3 ; 1827, 65.6 ; 1828, 65.6. Mean of the three, 65.2.

The instruments and their situations are the same as they were last year.

The *barometer* is observed *once only* (10 A. M.) during the four-and-twenty hours, in consequence of the very slight diurnal variation.

The *winds* are ascertained by looking to sea with a glass, (for all indicators on shore here deceive,) but they are not given as being strictly true.

The *mean* of the diurnal range of the thermometer is that derived from the maximum and minimum, which is thought near enough to the exact mean for such observations.

ART. V.—*Observations on the Mean Annual Temperature of Funchal in Madeira.* By C. HEINEKEN, M. D. Communicated by the Author.

THE following are all the *annual means* of the temperature of *Funchal* which I have been able to meet with.

Kirwan,	68.9
Cavendish, (as quoted by Humboldt in the *Personal Narrative*,)	68.9
Brewster's Formula,	68.7
Humboldt (*Treatise on Isothermal Lines*,)	68.5
Heberden, corrected by Schouw, (as quoted by Humboldt in Risso's *History of Nice*, &c. in 1826,)	67.3
Gourlay, (as quoted by Bowdich, in *Excursions in Madeira, &c.*)	66.2
Bowdich, (as *implied* in *Excursions in Madeira, &c.*)	66.
My own for 1824,	68.2
1825,	68.6
1826,	64.3
1827,	65.6
1828,	65.6

The *two first* of these, (Kirwan's and Cavendish's,) are, I suspect, (but I have not here the means of ascertaining it,) derived from the *same source*, and they have either quoted one another, or have been misquoted, as it regards names, by others. Humboldt, in the *Memoires d'Arceuil*, (Bowdich says) has found *Kirwan's* mean of the equator 3° too high,—it is therefore, I think, very probable, that his estimate for Funchal may be in the same predicament, Humboldt himself appears to *prefer Heberden's* mean, (which is 1.2 *lower*) to *his own;* for he gave it in 1826 to Risso, as "THE MEAN" of Funchal. *Gourlay's* and *Bowdich's* it would be much more convenient to me to prefer to all the rest, for, excepting my own, they are the *lowest* upon the list,—unfortunately, however, they are, I am afraid, the *lowest* also in *authority*, for the observations published by Gourlay were *not made in Funchal*, (as it is implied,) but at a height of from 200 to 300 feet above it. No one knows any thing of the instruments, their situations, or the hours of observation; and the observer, (Mr J. Murdoch,) was, I suspect, a mere noter of weather-glasses, and perfectly innocent of all scientific attainments. *Bowdich's* were continued but for a short period, and in a desultory manner. I cannot understand in what way the blanks (,,) in his monthly means are to be filled up; and he does not himself state in *direct terms* what he considers his own annual mean. It is therefore only left to me, (*after excluding* what would otherwise appear to be *my own evidence against myself*, (the 1824 and 1825 means) on the ground *that* I *knew* them to be *incorrect, and never published them as* TRUE *means*,) to suggest, that, as the mode *generally in use* for obtaining maxima observations is *at best* but an *approximation to the truth*, and as *mine*, although *I* may see reason sufficient for giving it the preference, at least wants confirmation, we should adopt a mean, derived from the three *unimpeached* previous deductions, and what *I consider as those only* of mine (three also) which *approximate the truth*; *instead of that which the Editor has suggested in the last Journal.*

It will then stand thus:—

Humboldt,		68.5
Brewster,		68.7
Heberden,		67.3
Mine for	1826,	64.3
	1827,	65.6
	1828,	65.6

giving 66.7 for the mean.

Or, if it did not look self-opiniated and presumptuous, I must own that I would go a little farther, and say 66.3; the result of Dr Heberden's,* (67.3), and my *3 years mean* (65.2); and I would do so, because they *were both derived from personal* and *continued* observations, which *none* of the others I believe, (Gourlay's and Bowdich's being placed *hors de combat* for the reasons given), were, and because I cannot attribute the cause of mine being *lower* than *all others* to the *situation* having been *artificially* cooled, knowing that whatever slight *artificial draught* it might have acquired was *at least* compensated by the *winds*, from which it was occasionally sheltered, and the *artificial heat* which it necessarily acquired, during the four days in the win-

* Dr Heberden was a resident in Funchal during several years.

ter, when the doors of the house were closed,—had it been the reverse,—had *my mean* been *higher* than that obtained by the *usual method*,—I should at once, and without a word in its support, have given it up. I believe that the means given for *all low latitudes* are *considerably too high*,—that, (as I have said before,) there is *only one* method of arriving at *the truth*, namely, by observations made on *several instruments*, and in *various situations*; and if it be not pushing the matter too far, I could almost think it more probable that maxima observations made in the mode in which mine were made would be proved *too high*, rather than *too low*, by this criterion,—assuming, be it remembered, that *positive* and *artificial shade* is implied by the terms, " shade maxima," and that a *true mean* temperature can be obtained by *such shade only*.

I shall endeavour throughout the current year to make the ten o'clock morning and evening observations, and those also regarding wells, which the Editor was kind enough to suggest in the last number of this *Journal;* and I am also daily expecting a couple of maximum thermometers from England, which I have directed to be made to correspond accurately, and which I intend to place—the one, in what appears to be the most perfect *external* shade, throughout the four-and-twenty hours which the situation admits,—and the other in the *same situation* in which my *internal* maximum instrument now hangs. I hope then, that, between the *three* modes, we shall succeed in obtaining a truer mean than has yet been given for the temperature of this place.

FUNCHAL, *Madeira*, 10*th February* 1829.

ART. VI.—*Account of the Sirocco Winds at Funchal, in the Island of Madeira, during the years* 1826, 1827, *and* 1828. By C. HEINEKEN, M. D. Communicated by the Author.

THE following observations were made during the prevalence of the Sirocco winds in 1826, 1827, and 1828.

Date.		Temp of Air.	Dew-Point.	Dryness.	Min. temp. of preceding day.	Remarks.	Wind.
1826, Feb.	28,	68	54	24	55	The true Sirocco.	S. E.
Mar.	1,	70	40	30	63		
Ap.	5,	67	49	18	55	Partial.	
	6,	69	47	22	60		
	15,	73	46	27	61	True	
	16,	73	46	27	64		
Aug.	6,	75	70	5	67	Thick and imperfect.	E.
	7,	76	71	5	69		
	8,	76	74	2	69		
	23,	77	74	3	68	Cloudy and partial.	S. E
	24,	76	75	1	69		
	30,	77	69	8	68		E.
1827, Jan.	16,	67	46	21	53	Partial Sirocco.	S. E.
Feb.	28,	64	62	2	54	Thick, imperfect.	E.
Mar.	1,	65	61	4	56	Hazy.	
	2,	66	63	3	55		
	12,	66	59	7	56	Slight.	S. E.
	22,	65	56	9	54	Imperfect.	E.
	23,	67	59	8	56		
	24,	68	56	12	53	Some light clouds, but still more true Sirocco.	S. E.
Ap.	7,	68	56	12	57	Imperfect Sirocco.	E.
July	11,	79	74	5	68	Thick, cloudy.	
Aug.	12,	81	66	15	66	Imperfect.	
	25,	82	56	26	66	True.	S. E.
	26,	84	48	36	68		
828, Mar.	11,	67	56	11	54	Thick, imperfect Sirocco.	E.
June	2,	73	70	3	64	Imperfect.	
*	3,	74	60	14	67		
Oct.	8,	75	66	9	64	Imperfect Sirocco.	E.
	9,	75	71	4	63		S. E.
	10,	76	66	10	63		

* In September there was a *true* Sirocco, but, being from home, no ob-rvations were made.

Date.		Temp. of Air.	Dew-Point.	Dryness.	Min. temp. of preceding day.	Remarks.	Wind.
1828, Ap.	28,	68	59	9	56	Slight Sirocco.	E.
	29,	69	53	16	57	More, but still partial.	S.E.
	30,	73	45	28	59	True but moderate.	
May	1,	71	54	17	60	Slight.	E.
Oct.	14,	77	61	16	65	Imperfect.	
	15,	77	72	5	66		
	16,	77	74	3	64		
	17,	77	74	3	63		

The *hygrometer* is Daniel's, and used at an open window facing the south.

The *minimum* temperature of the four-and-twenty hours immediately preceding the sirocco is the minimum obtained in the *general* observations for temperature.

The *terms* are of course (as I am not aware of any standard) arbitrary and relative. By a "*true Sirocco*," I mean a dry, hot, parching wind, coming in *puffs* from the south east, and with a *perfectly cloudless* sky, of a pale, warm, *peculiar* blue. By "*partial*," either a *true* sirocco mixed with, or diluted by, (if I may be allowed such expressions) the common atmosphere—or unmixed, but confined in extent, and in both instances retaining the peculiarities of the true sirocco, but in a milder form. By an "*imperfect*" sirocco I would imply simply a *hot wind*—generally blowing from the east—*not necessarily dry*—or in *puffs*, or accompanied by a *peculiar sky*. "*Thick*," "*hazy*," and "*cloudy*," as applied to our hot winds, I know not how to explain satisfactorily to a stranger to the climate. There are no atmospheric appearances in a northern climate to which, as far as I remember, I can compare them. I fear almost, that, like another *peculiarity* of *climate*, (the duties of a Cavalier servènte) I must, in the words of the poet, leave my readers to "suppose them." The first gives the idea of simple density in the air without any apparent cause,—the second shows something approaching to a mist as a cause,—and a cloudy sirocco is a grey overshadowing of the sky, rather than positive cloudiness. I can explain them in no better manner.

It will be seen by the foregoing tables, that the *true* sirocco does not visit us more than twice, or at most thrice annually,—that it never lasts *above* three days—that it *always* blows from the *south* of *east*—and that it is *always remarkably dry*. (In the Mediterranean, I am told, it is as remarkable for its *dampness*.) I have never experienced the sirocco wind excepting here. I have never read or heard a detailed account of it *elsewhere*, and I am quite ignorant of the *theory* of it, if there be one. I suppose, however, that it is amenable to the general law governing local and periodical winds, and that it arises from an effort to restore an equilibrium, which has been somewhere, and from some cause, disturbed. This disturbing cause I should imagine to exist at the *origin* of the peculiar wind to which it had given rise, viz. in some part of Africa, where a large body of highly heated air becoming *suddenly condensed*, an immediate rush of the surrounding denser medium had set a quantity of the rarefied atmosphere in motion, in that direction in which it had the least resistance to overcome; and that such current so produced constituted the *sirocco wind*, varying in temperature and dryness, according to the distance and medium through which it had passed, and ceasing altogether as soon as it had acquired the density of the surrounding atmosphere. That the point of its perfect condensation is in what I have termed our *true* siroccos, not very far to the north-west of the island, is probable from the frequency of thick, imperfect, and partial siroccos, compared with those which are complete; and I have little doubt, that, in accompanying the *true* siroccos in that direction, we should have a gradation of such changes, and terminating perhaps in rain. It seems to me that the reason why the *Mediterranean* siroccos are *damp* is because they have there arrived nearly at their utmost limit, and have met with a *sufficient decrease* of temperature to induce a deposition of *some* of the water which they held in solution—that in our *true* siroccos, on the contrary, a *much lower* temperature than they meet with here is required for that purpose—that in "*our partial*" ones but little lower would produce the effect—that "*our imperfect*" ones are perhaps no more than the usual atmosphere heated by a sirocco which has fallen short of the island,—and that such as are "*thick*, *hazy*, or *cloudy*," are a little further removed from their termination than those of the Mediterranean, but still so near it as to give out an aërial vapour (if the term be allowable) amenable to instruments, but not sufficient to affect goods, meat, paint, &c. (as I have heard to be the case in that part of the world) and still less to form palpable mists, fogs, or clouds. I am led to these conclusions from the evidence which the tables* afford. It will there be seen, that the *lowest* temperature of the preceding four-and-twenty hours has *invariably* been *much above* the *dew point* during the "*true*" siroccos—that it has *generally* been *above* it also in the "*partial and imperfect*"—and *always below* it in those which were "*thick*, *hazy*, or *cloudy*."

I fear that in thus venturing on a subject upon which I have had no means whatever, but those afforded by very local and confined observation, of informing myself, (for, as I said before, I have never met with either a book or a person conversant with it,) I may have sadly committed myself, and either repeated what has been better said before, or hazarded in theory what may have been disproved in fact. If such should be the case, I shall feel obliged to any one who will take the pains of setting me right, even at the expence of a *moderate* exposure; and would only say in palliation, that he who for eight long years of his life has been doomed,

> "To sigh forth his breath in foreign clouds,
> And eat the bitter bread of banishment,"

in such an *ultima thule* as this is with regard to literature and science, and with ill health for his gaoler—may be excused for *knowing* little more about its sirocco winds, than that they annoy him while they last, and may plead the "general issue"

* Had the "remarks" on each individual sirocco been made at the time with the intention of establishing or supporting a theory, they should have been more minute and particular. They were made along with, and taken from, the *general* observations, and, although less complete, are at all events *better evidence* on this account.

of valetudinarians, for *writing* about what he may chance not to understand so well as his neighbours.

FUNCHAL, MADEIRA, 10*th February* 1829.

ART. VII.—*On the Electricity of Elastic Fluids, and on one of the causes of the Electricity of the Atmosphere.* By M. POUILLET*.

SINCE the discovery of Franklin respecting the electricity of the atmosphere, there have been made in all civilized countries numerous observations upon the phenomena which are dependent upon this natural electricity. These observations have proved that stormy clouds are strongly electrified, the one positively, and the other negatively; that ordinary clouds have almost always one of the two electricities, but too weak a charge to produce the explosion of thunder;—in short, they have found that in a sky pure and cloudless, the air itself has a certain electric intensity, and that this intensity seems to increase in proportion as we rise in the higher regions.

During the storms of our climates, and particularly during the most violent storms of the tropics, the electricities of the atmosphere recompose themselves in great quantities, and destroy one another; for lightning, it is well known, is a recomposition of contrary electricities. It must, therefore, during the course of a year, reproduce as much electricity in the atmosphere as was destroyed by the storms and by the other electrical phenomena. Hypotheses without number have been made upon the origin and upon the formation of this prodigious quantity of electricity; but the hypothesis of Volta seems to be the only one which has any foundation. He supposes that bodies become electric in changing their condition, and that in consequence, the vapours of water which rise incessantly upon the continents and upon the seas, ought to be electrified by the single fact of its passage into the state of an elastic fluid. This opinion has been but seldom contradicted,

* Translated from the *Ann. de Chim.* tom. xxxv.p. 401.

and to combat it now we must bring very decisive and direct proofs.

In repeating the experiments by which the ablest observers have supposed it proved that changes in the condition of bodies are attended with a disengagement of electricity, it has happened to me, that the electrical signs have been so much weaker the more pains I took to avoid these foreign causes. For instance, when I took water perfectly pure to make it evaporate, either slowly or rapidly, upon a body which could not combine either with itself or with its elements, it was impossible to collect the least signs of electricity.

From numerous experiments made upon different bodies, I find that it is not the change of condition which disengages electricity, but always a chemical action more or less vigorous, which exerts itself between the elements of these bodies and the vessels which contain them; for by avoiding these chemical actions, every trace of electricity diappears.

As the origin of the electricity of the air, therefore, cannot be that which Volta has assigned, I have been induced, with some confidence, to suppose another origin which I have thought of for a long time past.

It appears to me that the phenomena of vegetation cannot be accomplished without a disengagement of electricity, and experience has, in fact, confirmed this idea in the most striking manner.

But before undertaking direct experiments upon vegetable actions, which must necessarily be very delicate and complicated, it is necessary to examine the electric properties of the gases at the moment of their combination.

This paper, therefore, will be divided into two parts. The first relative to the electricity of the gaseous combinations, and the second to the electricity which is developed in vegetation.

Part I.

On the Electricity of Gaseous Combinations.

The first discovery of the developement of electricity by chemical action was made in 1781. At that time Volta was in Paris. Already celebrated in Italy, he had travelled over all Europe to visit its philosophers. Among the remarkable inventions by which he was distinguished, that of the condenser was then the most recent, and excited the liveliest interest. This instrument could not fail to be well received and appreciated by the Academy of Sciences in Paris; and indeed two of the most illustrious members of that body, MM. Lavoisier and La Place, had hardly become acquainted with the instrument, when they consecrated its immense utility by a great discovery. They saw for the first time, in concert with Volta, that in chemical combinations electricity is developed, and that, by means of the condenser, it can be collected and rendered sensible. These experiments, which opened up a new career, have been since repeated with various success. Volta relates in his works, that he never failed to obtain electricity by the evaporation of water, and by the combustion of charcoal. De Saussure, on the contrary, who made such exact and curious experiments on the formation of vapour, never succeeded in obtaining electricity by combustion. Neither could Sir H. Davy discover any trace of electricity by the combustion of iron or of charcoal in pure oxygen or in air. More recently other natural philosophers have made new inquiries upon the electricity of flame, but their hypotheses have not led them to the truth*.

The fundamental result to which I have arrived explains very simply these contradictions and these errors. In repeating these experiments, I applied myself, in the first place, to the combustion of charcoal, and, in my first attempt, I saw with great surprise, that one could draw from it sometimes positive electricity, sometimes negative, and that at other times there was no means of obtaining the slightest signs of electricity. From these different and even opposite results, it appears at first that there is nothing to be deduced; but upon reflection, we see for certain that the combustion of charcoal gives electricity; for if it did not give any, it could not have been observed. Besides it is clear that it gives the two electricities, since sometimes the resinous and sometimes the vitreous has been obtained. Supposing, then, that one of the electricities is taken by the charcoal, and the other by the oxygen or by

* *Ann. de Chim.* tom. xxv. p. 378, xxvii. p. 5.

the carbonic acid, the surest method to obtain regular and constant effects will be, to isolate these electricities at the moment of their formation; and, in order to do that, we must separate as much as possible the burning body from the combustible body.

In arranging the experiment acccording to this plan, all the contradictions disappear; we can at pleasure collect the electricity of the charcoal or that of the carbonic acid, and thus the phenomena are seen perfectly similar and with great intensity. After many trials, I fixed upon the following arrangement: To obtain the electricity of the carbonic acid, it is sufficient to take a single piece of charcoal, of a sufficient diameter to give it the form of a cylinder whose bases are nearly plain, and to place it vertically six or eight centimetres below a plate of brass, which rests upon one of the disks of the condenser; then, if the charcoal communicates with the ground, and is lighted at its superior base without the fire reaching the lateral surface, there rises a column of carbonic acid which strikes the plate of brass, and in a few seconds the condenser is charged. The electricity which it receives from the carbonic acid is always positive. If, instead of holding the charcoal quite upright, we give it nearly a horizontal direction, so that the carbonic acid which is formed can only rise by ascending along the base of the charcoal, which is thus vertical, no sensible effect is obtained. In the same way, if, while holding it vertically, we light the lateral surface as well as the superior surface, an uncertain result is observed.

To obtain the electricity which the charcoal itself takes by combustion, we must place it by its inferior end directly upon the disk of the condenser; then, upon lighting its superior base, the fire is sustained by a gentle current of air, and in a few minutes the condenser is charged. The electricity which it receives from charcoal is always negative. If the charcoal touches the condenser only in some points, or if it burns on all its surface, no effect is obtained. Without doubt, in the first case, a small number of points of contact gives passage only to too small a quantity of electricity, and, in the second place, the carbonic acid being electrified positively at the instant when it is formed, and touching the lateral surface of the charcoal, which is negative, the two contrary electricities recombine.

To obtain more intense and rapid effects, we may take several cylinders of charcoal having the same height, and place them on their end, and very near each other, upon a sufficiently large plate of brass; then, after having set on fire all the superior bases, we have a large column of carbonic acid, which ascends, and is received against another plate of brass raised some inches, or even as much as a foot, and communicating with the condenser. With this arrangement the experiment is very quick, and in a few seconds we have a strong charge of vitreous electricity in the disk which communicates with the carbonic acid. On the contrary, when we would have the electricity of charcoal, we join the condenser to the brass plate, upon which all the burning cylinders are standing. It requires a few seconds of time before the condenser takes abundantly the resinous electricity. When the combustion is fed by a current of oxygen, the electricity disengages itself more rapidly, and takes a much stronger tension. A single instant is sufficient for the gold leaves of the condenser to reach the highest degree of divergency. But, in every case, whether it operates on little or on great surfaces of charcoal,—whether the combustion is left to itself,—whether it is increased by a current of air or a current of oxygen, more or less lively,—if we would obtain signs of electricity always certain and identical, the essential condition is to inflame only the horizontal surface of the charcoal in such a manner that the carbonic acid forms and ascends in a moment, and without having touched any body before arriving at the brass plate where it ought to deposit its electricity. This condition is so decisive, that if we direct, for example, a jet of oxygen against the side of a cylinder of charcoal, which is standing upon the condenser, and if we thus excite a very lively combustion, which even forms a deep cavity, it is impossible, in spite of the excessive rapidity of the combustion, to collect sensible quantities of electricity; or rather the signs which are obtained are sometimes positive and sometimes negative.

After this, it is sufficient to know that Lavoisier and La Place, Volta, and de Saussure, made these experiments in a metallic chaffing dish, in order to give an account of the oppositions and the uncertainties of their results.

After having removed these first difficulties of the experiment, I was enabled to enter upon the fundamental question which I had in view, viz. to know whether electricity is produced by change of condition or by chemical affinity. Volta had supposed, and it had been generally admitted, that charcoal, in passing from the solid to the gaseous condition, absorbs the vitreous electricity, and leaves to the remaining solid parts the resinous electricity which is discovered in it.

Other researches on the electricity of chemical combinations led me, on the contrary, to suppose, that if two elements which combine disengaged electricity, one of them would disengage the positive fluid, and the other the negative fluid, and reciprocally, that, when they separate, each of them would take up the fluid they had lost.

To resolve this question, and to arrive at the true origin of chemical electricity, we must form combinations which are not accompanied by changes of condition; and from among all those which presented themselves, I chose first that of oxygen and hydrogen, as being the easiest to produce in the required conditions.

The flame of the hydrogen gave contradictory results, like the combustion of charcoal. In the course of a few minutes it gives in succession positive and negative electricity, very intense and very weak indications; and often it was even impossible to obtain any effect. While endeavouring to discover the cause of these contrarieties, I thought of many without finding the real and the most essential one. I had observed, first, that every thing which surrounded me had an influence upon the results; for example, a window shut or open, a small fire in the laboratory, or even a lighted candle, a pile in activity, or a small machine, the plate of which had only been turned a quarter of a round, all these circumstances and others were sources of discordance among the results. Nevertheless, all these accidents depended on one cause so simple, that it did not detain me long.

It is known that gases are not very good conductors of electricity, and it can be proved by a curious experiment: Place a very small spirit of wine lamp upon a common electroscope, and at 5 or 6 feet above it, a stick of electrified rosin, or a plate of glass, or any other body very feebly charged, at the instant we behold a very great divergence in the plates; notwithstanding the same body with the same electric charge would give no sign of divergence if it was presented to the electroscope without flame, and even at the distance of an inch. This apparatus I have found very useful in discovering the smallest trace of electricity, and it has made me understand all the accidents of which I have spoken. When we turn the plate of an electrifying machine, the air of the room is electrified, and the flame which ascends in that air, is charged at the instant with electricity of the same name, and conveyed to the condenser. A pile in action electrifies the air like a machine, and the flame of the electroscope affords a proof of it; a fire of charcoal, or even a lighted candle, produces carbonic acid electrified positively; and the flame of the electroscope betrays again the presence of this electricity. In short, the atmospheric air is always electrified, and if it penetrates into a room by an open window, and is renewed, I am certain that it can preserve its electrified condition for a sufficiently long time to cause great disturbance in the inquiries which are making upon very weak quantities of electricity. But there are means of excluding all these causes of error, and it must be allowed that in all that follows they have had no sort of influence upon the results.

We now return to the combustion of hydrogen. The gas flows out by a tube of glass; the flame is vertical, presenting a breadth of 4 or 5 lines upon a length of about 3 inches; the electricity is conducted to the condenser no longer by a plate of brass, but by a platina wire, whose end is coiled into a spiral. The spire is always vertical; but sometimes the circumvolutions are of a diameter large enough to envelope the flame without touching it, and sometimes they are small enough for the whole spire to be completely enveloped in the interior of the flame.

When we approach the flame from the exterior outline of the spire, and keep it 10 millimetres distant, we obtain signs of vitreous electricity. These signs become more and more intense in proportion as the distance diminishes. But when the flame touches the spire, the electrical signs become weak and uncertain. It is the same when the flame passes to the interior of the spire, and in the direction of its own axis. Therefore, around the apparent flame of the hydrogen there is a sort of atmosphere more than 10 millemetres in thickness, which is always charged with vitreous electricity. Since vitreous electricity is developed in the phenomenon of combustion, it follows that there must be some part which has resinous electricity, which we shall now try to discover. As it does not appear in any points outside of the flame, we must try to penetrate into the interior, avoiding as much as possible the exterior outline, which always gives vitreous electricity. To do this, it is sufficient to take a spire of a small diameter, and to place it in the middle of the flame in such a manner, that it is enveloped on all sides; in this way, indeed, the condenser is charged again, but the flame now gives it resinous electricity. Thus both the inside and outside of the flame are in opposite electrical states; the outside is always vitreous, and the inside always resinous. It follows from this that there is a layer of the flame where the electricity is nothing, and, indeed, if we plunge the spire in such a manner that it penetrates nearly one-half into the bright part of the flame, all electrical indications disappear. Here then is a very striking analogy between the combustion of hydrogen and that of charcoal. Certainly, in all the thickness of this exterior atmosphere, where we find vitreous electricity, the combination is not made, for the hydrogen cannot arrive there. It is necessary, then, that this electricity which we observe is an electricity communicated, and from whence can it come, if not from the combustion itself, or rather of the oxygen which is predominant on the outside, and which envelopes, in some measure, all the jet of hydrogen?

It follows, then, that this oxygen which is combined, disengages vitreous electricity, which communicates itself to the neighbouring strata of air, raised to a sufficiently high temperature to perform the office of a conducting body; and, in like manner, in the interior of the flame, it is the hydrogen which is in the greatest proportion; and since we find the resinous electricity, it must be disengaged from the hydrogen which burns, and which it communicates to the excess of hydrogen which is not combined. If the phenomenon takes place in this manner, it is probable that at a certain distance above the flame the two contrary fluids ought no longer to appear, because they will have combined; and this in fact happens when we try to collect the electricity at a distance sufficiently great above the vertical flame; but at the distance of a few inches only, we obtain other effects,—the two electrical fluids appear in the same quantity, but they are not recomposed; for if we present a soldered plate of zinc and copper, the zinc part attracts the resinous, and the copper plate the vitreous electricity. If, instead of making the hydrogen flow out by a tube of glass, we make it flow out by a tube of metal, which does not communicate with the ground, but only with the condenser, we see that this metal tube, which touches the hydrogen without even touching the flame, takes always the resinous electricity; and if, on the contrary, it is made to communicate with the ground, it loses the resinous electricity which it had lately carried to the condenser, and the product of the combustion preserves an excess of vitreous electricity.

These experiments upon the combustion of hydrogen and of charcoal made it easy for me to examine other combustible substances, whether they were solids, liquids, or gaseous. It would be tedious, and perhaps useless, to relate the numerous experiments I made upon alcohol, ether, wax, the oils, the fatty substances, and many vegetable bodies. The flames of all these bodies presented exactly the same phenomenon as the flames of hydrogen.

I remarked only that the particles of charcoal which were floating in the flames of this substance, and which, according to the observations of Sir H. Davy, gives them their shining lustre, makes them also better fitted to show the resinous electricity. From the whole of these experiments, we may deduce the general principle, viz. that in combustion the molecules of oxygen which combine, disengage positive electricity, which can be communicated to the nearest molecules not yet combined, while combustible bodies, on the contrary, disengage negative electricity, which can, in a similar manner, communicate itself to all the surrounding combustible parts.

Part II.

On the Electricity produced by Vegetables.

After having ascertained as far as was in my power the truth and fertility of the principle I have now announced, saw the possibility of applying it to the combinations which operate in nature, and especially to those which are incessantly produced by the leaves of vegetables with atmospheric air. We know by the experiments of Priestley, Ingenhouz, and Sennebier, and above all, by the accurate and ingenious inquiries of Mr. Theod. de Saussure, that the various parts of plants act upon atmospheric air; that sometimes they form, at the expence of the oxygen, a large enough quantity of carbonic acid, which disengages itself insensibly; and that they sometimes exhale the oxygen pure, proceeding from some combination which takes place in the interior of the plant.

But if it is true that all carbonic acid is electrified vitreously at the moment of its formation, it follows that the plants ought to produce in the air by the exhalation of this acid, a quantity of vitreous electricity more or less considerable. This was the chief object of my researches; and I was very impatient for the fine weather to arrive, to prove this result, which appeared to me a necessary one. Since the month of March, I have made in my laboratory a sufficient number of experiments to show that vegetation is an abundant source of electricity; and consequently, a powerful cause to produce atmospheric electricity. My experiments were made in the following manner:—

Twelve capsules of glass from 8 to 10 inches in diameter, are coated externally, and only towards the edge, to a distance of one or two inches, with a film of gum lac varnish. They are arranged in two rows beside one another, either by placing them simply upon a table of very dry wood, or by putting them upon a table itself varnished with gum lac. They are filled with vegetable mould, and they are made to communicate with each other by metallic wires, which go from the interior of the one to the exterior of the other, passing over the edges of the capsules. Then all the insides of the twelve capsules, and the mould which they hold, form but one conducting body. Suppose that from any cause whatever electricity is communicated to this system, it will distribute itself in the twelve capsules, and cannot run into the earth, nor even pass into their exterior surface, for it will be arrested upon the edges of each of them by means of the film of gum lac. But instead of thus giving them electricity, which it would be perhaps difficult afterwards to take away, they are made to approach a condenser. The superior plate is put in communication with one of the capsules by means of a wire of brass, and its inferior plate in communication with the ground by the same means. These communications are established in such a manner as to maintain themselves during several hours or even several days. In the mould of the capsules we now sow the grain, (corn for instance), the effects of which we intend to study. The moment the experiment is begun, the laboratory is to be closely shut, and neither fire nor light, nor any electrical body, is to be admitted.

In the dry north and east winds of the month of March, these precautions were sufficient, and I observed the following phenomena :—

During the two first days, the surface of the mould was dried up, and the grains swelled ; the germ had come out of its envelope about a line, without appearing above the thin stratum of earth which covered the grains ; and the condenser, after repeated trials, gave no trace of electricity. The third day the germs had come out of the ground, and begun to raise their points towards the window, which had no shutters ; then, upon trying the condenser, I saw for the first time a divergence in the gold leaves. Thus the rapid action which the rising germ exercises upon the oxygen of the air disengages electricity. This electricity is resinous in the capsules, and consequently vitreous in the gas which it disengages. The apparatus is put into its usual state, and after the lapse of some hours, it is charged with a fresh quantity of electricity. It is curious to observe the effects of night, for we know that during this period in general the plants comport themselves otherwise with respect to air.

The next day, in the morning, upon visiting my apparatus, it gave a strong electric charge, and the electricity had not changed its nature. From this moment the vegetation has continued active enough during eight days ; and in this interval, I had incessantly observed the condenser at all hours of the day;—and during the night after sunset, or at an hour of the night more advanced, or early in the morning, or at sunrise, the electricity had always shown itself in quantities more or less great, according to the time that had elapsed. After twelve hours, the divergence of the gold leaves was more than an inch, and in all these experiments the earth of the capsules took always the resinous electricity.

After the first eight days the weather changed; a great humidity penetrated into the laboratory in spite of every precaution, and it was then impossible to collect the least quantity of electricity.

Twelve other capsules were ready, in which another vegetation had begun; and as it had been very active while the first had become languishing on account of the dryness, I imagined that this new vegetation would give me very strong marks of electricity; but after having tried it with the greatest attention, I found it impossible to draw any thing from it. Thus thwarted by the weather, there was but one way for me to counteract these variations, and to make continuous experiments: It was to shut the laboratory still closer, and to maintain a suitable degree of dryness, by means of absorbent bodies. Several bushels of quicklime broken into small fragments were spread in a very large apartment; several kilogrammes of muriate of lime distributed in saucers of porcelain were placed near the capsules of vegetation, and at last, after five or six days of the drying action of all these united agents, I produced artificially an atmosphere sufficiently dry and similar to that of the month of March. After this all the electrical signs appeared again, even with more intensity, and henceforth, as I could counteract the influence and the variations of the weather, I multiplied the experiments as much as was necessary. I made in this manner two vegetations of corn, two of cresses, one of gillyflower, and one of lucerne. In each operation the developement of the vegetable action, and that of the electrical phenomena which accompanied it, were observed during ten or twelve days.

It was a singular circumstance, that after the three or four first days of vegetation, if the condenser was put into a natural state after one observation, and if it was then replaced for experiment only during one second, it was then found to be charged with electricity. But it is evident, that during one second the weight of oxygen, which combines or disengages during a languid vegetation, which has only three or four square feet long, is a weight so feeble, and a fraction of a millegramme so imperceptible, that the electricity which it disengages is not sensible to the condenser. One is apt to fear after this that the electricity has another source, and that it can only be developed by some foreign cause; but upon reflection we see that the earth of the capsules is so dry that it becomes an imperfect conductor; that the electricity is retained; and that it is it which charges the condenser. To be certain of this, it is sufficient to place successively in contact with the condenser 1, 2, 3, or a greater number of capsules, and we shall see the charge increase in proportion as the number increases; in short, it is sufficient to place them in communication with the ground for a long time, when they will no longer give a charge to the condenser, and it will be many hours after that before they communicate a sensible electricity. It is without doubt this imperfect conductibility of the dried earth which has rendered it impossible for me to observe until now any electrical charges during the periods of day or night, although I took every precaution to observe it, presuming, that, if the disengagement of carbonic acid produce resinous electricity in the ground, the disengagement of oxygen ought, on the contrary, to produce vitreous electricity.

It is perhaps the same cause which has given birth to another phenomenon, which I have not yet studied sufficiently to give an exact account of it. It happened twice that the electric signs had ceased during two or three days, and that they were then presented in opposite directions,—that is to say, the capsules had exhibited vitreous electricity, and had continued to exhibit it with a very weak intensity during the rest of the vegetation.

The following are the results of all these experiments:—

1. That the gases disengage electricity when they combine either with one another, or with solid or fluid bodies.

That in these combinations the oxygen disengages always positive electricity, and the combustible bodies negative electricity; and that reciprocally, when a combination is dissolved, each of the elements wanting the electricity which it had disengaged, finds itself in an opposing electric condition. This reciprocity shows in what the nascent state differs from the definitive state of a body.

2. It follows that the action of vegetables upon the oxygen of the air is one of the most permanent and powerful causes of atmospheric electricity; and if we consider, on the one hand, that a gramme of pure charcoal passing into a state of carbonic acid disengages electricity sufficient to charge a Leyden phial; and, on the other hand, that the charcoal which is engaged in the constitution of vegetables does not give less electricity than the charcoal which burns freely, we may conclude, as my experiments all tend to establish, that upon a surface of vegetations of 100 metres square, there is produced in one day more vitreous electricity than is wanted to charge the strongest electrical battery.

This origin of the electricity of the atmosphere being once demonstrated by rigorous experiments, it remains to be seen what becomes of it,—by what laws and what properties it is propagated in the air,—disperses, ascends, and accumulates in the highest regions of the atmosphere. I have already collected some fundamental data upon this subject, and I hope that my other occupations will allow me to prosecute the inquiry.

ART. VIII.—*Comparative experiments on different Dew-point Instruments; with a description of one on an improved construction.** By Mr John Adie. Communicated by the Author.

Having had occasion to make use of dew-point instruments in some late experiments with the barometer for measuring heights, and having observed in the public journals various objections to different constructions of that instrument now in use, my attention was called to these defects; and the results which I obtained during this examination will form the substance of the following paper.

Mr Daniel objects to the simple instrument constructed by Mr Jones of London, and at the same time by Dr Coldstream of Leith, because a part only of the oblong bulb is exposed to a depression of temperature produced by the evaporation of

* Read before the Royal Society of Edinburgh, Feb. 2, 1829.

ether, while the upper part of the bulb, where the deposition of dew is observed, is cooled only by the conducting power of the mercury, its temperature being also kept up by that of the atmosphere.

The error in Mr Daniel's instrument is the converse of this, as stated by Mr Foggo in the *Edinburgh Journal of Science*, (No. xiii. p. 37.) The ether inclosed in the bulb on which the deposition is observed being cooled by the evaporation from its surface, the whole mass must acquire the temperature from the conducting power of the fluid alone; and as the enclosed thermometer is half immersed in the ether, and half exposed to the temperature of its vapour, while the deposition takes place only on a zone at the surface of the ether, a zone only on the bulb of the enclosed thermometer is exposed to the dew-point temperature, the other parts retaining the temperature of the ether below, and of the vapour above; thus the instrument gives a dew-point always at a higher temperature than the truth. These results I have obtained in using the instruments, and I shall illustrate them hereafter.

To obviate these defects, I first proposed to construct Dr Coldstream's instrument with a round instead of an oblong bulb, covering it entirely with muslin, except a small space $\frac{1}{5}$ of an inch in diameter, where the deposition might be observed, instead of covering little more than the half, as done when the latter is used, and thus get the better of unequal cooling; yet on trial it was found that this instrument differed in its results from the others, and also from Saussure's method, viz. the slow cooling of a quantity of water in a bright vessel, until a deposition is observed on its surface, which appears the best method of obtaining the dew-point of the atmosphere, the only thing required being to construct a convenient and portable instrument from which the same result should be obtained.

Having failed in this attempt, the interposition of a stratum of liquid between the cooling surface and the thermometer next suggested itself. The advantages of this method appeared to be, that the liquid might be kept in motion round the bulb of the thermometer, and therebykeep all at an equal temperature: this was obtained by the following construction:—

A thermometer having a small bulb is enclosed in a bulb or case of black glass, covered with silk, leaving a small space of about $\frac{1}{4}$ of an inch in diameter, where the deposition is to be observed; the space between the outer and inner bulb is nearly filled with any liquid which will not freeze by the depression of temperature required for obtaining a dew-point, as alcohol or water mixed with a quantity of salt: when the instrument is used, the liquid is kept in motion by the hand.

With this instrument, which is shown in Plate II. Fig. 1, I have obtained constant results, and a dew-point always the same as given by Saussure's method, the greatest difference being half a degree, and that only in three or four instances,—a difference that may very well arise from errors in observation. The temperature of the atmosphere is first found with it as with the common thermometer; the result is easily obtained, and without the use of much ether in cooling: and the instrument is not larger than the common pocket thermometer.

I may here mention that I had constructed an instrument with a brass covering bulb, into which was inserted a piece of black enamel, on a thin plate of gold, which was in contact with the enclosed liquid; with this I obtained rather an unlooked for result. When the ether was applied, the brass, being exposed to a great depression of temperature, conducted the heat from the enamel with greater rapidity than the enclosed liquid could impart its temperature; thus the surface of the enamel acquired a lower temperature than the enclosed thermometer, and gave a dew-point higher than the truth. As this effect might also have arisen from the tendency of the surfaces of different substances to acquire moisture from the atmosphere, when reduced to a temperature near the point of saturation, to determine this, I made the following experiments:—A vessel of brass having a piece of gold, silver, enamel and glass, set into it, was used to cool water by the slow process of Saussure, and a deposition of moisture was observed on all the surfaces at the same temperature. The glass took a little longer time, but required no farther depression of temperature. An instrument was constructed with a small plate of gold, which gave the same results as when the enamel was used.

Mr Daniel's instrument is always higher than the truth. In some cases the error amounts to 6 or 7 degrees, as shown by the following table; and the mean of 28 observations gives +2.9°. I have also found the indications of this instrument to depend very much on the purity of the enclosed ether used in its con struction. The purer it is the sooner will a dew-point be obtained, and the farther from the truth. The first I used was filled with the common ether of commerce. This gave a dew-point lower than when filled with that of a purer quality. Some of the first observations in the table were made with this instrument, but in some cases a dew-point could not be obtained; and in all cases in which it was obtained, a much greater quantity of ether was expended on the cooling bulb. I had, therefore, an instrument constructed with pure ether, which gave a result with great ease, but it always required a greater quantity of ether to cool it than the other instruments. The error in this instrument may easily be shown by the following experiment:—When the ring of dew is formed round the bulb, and its temperature observed, let the enclosed ether be agitated, keeping up at the same time the process of cooling, and the whole bulb will be dewed over, the enclosed thermometer being then observed, it will have sunk several degrees, and will be at the correct dew-point.

The result obtained by the thermometer having its bulb covered excepting a very small spot is most remarkable. The error in its indications seems to arise from two causes: *First*, a stratum of mercury in contact with the glass is constantly exposed to a depression of temperature which must be communicated to the mass. *Secondly*, glass being a bad conductor of heat, the space where the deposition is observed requires a certain portion of time to acquire the same temperature as the mercury; so that to render such an instrument perfect, it would be required that both mercury and glass should be instantaneous conductors of heat; the principle of correct thermometers being, that all their parts shall have the same temperature; and where any substance is used as a measure of temperature, it must expand in all its parts simultaneously, or an allowance be made for a known quantity having a different temperature. These effects may be illustrated by using more or less ether in the process of cooling; for the more quickly the temperature is depressed, the farther will the dew-point given be from the truth, in some cases 5 or 6 degrees; whereas when it is cooled very slowly, a result may be obtained within 2 or 3 degrees of the correct point. The mean of 28 observations given in the following table is —4.78 degrees. The oblong bulb gives a dew-point 8 or 9 degrees below the truth; and the mean result from 28 observations in the table is —6.6 degrees. In addition to the objections mentioned by Mr Daniel to this instrument, those here stated regarding the round bulb tend to cause a similar error in its results, though to a less extent.

Table of the Dew-Point given by the different instruments.

1828.		Temp. Air.	Saussure's method.	New Instru-ment.	Daniel's.	Round Bulb.	Elong. Bulb.	Diff. betwixt air and D. P. by Saussure.
Aug.	13,	55°	45°	45°	46	43		10
	14,	55	44	43.5	45	41	41	11
evening		52	44	44	47	42	39	8
	15,	54	41	41	42	37	39	13
Oct.	1,	63	54	53.5	55	50	33.5	9
	14,	50	43	43	44	41	47	7
	15,	54	44	43.5	45	40	40	10
	18,	51	45	45	47	42	40	6
	19,	42	32	32	32	30	41	10
	23,	55	46	46	47	44	29	9
Nov.	3,	47	39.5	39	41	38	40	7.5
	4,	51	43	43	46	41	34	8
	8,	50	38	38	41	35	35	12
	10,	42	28	28	31	23	32	14
	19,	34	27	27	32	24	20	7
	22,	48	39	39	46	37	18	9
Dec.	1,	47	38.5	38.5	42	32	32	8.5
	3,	42	30	30	34	23	30	12
	15,	45	35	35	39	32	20	10
	18,	42	33	33	38	29	25	9
	24,	47	42	42	43	37	26	5
	25,	43	30	30	35	27	35	13
1829.	28,	41	32	32	35	29	25	9
Jan.	4,	39	26	26	29	23	25	13
	15,	39	26	26	29	22	20	13
	19,	38	26	26	31	22	22	12
	20,	28	21	20.5	24	17	17	7
	21,	32	17.5	17	24	14	16	15
							13	
Sum,		1286	1009.5	1006.5	1090	915	824.5	277
Mean,		45.90	36.03	35.93	38.93	31.25	29.43	99.

ART. IX.—*Notice of the performance of Steam-Engines in Cornwall for January, February, and March* 1829. By W. J. HENWOOD, Esq. F. G. S., Member of the Royal Geological Society of Cornwall. Communicated by the Author.

Reciprocating Engines drawing Water.

Mines.	Diameter of cylinder in inch.	Length of stroke in cylinder in feet.	Length of stroke in the pump in feet.	Load in lbs. per sq. in. of area of piston.	No. of strokes per minute.	Millions of lbs. weight lifted 1 foot high by the consumption of 1 bush. of coal.
Stray Park, -	64	7,75	5,25	7,6	5,2	25,
Huel Vor, -	63*	7,25	5,75	17,5	5,5	27,3
	53	9,	7,5	19,5	6,	40,6
	48	7,	5,	8,	6,1	31,4
	80	10,	7,5	13,8	6,4	55,9
	45	6,75	5,5	13,7	6,5	48,2
Poladras Downs,	70	10,	7,5	9,	6,6	47,2
Huel Reeth,	36	7,5	7,5	15,2	3,9	26,3
Balnoon, -	30	8,	7,	7,4	3,8	23,4
Huel Towan, -	80	10,	8,	10,3	6,6	76,
	80	10,	8,	5,1	3,8	57,5
United Hills, -	58	8,25	6,5	6,7	4,7	37,9
Great St George,	70	10,	7,5	10,2	5,4	35,2
Perran Mines,	38	6,75	6,	8,2	9,6	20,6
Crinnis, -	56	6,75	6,75	9,1	6,	42,9
Huel Unity, -	52	6,666	5,75	6,6	7,9	21,9
	60	7,25	5,75	14,4	6,4	33,
Poldice, -	90	10,	7,	10,1	6,1	44,5
	60	9,5	6,25	11,9	7,5	32,
Huel Damsel, -	42†	7,5	5,75	21,5	6,9	32,7
	50	9,	7,	8,2	3,	34,7
Ting Tang, -	63	7,75	6,75	15,	7,5	40,6
	66	9,	7,5	9,4	2,	38,4
Cardrew Downs,	66	8,75	7,	8,2	6,9	49,8
Huel Montague,	50	9,	7,	8,3	6,4	38,6
Great Work, -	60	9,	7,	9,1	7,	42,2

Mines.	Diameter of cylinder in inch.	Length of stroke in cylinder in feet.	Length of stroke in the pump in feet.	Load in lbs. per sq. in. of area of piston.	No. of strokes per minute.	Millions of lbs. weight lifted 1 foot high by the consumption of 1 bush. of coal.
Huel Penrose, -	36	8,5	6,6	11,	10,1	33,3
Carzise, -	50	8,5	7,5	7,3	4,	22,8
Huel Caroline,	30	7,	6,	28,	12,6	34,7
St. Ives Consols,	36	7,	7,	16,1	6,4	27,9
Lelant Consols,	15	7,5	4,5	17,2	3,	14,4
Binner Downs -	70	10,	7,5	10,1	8,1	63,9
	63	9,	7,5	7,8	9,5	37,
	42	9,	7,5	12,4	7,4	41,8
Dolcoath, -	76	9,	7,5	11,8	5,9	42,7
Consolidated Mines,	90	10,	7,5	8,8	5,3	58,5
	70	10,	7,5	9,4	5,6	61,2
	58	7,75	6,5	18,5	7,	45,1
	90	10,	7,5	7,8	4,6	60,5
	90	10,	7,5	10,3	3,8	40,3
	70	10,	7,5	8,8	5,3	62,8
United Mines, -	90	9,	8,	7,9	5,2	43,
	30	9,	7,5	12,9	7,8	33,4
Huel Beauchamp,	36	7,75	6,	12,7	4,1	30,2
Huel Busy, -	70	10,	7,5	11,5	8,4	48,2
Huel Rose, -	60	9,	7,	12,	6,3	48,4
Pembroke, -	80	9,75	7,26	11,4	3,7	48,2
	40	9,	6,5	6,1	2,3	24,6
	50	9,	6,5	9,4	5,8	38,8
East Crinnis, -	60	5,5	5,5	8,5	4,8	25,4
	70	10,	7,	8,4	4,9	36,6
East Huel Unity,	45	8,75	6,75	7,9	4,5	27,8
Huel Hope, -	60	9,	8,	10,5	6,4	69,7
Huel Tolgus, -	70	10,	7,5	7,7	4,6	48,
Tresavean, -	60	9,	7,	5,6	4,	20,
Huel Falmouth,	58	8,75	6,5	3,3	7,5	26,

All the other reciprocating engines are Watt's single. Average duty 39.64 millions of lbs. lifted one foot high by the consumption of one bushel of coal.

Watt's rotatory double engines employed to move machinery for bruising tin ores.

Huel Vor,	24.	6.	6.	12.	17.2	19.5
	27.	5.	5.	12.	16.7	18.7
	16.5	5.	5.	8.5	25,4	12.6

Average duty of rotatory engines, 16.9 millions.

* Watt's double engine.

† The steam after passing through a high pressure escapes into a Watt's single engine.

Art. X.—*Notice respecting a spontaneous emission of Inflammable Gas, near Bedlay, about seven miles north-east from Glasgow.* By Thomas Thomson, M. D., F. R. S. L. and E., &c. Regius Professor of Chemistry Glasgow*. Communicated by the Author.

About five weeks ago, a pretty copious emission of inflammable gas was observed along the banks of a rivulet which crosses the north road between Glasgow and Edinburgh, a little to the east of the seventh milestone from Glasgow, and only a few hundred yards from the house of Bedlay. The emission of this gas has been observed only on the south side of the road. It it is said to extend for more than half a mile along the banks of the rivulet. But I myself saw it only in a space which might be fifty yards in length, and perhaps half as much in breadth. The emission of gas was visible in a good many places along the declivity to the rivulet, in the immediate neighbourhood of a small farm-house. The farmer had set the gas on fire in one place about a yard square, out of which a great many small jets were issuing. It had burnt without interruption during five weeks, and the soil (which was clay) had assumed the appearance of pounded brick all around. The flame was yellow and strong, and resembled perfectly the appearance which *carburetted hydrogen gas* or *fire damp* presents when burnt in day light. But the greatest issue of gas was in the rivulet itself, distant about twenty yards

* Read before the Royal Society of Edinburgh, January 5, 1829.

from the place where the gas was burning. The rivulet when I visited the place was swollen and muddy, so as to prevent its bottom from being seen. But the gas issued up through it in one place with great violence, as if it had been in a state of compression under the surface of the earth; and the thickness of the jet could not be less than two or three inches in diameter. We set the gas on fire as it issued through the water. It burnt for some time with a good deal of splendour; but as the rivulet was swollen, and rushing along with great impetuosity, the regularity of the issue was necessarily disturbed, and the gas was extinguished.

There is a thin bed of very fine-grained blue limestone in the immediate neighbourhood, which had been wrought formerly a little to the east of the field where the issue of inflammable gas is at present observed. During the course of last summer, Mr William Dickson began to work this lime-bed about three quarters of a mile to the south of the Cumbernauld road. The limestone bed is about five feet thick, and, like all the other beds in this immediate neighbourhood, dips to the north-east, just in the opposite direction that the beds a little to the west and south take; all of which dip towards the Clyde. No doubt the dip has been altered by the intervention of some greenstone dike; and indeed greenstone may be seen a little to the west; but neither the weather nor the state of the country permitted me to trace the connection between the greenstone and the dip of the strata.

A good section of the strata is presented by the railway that has been lately made directly to the east of the rivulet, and which passes through a tunnel immediately under the Cumbernauld road. This section presents the usual coal metals, slate-clay, limestone, coal, shale. Figure 2 of Plate II. presents a rude outline of the position and relative thickness of the different beds. The uppermost bed of slate-clay, about twenty feet thick, is composed of innumerable thin strata of slate-clay, some of them blue and some black, like shale. The limestone immediately under the slate-clay is five feet thick. Next comes a bed of coal one foot thick. Below the coal is a bed of slate-clay of unknown thickness, as it has not been cut through. These beds, (if we make allowance for the supposed change of dip) are undoubtedly under all the Glasgow coal beds. From the situation of the field where the evolution of inflammable gas takes place and the dip of the strata, I conceive that the rock or bed through which the gas issues is the undermost bed of slate-clay in the preceding sketch.

When Mr Dickson began last year to work the lime-bed, the workmen were so impeded by water that they could not proceed in their operations. This induced him to set up a small apparatus for draining the quarry. It was after this had been acting for some time, that the issue of gas began to be observed. Suspecting that the great quantity of water which incommoded his quarrymen proceeded from the rivulet, making its way through the slate-clay bed, and rising to the limestone, Mr Dickson employed persons to examine the course of the rivulet, to endeavour to discover any indications of the supposed sinking of the rivulet. The issue of gas through the rivulet was observed and considered as favourable to this notion, that the water in the quarry proceeded from the rivulet. It was supposed to be common air issuing from the hollow places under ground, as they became filled with water. But some persons happening to apply a light to the gas, it took fire, and showed that it was of an inflammable nature.

Whether the gas had been issuing before Mr Dickson's attempt to drain his quarry, or whether the issue was not the consequence of this draining process, must be left to conjecture. I am inclined to believe that the draining occasioned the appearance of the gas. Coal has never been wrought in the immediate neighbourhood. The nearest workings are about three miles to the south-west, and from the dip of the strata it is impossible that the gas can proceed from them. The coal beds in that place must be situated higher up than the stratum out of which the gas comes. I am not aware of any coal mines to the north-east nearer than eight or ten miles. I think it probable from this, that some cavity exists in the earth situated below the slate-clay, through which the inflammable gas issues, and situated to the north-east of it—that a bed of coal forms the floor of this cavity—that water had got access to this cavity, and, acting on the coal, had occasioned the evolution of carburetted hydrogen gas. The slate-clay, from being always soaked with moisture, would be impervious to this gas, so that it had been accumulating for some time, and existed in the cavity in a state of condensation. It is easy to see how the draining process would occasion a shrinking in the bed of slate-clay; cracks would be formed in it through which the gas would find its way to the surface. If this explanation be the true one, the escape of gas will continue till the gas in the cavity has reduced itself to the same state of elasticity as the external air. It will then stop or will only appear occasionally, and will become gradually diluted with common air, so as at last to lose the property of inflaming. To enable the evolution of gas to be perpetual, it is obvious that it would require to be formed as fast as it escapes.

I collected a quantity of the inflammable gas, and subjected it to a chemical examination. It is destitute of all smell, and possesses all the mechanical properties of common air. It contained not the least trace of carbonic acid gas, which is always present when this gas is formed at the bottom of stagnant pools of water. As the gas which I collected ascended through the rivulet, flowing at the time with considerable velocity, it may be supposed that carbonic acid gas might have been mixed with the gas in its original repository; but that it was all washed away while the mixture passed up through the water. But from the great violence with which it issued, I do not think that this could have been the case. For if you mix carbonic acid and carburetted hydrogen gases, and pass the mixture through a column of water several feet long, a considerable portion of the carbonic acid will still continue in the mixture.

The gas was not quite free from common air. A careful set of experiments varied in different ways satisfied me that the volume of common air in the gas was exactly 12.5 per cent. or the gas was a mixture of

Carburetted hydrogen,	87.5 volumes
Common air,	12.5
	100

The carburetted hydrogen gas which constituted so great a portion of the mixture was quite pure. For it took exactly twice its volume of oxygen gas to consume it, and it left, when fired by electricity, exactly its own volume of carbonic acid gas.

It is well known that carburetted hydrogen is a compound of

	Sp. gr.
2 volumes hydrogen gas,	0.1388
1 volume carbon vapour,	0.4166
	0.5555.

condensed into one volume. Hence its specific gravity is 0.5555. And the specific gravity of a mixture of 87.5 volumes carburetted hydrogen and 12.5 common air, is 0.6109. This gas then, which issues in such abundance, might be used to fill air balloons. It would answer the purpose almost as well as coal gas. Were we assured of its continuing to issue always in as great abundance as at present, it might be employed lighting the streets of Glasgow. But pure carburetted hydrogen gas would not give so much light as coal gas. For I find that coal gas is always mixed with more or less of the vapour of naphtha, which adds considerably to the brilliancy of its combustion.

GLASGOW, *27th December* 1828.

ART XI.—*Observations on the spontaneous emissions of inflammable Gas, in particular of Carburetted Hydrogen.** By ROBERT BALD, Esq. F. R. S. E. &c. &c. Communicated by the Author.

AN interesting paper having been read before this Society last month by Dr Thomas Thomson, Regius Professor of Chemistry in Glasgow, regarding the spontaneous emission of inflammable gas near Bedlay, situated about seven miles N. east from Glasgow, I beg leave to offer a few observations thereon, and have also to state where similar phenomena have taken place.

The issue of gas in the rivulet at Bedlay may not have been observed till lately, and perhaps it did not issue until the

* Read before the Royal Society of Edinburgh, March 16, 1829.

operation in the lime-quarry took place, as particularly noticed by Dr Thomson; but that inflammable gas issued from the cutters of this lime rock in great abundance has been known to me for at least twenty-five years; and it was a common practice of the workmen to keep the gas burning as a matter of curiosity. It was then concluded that the gas was generated in the coal which lies immediately under the limestone, and if the slate-clay under the coal is full of cutters or fissures, an additional supply of gas may proceed from an under-coal; or, if the slate-clay is of a close and impervious nature, and the coal full of fissures, a slip of the strata, so common in coal-fields, may connect the whole of the coals of this place together, and produce an uncommon issue of inflammable gas.

This is a circumstance well known in the collieries situated on the rivers Tyne and Wear in the north of England; and it is in this instance of slips or dislocations of the strata having an open vize or fissure, that those terrible and most dangerous issues of inflammable gas, known there by the name of blowers, are found. These, when first struck, issue with the force of steam from an engine boiler, and with uncommon noise; and this issue continues sometimes for many years.

It is, I think, probable, that the gas has issued from the bed of the brook, as noticed by Dr Thomson, for many years past; and the circumstance of the workmen looking carefully for the ingress of water into the quarry from the bed of the river, may have led to the discovery of the issuing of the gas; and I think it very likely with Dr Thomson, that the water filling the excavated part of the lime rock may have greatly contributed to the more violent issue of the gas at the time when Dr Thomson made his observations; besides, if the slate-clay is of the bituminous kind, it may be another source from which the gas comes. Some of this kind of slate-clay burns with a strong and lively flame, but the bulk of the slate-clay is very little altered by the burning

Dr Thomson notices, that at the excavation made in the surface soil, where the gas had continued burning for several weeks, the clay had the appearance of pounded bricks. I have observed that when miners kept the gas which issued from the coal burning constantly, and which had a flickering flame, for the purpose of lighting their candles, it had the effect of gradually perforating the solid coal, and forming a recess like a bason, but the coal never appeared to be ignited; on the other hand, when blowers have ignited, their strength and force of flame are such, that they have acted like an immense blow-pipe, and set on fire the coal at the distance of twelve feet from the spot where the gas issued.

When the engine-pit at Preston Island was putting down, which is situated upon the estate of Sir Robert Preston, Bart. near the town of Culross, and about three-fourths of a mile within the high water mark of the river Forth, I went several times down this pit to see the issuing of the inflammable gas. We knew that the strata had not been opened up before; but there were many cutters, fissures and beds in the rock, and long before the miners reached the coal, the inflammable gas issued through the fissures and beds of the sandstone rock, and made the water in the pit boil like a pot, or not unlike liquor in a violent state of fermentation. During the process of sinking, the workmen observed air bubbling up in many places through the sludge or sleech which composed the surface which was laid dry at every ebb tide, and at a considerable distance from the pit; and it was a common amusement of the workmen to make cones of clay, each perforated with a small hole, and put them over the places where the gas issued. These they ignited, and they flamed like large coal-gas burners.

Upon the miners reaching the coal it proved to be very full of inflammable air, and several severe and fatal accidents were the consequence; one blast was so remarkably strong as to kill a workman who was standing at the surface by the side of the pit.

I have also to remark, that it is a common occurrence where bores are put down to coals which are full of inflammable gas, and intersected with fissures, to find a strong issue of gas at the surface, and this will continue to be emitted for years. A remarkable instance of this happened in putting down a bore in the trough of the Glasgow coal-field some miles east of the city. When the gas was ignited, the discharge was such as to produce a flame of from eight to ten feet in height. From this we infer that a large cavity is not necessary for producing the spontaneous emission of gas; but a cavity or excavation greatly favours its flowing from the coal, for a long mine or gallery cross-cuts the fissures, and allows the gas to come off freely, and in great abundance.

Dr Thomson mentions the proximity of greenstone to the spot where the coal and lime-rock of Bedlay are found. I have upon this to remark, that in the coal-fields of Scotland inflammable gas is frequently found in the coal-fields where beds of greenstone abound.

The following collieries have inflammable gas and beds of greenstone in connection with the strata:—Bannockburn, Plean, and Greenyards in the vicinity of Stirling; Bo-ness, and the greater part of the collieries in Ayrshire, and at Mr Houston's colliery of Johnston in Renfrewshire.

It is remarkable, that in the great and extensive collieries and coal-fields in Clackmannanshire, and along the whole coast of Fife, inflammable gas has not been found, excepting in a trifling instance at Lord Elgin's colliery, and in it greenstone also abounds. Preston Island is in the county of Perth.

Upon the south side of the Forth, with the exception of the Bannockburn and Bo-ness districts, there is no inflammable gas found; and the districts free from inflammable gas comprehend the very extensive coal-fields around the Carron works and Falkirk, and, without exception, all the collieries in the Lothians; but in these collieries beds of greenstone are not found. Dikes or vertical veins of this are found in several places.

What is thus stated as to the connection with greenstone and inflammable gas is not the case in every instance; for the Glasgow coal-field abounds with hydrogen gas, and there are no beds of greenstone in the chief part of the field; but they very much abound in the district around the town of Airdrie, and are found in the north part of the city of Glasgow, and to the north-east.

It appears that some coals throw off inflammable gas freely, while others of the same quality and constituent parts will yield no gas without the application of heat—a circumstance not easily accounted for,—at the same time I am led to infer, that when the Scotch coals are long exposed to the action of the sun and weather, much of the inflammable gas is dissipated, and this

I conceive to be the cause why coals so exposed are so much deteriorated in quality, and make so dull and cheerless a fire; and we know that no coals give so much flame and heat as when drawn wet from the mine and put into the fire.

This striking difference in coal-fields as to inflammable gas abounding in one district, and not being found in another, is a matter upon which no satisfactory theory has as yet been formed.

In some of the Newcastle coals, the inflammable gas is so very easily disengaged, that there have been several instances where coals recently drawn from the mines and instantly shipped, have, by the fall and breaking of the coals descending into the ship's hold, disengaged such a quantity of inflammable gas, as to ignite from the flame of candle, by which the hatches were violently blown up, and the sailors severely scorched. This circumstance shows how very easily this gas is in some instances emitted from coal, and it must be in great abundance when we know that one pound weight of some coals, will yield five cubic feet of this gas when exposed to fire in a retort.

The analysis of the gas issuing at Bedlay is interesting, and the more so from the dependence which can be placed on Dr Thomson's accuracy.

Art. XII.—*On a solid form of Cyanogen or its Elements, and a new compound of Carbon and Azote.* By James F. W. Johnston, M. A. Communicated by the Author.

I. When cyanide of mercury is employed in the preparation of cyanogen, there remains in the tube after the gas ceases to come over, a blackish residuum resembling charcoal. The weight of this substance obtained from a given quantity of the pure dry salt is at all times small, though it varies much in apparent quantity. Sometimes it is exceedingly light and bulky, at others it has a fused appearance, and where it has been in contact with the tube, a shining metallic lustre. It varies also in hardness and density, occurring in all states from that of a "light charcoal," as described by Gay Lussac, to that of a hard, dense, and sonorous body. In the mass it is of a black or olive black colour, but where thinly spread over the inner surface of the tube, it is by transmitted light of a brownish red. It is easily rubbed to powder, and soils the fingers. In the flame of a lamp it burns very slowly and without noise or flame. Heated to redness in a glass capsule it gives off no fumes, and is dissipated with extreme slowness, leaving no appreciable residuum;—at a higher heat in a silver or platinum crucible, it melts and disappears more rapidly. It will be seen in the sequel of this paper, what changes take place in the chemical constitution of this substance when thus heated in contact with atmospheric air.

In the state of fine powder this substance is insoluble in alcohol, ammonia, or nitric acid. It dissolves in hot and concentrated sulphuric and muriatic acids, giving with the latter a light yellowish brown solution. After evaporation to dryness, the residuum from both acids is insoluble in water, that from the muriatic acid is of a reddish, from the sulphuric of a grayish black colour. It is partly taken up also by caustic potash, probably by its agency in effecting decomposition. Triturated in a mortar with chlorate of potash, it detonates by heat, but not by percussion.

This residuum has hitherto been regarded as a variety of carbon, and has, therefore, obtained little attention. It has been thought, that during the decomposition of the cyanide, a portion of the cyanogen also was decomposed, the carbon remaining in the tube, and the azote passing over with the cyanogen. But cyanogen is often obtained, almost, if not perfectly pure, while still a considerable portion of the carbonaceous matter is found in the tube. If such be the case, it is obvious that this substance must be something more than mere carbon. Accordingly, in analyzing it by means of chlorate of potash, I found it to be identical in constitution with gaseous cyanogen. The results of analysis uniformly give carbonic acid and azote in the proportion almost exactly of two volumes of the former to one of the latter. The mean of seven experiments which I here subjoin gives

Carbonic acid, - -	2.32	inches.
Azote, - - -	1.173	

where the proportion is clearly as above stated. While in each of the following experiments, the carbonic acid is so nearly double of the azote, that there can be no doubt of this substance being composed, like gaseous cyanogen, of

2 atoms carbon,
1 azote,

	Gas collected.	Carbonic acid.	Azote.
No. 1	3.04 inches.	2.0 inches.	1.04 inches.
2	4.99	3.2	1.79
3	1.89	1.28	.61
4	4.41	3.0	1.41
5	3.4	2.2	1.2
6	2.725	1.8	.925
7	5.7	2.76	1.24
Mean,	3.493	2.32	1.173

When prepared according to this process, I have found it impossible to obtain this compound entirely free from metallic mercury ; minute globules remaining attached to it after the most careful separation. Hence, in analyzing it, the weight of the resulting carbon and azote, in the above experiments, never equalled the weight of the carbonaceous matter employed. By exposing it to heat in a glass capsule over the flame of a spirit lamp, the mercury is volatilized and completely driven off; but before this is effected, a change in the composition of the substance itself takes place, of which I shall have occasion hereafter to take notice.

It became desirable, therefore, to find another process for obtaining this compound by which the presence of metallic or other foreign bodies might be completely prevented. It is known that when cyanogen is allowed to stand over mercury for a sufficient time, a black substance is deposited on the sides of the containing vessel. It is known also that when a solution of caustic potash saturated with cyanogen is exposed to an excess of that gas, the liquid becomes brown from the intermixture of black particles which seem about to be deposited. In both cases, it has been supposed, or rather taken for granted, that a portion of the cyanogen is decomposed, and that the black deposit is simply carbon. From what follows, I think it more probable that the deposit in both cases is the *bi-carburet* of azote I have above described.

When cyanogen is passed into alcohol over mercury, it is absorbed with great rapidity. According to Gay-Lussac, alcohol in this way absorbs twenty-three times its volume of the gas. If a solution thus saturated be left in contact with cyanogen over mercury for twenty-four hours or upwards, a further absorption takes place, amounting in all to thirty or forty volumes—the solution like that of potash becoming brown, then brownish red, and gradually deepening in colour as it stands. Of cyanogen which had stood over mercury for twelve hours, I found the alcohol of the shops on one occasion to dissolve forty volumes in a few minutes, becoming at the same time of a dark reddish brown colour; in general, however, it requires a much longer period. If now drawn off and set aside in a close vessel for several days, this solution deposits a sediment black by reflected and reddish-brown by transmitted light. The alcohol passes through the filter colourless, but on being set aside a second deposition of the black sediment frequently takes place, which, after a few days, may be separated in the same manner.

When the matter collected on the filter is washed with distilled water, the washings pass through of a yellow colour, showing that in this state it is partially soluble in water. Dried in a glass capsule at first by a gentle heat, and afterwards by the flame of a spirit lamp, one portion heated with chlorate of potash gave

Carbonic acid	= 2.92 inches
Azote	= 1.502

where the carbonic acid is to the azote nearly as 2 to 1.

A second portion, without any previous washing with water, was carefully dried at a heat not exceeding 212°. In mass it was of a shining black, in powder of a deep chocolate colour. Of this 7 grains were heated with 5 grains chlorate of potash. The gas given off amounted to 4.7 inches, and the loss of weight to 2.6 grains. The gas consisted of

Carbonic acid,	2.2 inches	= 1.025 grs.
Azote -	1.1	= .3261
Oxygen	1.4	= .4748

Weight of gas given off, = 1.826
Loss of weight in the experiment = 2.6

Deficiency, .774 grs.

Now the carbon in 2.2 inches carbonic acid = .2794 grs.
and 1.1 azote = .3261

.6055
Weight of substance analyzed, = .7

Deficiency, = .0944 grs.

But .0944 × 9 = .8496 gr. not greatly exceeding the first deficiency, which was therefore due to the formation of water, a very small quantity of which had most probably remained in the tube.

We have consequently the weight of hydrogen in the substance = .0944 grs.

Atom of Cyanogen.
and .605 : .0944 : : 3.25 : .505

But .505 is almost exactly 4 atoms hydrogen.

Therefore, in this state and dried at 212°, the substance, as deposited from the alcoholic solution, consisted of

Cyanogen or its elements,	1 atom
Hydrogen, - - -	4 atoms

By farther heat the hydrogen is driven off in combination probably, and there remain simply the elements of cyanogen.

Instead of filtering the alcoholic solution, the sediment may be obtained by distilling it in a retort. In this case also the colourless spirit which comes over being set aside, again becomes yellow, then dark red, and deposits a further sediment, provided it have not already stood so long as to give time for a deposition in the solid state of all the cyanogen it contains.

The solid matter remaining in the retort being taken out, and carefully dried at a heat not above 212°, gives a deep chocolate powder, having the smell and taste of rhubarb. Caustic potash decomposes it, giving off ammonia. Heated in a tube it gives white fumes, which condense on the sides of the tube and resemble rhubarb in colour, smell, and taste. When fumes cease to be given off, there remains a bluish-black substance of considerable density and lustre, and breaking like thin layers of coal into rectangular fragments.

Of this substance 8 grains were exploded with 8 grains of chlorate of potash. The products were

Carbonic acid,	- -	2.75 inches
Azote,	- - -	1.4

Now the weight of the carbon in the carbonic acid, added to that of the azote = .77 grains, which is near enough to .8 grains, the quantity employed, to be completely within the limits of error.

In this experiment, as in all the others above detailed, the carbon is to the azote as 2 : 1; we are warranted therefore in concluding, that the deposit from alcohol supersaturated with cyanogen, when dried by a sufficient heat, is a solid *bi-carburet* of azote. In the sequel of this paper we shall have further reason for concluding it to be identical also with the carbonaceous matter remaining from the decomposition of the cyanide of mercury.

One question occurs here to which we may advert. If this substance be identical in composition with gaseous cyanogen, does the difference in their properties arise from a new arrangement of the elements, or merely from their closer aggregation? It is nothing new in chemistry for substances of very different properties to be identical in composition. Of this kind are the acetic and succinic acids, but the number of atoms (=9) they contain, leaves ample room for various changes of arrangement. In the present case, however, there are only three atoms united, and of these two are carbon, so that there cannot by possibility be more than two identical combinations of these elements. There may be more if the atom of this solid bicarburet be greater than that of cyanogen, but this we are not entitled to assume till its compounds be investigated. Still, even if the atomic weights of these two compounds be the same, one different arrangement *may* take place; and I shall here state one fact, which inclines me to think that such a change of arrangement has taken place.

Alcohol newly saturated with cyanogen gives no precipitate with bichloride of mercury. But when it has become of the reddish brown colour already mentioned, it throws down a precipitate which at first is brown, but afterwards assumes a reddish tint. With nitrate of silver it gives a precipitate which is entirely characteristic. This precipitate at first is brown like that from mercury, but it gradually darkens, and, together with the supernatant liquid, becomes finally of a beautiful purple. Aqueous cyanogen gives with nitrate of silver a dirty black, and hydro cyanic acid a white becoming black, from both of which this purple precipitate is very distinct. We may infer then that the precipit*ant* is also distinct. Yet the carbon and azote exist in it in the same proportions; for the precipitate from mercury detonated with chlorate of potash gave me gases in the proportion of

2 volumes carbonic acid
to 1 azote,

The nature of this class of compounds, however, I intend, on some future occasion, more fully to investigate. *

II. Of the proto-carburet of azote.

I have already alluded to the change of composition which the bicarburet undergoes when subjected to a heat sufficient to volatilize and drive off the mercury with which it is mixed when obtained from the decomposition of the cyanide of that metal. The nature of this change will be seen from the following results:—

1. An indeterminate portion, after heating in this manner, was decomposed by chlorate of potash. The results were,

		inch.
Carbonic acid,	=	.93, or 3 atoms,
Azote,	=	.62, or 2 atoms.

* By a series of experiments, yet unpublished, I find the cyanide of mercury to consist of two atoms cyanogen, and one atom mercury. It is possible that these precipitates may consist of one atom cyanogen or its elements to one atom mercury. See Art. XIX. in this Number.

Two other experiments gave me similar proportions of these two gases, so that the substance analyzed may either be considered as a *sesqui*-carburet, or as a mixture of a proto-carburet with the bicarburet. The latter is the more probable.

2. Of another quantity heated in the open air till all metallic fumes had entirely ceased, .2 grs. were mixed with 3 grs. chlorate of potash, and 10 of pounded glass,* and exposed to the flame of a spirit lamp. The results were,

	inch.	
Carbonic acid,	= .55	or 7 atoms,
Azote,	= .455	or 6 atoms,

	inch.	
Now the carbon in	.55 of carbonic acid,	= .0698 grs.
and	.455 azote,	= .1349
	whole weight,	= .2047 grs.

Almost exactly the weight experimented upon.

Again 15 grs. of cyanide decomposed in an *uncovered* glass capsule by the heat of a spirit lamp left .35 of carbonaceous matter. Of this .3 detonated with 3 grs. of chlorate gave

	inch.	
Carbonic acid,	.82 or 7 atoms,	
Azote,	.685 or 6	
Now, the carbon in	.82 carbonic acid,	= .104 grs.
and	.685 azote,	= .203
Whole weight,		= .307

which also is exceedingly near the weight employed.

In these two experiments, then, the substance analyzed was a compound of 7 atoms carbon to 6 atoms azote.

3. The above experiments indicate a combination of the elements atom to atom, as the limit of the change produced by heat. To obtain this, a quantity of the bicarburet was heated in a glass capsule, till a great part of it was dissipated. Of

* In all these experiments, a considerable mixture of pounded glass was necessary to make the decompositions so progressive that the products could be collected.

the residue .55 grs. detonated with 10 grs. chlorate of potash gave,

	inch.	
Carbonic acid,	1.2 or 1 atom,	
Azote,	1.214 or 1 atom,	
And the carbon in	1.2 carbonic acid,	= .1524 grs.
and	1.214 azote	= .3599
	whole weight,	= .5123

which is also sufficiently near the weight employed to be within the limits of error.

In this case, then, the two elements were combined atom to atom, forming a simple carburet of azote,—a substance which, in external characters, much resembles the bicarburet already described. What action other substances have upon it I have have not ascertained.

The suite of proportions obtained in these experiments show very beautifully the nature of the change which takes place upon the bicarburet when heated in the open air. When newly prepared, the carbon is to the azote as 2 : 1. After considerable heating, the carbon is diminished, and the ratio becomes as 3 : 2. Again heated it diminishes to the ratio of 7 : 6—and finally, after a still longer heat, their gaseous volumes become equal. The carbon flies off in combination with the oxygen of the atmosphere, leaving the azote fixed till they reach this limit of equality, when by further heating both fly off together, and the whole is dissipated.

These substances, though hitherto unknown as compounds of carbon and azote, have often, I have no doubt, been met with by chemists, their appearance having generally led them to be considered simply as varieties of carbon.

Thus Scheele found that lithic or uric acid when distilled gave, among other products, a quantity of "coal which preserved its black colour on red hot iron in the open air." Now it has been shown by Dr Prout and Dr Thomson, that uric acid consists of

Carbon,	6 atoms,
Azote,	2
Oxygen,	1

There can remain little doubt, therefore, that the coal obtained by Scheele, was one of the carburets of azote above-mentioned; and the decomposition of uric acid by heat would probably give these substances more abundantly, and with greater ease than either of the methods I have pointed out. Other animal and azotized vegetable products, when decomposed by heat, may possibly leave similar compounds of carbon and azote.

A knowledge of the existence of such compounds will enable us often to state more distinctly the composition of animal and vegetable substances, as well as to reconcile to atomic proportions the presence of small quantities of azote among the other results of analysis, which, as in the case of mineral coal, some chemists have ascribed to the presence of foreign matter.

The external characters of these two compounds, so similar to those of coal, render more probable also those analyses of the various kinds of this mineral product, which show the presence of a large per centage of azote. Some chemists give only a small quantity of azote as the result of their analysis, while others find in some varieties, as in that of Newcastle, no less than 16 per cent. This estimate may probably be too high, yet, if one might judge from appearance, that of the bi-carburet would justify us in assigning to some varieties of coal a still larger proportion of azote. The slow combustion of these compounds, however, would lead us to expect much less azote in the caking coal of Newcastle, than in some of the less inflammable species called by mineralogists non-bituminous coal. The progress of analysis will probably soon put us in possession of results agreeing more with each other, and upon which, therefore, more entire reliance can be placed than upon those hitherto published.*

EDINBURGH, 14*th April* 1829.

* The experiments above detailed were performed in a small tube apparatus, the cubical contents of which were from .3 to .5 of an inch. In separating the gases, the carbonic acid was absorbed by caustic potash; the oxygen in the unabsorbed portion estimated by deutoxide of azote, after the method of Gay-Lussac, and the residue, allowing for the common air, was considered to be azote.

ART. XIII.—*A Letter to the Right Hon. T. P. COURTENAY, on the proportionate number of Births of the two Sexes under different circumstances.* By CHARLES BABBAGE, Esq. M. A. F. R. S. Lond. and Edin. Lucasian Professor of Mathematics in the University of Cambridge, &c. &c. Communicated by the Author.

DEAR SIR,

THE great interest you have taken in promoting all inquiries which might contribute to the security and improvement of the numerous Friendly Societies established in this country, induces me to address to you a few observations on some facts I collected in a recent tour. They do not pretend to that prominent importance which may justly be claimed for those inquiries of which you have had the direction, because they do not so immediately apply themselves to the comfort and happiness of a large portion of our population. They are, however, from the singular conclusion to which they appear to lead, highly calculated to promote inquiry, and consequently to elicit additional information; and some of them may perhaps be valuable, as connected with a kindred subject which occupies a considerable portion of public attention in this country, and whose benefits are now spreading on the continent. The system of life assurance, so widely extended in England, and so strongly indicative of the prudence and foresight of the people, is not yet in my opinion carried to those limits which it might reach, if those who deal in that species of security were perfectly satisfied with the accuracy of the tables they employ, and if the public were informed in a plain and popular treatise of the many ways yet *unnoticed*, in which it might be desirable to have recourse to it.

Facts and accurate enumerations are the great and only bases on which such transactions can securely rest, and, in this point of view, I cannot but congratulate the public on a most invaluable collection recently prepared by the command of the Lords of His Majesty's Treasury under the superintendence of Mr Finlaison. The circumstances under which the lives enumerated were placed, and the number of individuals whose period of existence has been precisely traced, give to this collection a great importance; and I am confident you will join with me in urging those who are at the head of our great establishments for granting assurances, to increase this collection of facts, by giving to the public the results of their own experience, which now becomes additionally interesting from the comparisons we should be enabled to make. I am aware that the time and expence of such an inquiry at the Equitable Society might be considerable, nor do I doubt but that the government, which has already shown the great importance it attaches to such information, would, if applied to, lend its assistance. I confess, however, as a member of that society I should regret exceedingly that the fear of expence should induce us to owe this advantage to any thing but our own exertions. We have succeeded and grown wealthy by availing ourselves of the experience which we owe to the industry of those who preceded us. They have now passed away; and although to them all expression of our gratitude is vain, we owe it to our own character, not merely to transmit unimpaired to our successors the light which conducted us to prosperity, but to collect and cherish every additional ray our experience has furnished, which may add to its permanence or utility.

There is one point, however, on which I would not be misunderstood. I should be sorry that any remarks of mine should throw an additional labour on our venerable actuary. His merits and his unwearied care of our interests stand recorded in the present state of our society too forcibly to be increased by any expression of mine; and, I am convinced, every member of that society would wish to lighten rather than to augment his labours. Mr Morgan has honoured my little book with his remarks, and, while I admit the justice of a small part of his criticisms, I cannot but regret that he should have so completely misconceived my meaning, as to have employed any portion of his valuable time in refuting what I have neither thought nor written. That the work alluded to should have induced him to publish his sentiments, is an advantage to the public; at the same time that it is gratifying to me to find that the opinions expressed by a mere amateur are in *reality* so accordant with those of one who has had the most extensive experience. I trust, therefore, that Mr Morgan will consider that it is from no want of respect that I neglect discussing the arguments which he has advanced, but that I do it from the belief that it is of more interest as well as more useful to the public, to endeavour to collect additional facts, which may furnish a securer basis for future reasonings.

You are aware of the singular fact stated in the *Annuaire*, * relative to the population of France, that the preponderance of male above female births was less amongst the *illegitimate* than amongst the *legitimate* population. That is, for every ten thousand *legitimate* females, 10,657 males were born; whilst for the same number of illegitimate females only 10,484 males were produced. This fact, resting on a very extensive enumeration, comprehending above half a million of illegitimate births, deserved considerable confidence, and it appeared to me desirable to examine whether in other countries differently circumstanced the same fact existed.

It is to be regretted that enumerations sufficiently precise are not made in all countries, and that it does not always happen that, when made, the results are published, or are even accessible to the public. In the kingdom of Naples, a volume of statistical details was published for the year 1824, containing a great variety of most valuable matter, and it is much to be regretted that such a work should not be continued annually. For my information relative to that country, I am indebted partly to the work alluded to and partly to the assistance of a most zealous and learned gentleman who has devoted a large portion of his time to these subjects.

In the kingdom of Naples, (excluding Sicily,) the number of births are stated in Table I. and it appears that in five years the births were as follows :—

	Legitimate. Reduced to 10,000 females.		*Illegitimate.* Reduced to 10,000 females.	
	Females.	Males.	Females.	Males.
1819,	10,000	10,433	10,000	10,752
1820,	10,000	10,579	10,000	10,131
1821,	10,000	10,341	10,000	10,197
1822,	10,000	10,451	10,000	10,343
1824,	10,000	10,450	10,000	10,407
Average deduced from all the births,	10,000	10,452	10,000	10,367

* *Annuaire presénté au Roi par le Bureau des Longitudes.*

From this table it appears, that in one year the excess of male births was greater amongst the illegitimate; but, in the other four, the preponderance was on the side of the legitimate. Where the numbers are small, greater deviations from the mean are to be expected, but the average deduced from above 50,000 illegitimate births, shows that the same fact exists in the kingdom of Naples.

In Prussia an accurate knowledge of the state of the population is considered an object of importance, and a particular department of the government is charged with the superintendence of this subject. The president of the board of statistics is M. Hoffman, a gentleman to whose obliging attention I am indebted for several of the very valuable tables which are subjoined to this letter. He will perceive, that in the few remarks I have offered on those tables, I have done little more than avail myself of some of the observations which he suggested to me.

Germany is now beginning to adopt the system of assurance on life, which has been found so advantageous in our own country; and I am confident she would gladly express her gratitude for the knowledge she has borrowed by affording us any information she possesses, which might be adapted to the more advanced state of our institutions. I mention this, with the hope of inducing the distinguished gentleman to whom I have referred, to direct a portion of his attention to the number of children resulting from marriages of the various classes which are given in these tables, and also with the hope that the reflections he has made on the immense variety and number of facts which come before him may be rendered more valuable to his country, as well as to my own, by being embodied either in the transactions of some society, or in some separate work.

The following table shows the proportion of births amongst the legitimate and illegitimate population of Prussia during a period of eight years:—

	Legitimate. Reduced to 10,000 females.		*Illegitimate* Reduced to 10,000 females.	
	Females.	Males.	Females.	Males.
1816,	10,000	10,586	10,000	10,236
1817,	10,000	10,544	10,000	10,294
1818,	10,000	10,621	10,000	10,228
1819,	10,000	10,611	10,000	10,263
1820,	10,000	10,619	10,000	10,281
1821,	10,000	10,648	10,000	10,313
1822,	10,000	10,611	10,000	10,129
1823,	10,000	10,624	10,000	10,482
Average of 8 years,	10,000	10,609	10,000	10,278

At Cassel I had the pleasure of becoming acquainted with M. Hassel, Chef de Division et Directeur de Bureau Statistique dans la Ministére de l'Interieure, to whose kindness I am indebted for the table which relates to the former kingdom of Westphalia. The following table has been computed from it:—

	Legitimate. Prop. to 10,000 females.		*Illegitimate.* Prop. to 10,000 females.	
	Females.	Males.	Females.	Males.
1809,			10,000	10,190
1810,	10,000	10,331	10,000	10,147
1811,	10,000	10,591	10,000	9,909
Average,	10,000	10,471	10,000	10,089

The same fact, therefore, reappears much more strongly marked in those provinces which comprised the kingdom of Westphalia. The total number of illegitimate births on which it rests, rather less than 20,000, might be objected to for the first establishment of this singular fact, but it must be admitted as a strong corroboration of other and larger enumerations.

The number of children born at Montpellier during twenty years, from 1772 to 1792, was

Legitimate.		*Illegitimate.*	
Males.	Females.	Males.	Females.
12,919	12,145	1373	1362

or, reduced to 10,000 females,

Legitimate.		*Illegitimate.*	
Females.	Males.	Females.	Males.
10,000	10,707	10,000	10,081

The result of these several enumerations will be better seen in the following table :—

	Legitimate. Females.	Males.	Number counted.	*Illegitimate.* Females.	Males.	Number counted.
France,	10,000	10,657	9,656,135	10,000	10,484	673,047
Naples,	10,000	10,452	1,059,055	10,000	10,367	51,309
Prussia,	10,000	10,609	3,572,251	10,000	10,278	272,804
Westphalia,	10,000	10,471	151,169	10,000	10,039	19,950
Montpellier,	10,000	10,707	25 064	10,000	10,081	2,735
Total counted,			14,463,874			1,019,845

It appears, then, from an enumeration of above one million illegitimate births, and fourteen million legitimate, that *the excess of males above females is less amongst illegitimate than amongst legitimate children.*

The climate of Naples is totally different from that of France. The sands of Brandenburg and part of Prussia, as well as the marshes of Westphalia, are different from either; we cannot, therefore, ascribe the fact to climate. It has been contended that the enumeration itself is incorrect, because it includes amongst the illegitimate all those who have been received into foundling hospitals ; and it is said that there is a greater tendency to expose female children than males, because the latter are able to gain enough for their subsistence at an earlier period. This circumstance does not appear to me to be sufficient to account for the difference which occurs in so many countries ; but in order to estimate its weight, we must have further information as to its extent. Laplace has stated in the introduction to his *Theorie Analytique des Probabilités*, (p.xlvii.) that from 1745 to 1809, there were admitted into the Foundling Hospital of Paris 163,499 boys, and 159,405 girls, the ratio of which is nearly that of 24 to 25, whereas the proportion of the sexes in the rest of the population is 22 to 21 ; and he finds that there are 238 to 1 in favour of some cause producing this difference in the ratio. I have annexed amongst the tables one of the admissions to the Foundling Hospital in Dublin during twenty-seven years, and the difference in the proportion of the sexes still more strongly indicates a cause.

The number of males which are still born in Westphalia is remarkable ; for every 10,000 females there are no less than 13,689 males.

The comparative number of illegitimate children in different countries stands thus: for every thousand legitimate there are

	Illegitimate.
In France,	69.7
Naples,	48.4
Prussia,	76.4
Westphalia,	88.1
Towns of Westphalia,	217.4
Montpellier,	91.6

I shall notice one other circumstance connected with this subject. It is the remarkable excess of males amongst the children of the Jews in Prussia. For every ten thousand females born amongst them there are 11,292 males.

It would be interesting to examine this fact amongst the Jews in other countries, and still more so, could we procure any correct enumeration of births in any country in which the Mahometan religion prevails.

I cannot conclude this subject, without recalling to your notice a statement, in the *History of the Academy of Sciences of Paris* for the year 1827. It is stated as the result of some experiments lately tried, that in a flock of sheep consisting of 71 females and 61 males, by selecting strong females and young males, and by feeding the females high and not the males, the result was amongst the births

	Males.	Females.
	53	84
by the reverse process	80	50

Another singular fact which appears from the annexed tables is the greater fertility of the Jews in Prussia than of the Christian inhabitants. Each marriage amongst the latter produces 4.78 children, whilst each marriage amongst the Jews produces 5.35 children, on an average.

I have now stated some of the conclusions to which the subjoined tables lead. Many others of great interest remain; but I hope I have stated enough to excite the curiosity of those who may have leisure and opportunity, and to induce them to add to our knowledge by the publication of similar enumerations.

I remain, my dear Sir, your faithful servant,

C. Babbage.

Dorset Street, Manchester Square,
May 7th, 1829.

TABLE I.] In 1,000,000 inhabitants, there are in the Prussian States,

In the years	BIRTHS. Legitimate.	BIRTHS. Illegitimate.	BIRTHS. Total.	DEATHS. In the months of January, February, March,	DEATHS. April, May, June,	DEATHS. July, Aug. Sept.	DEATHS. Oct. Novem. Decem.	DEATHS. Total.	Excess of Births.	MARRIAGES. Marriages from which children may be expected. Marriages at the most proper ages for producing children.	MARRIAGES. Marriages from which children may be expected. Marriages later.	MARRIAGES. Marriages not for children, but for convenience in more advanced age.	MARRIAGES. Total of Marriages.
1816,	39923	3202	43125	8118	7135	5548	6775	27576	15549	8087	2505	586	11178
1817,	39782	3176	42958	8014	7405	6103	7489	29011	13947	7748	2337	525	10610
1818,	39963	2863	42826	8424	7302	5998	7255	28983	13843	7473	2276	524	10273
1819,	41729	3103	44832	8790	7177	6666	7795	30428	14404	7353	2251	495	10104
1820,	39965	3010	42975	8641	6314	5088	6300	26343	16632	7167	2126	457	9750
1821,	40782	3095	43877	7653	6010	5203	6160	25026	18851	6857	1931	435	9223
1822,	40009	3112	43121	7558	6507	6254	6647	26966	16155	6888	1827	386	9101
1823,	39122	2983	42105	8836	6863	5220	6007	26926	15179	6510	1756	366	8632
Total,	321275	24544	345819	66038	54713	46080	54428	221259	124560	58088	17009	3774	78871
Mean from the above calculation.	40159	3068	43227	8255	6839	5760	6808	27657	15570	7261	2126	472	9859
	40155	3066	43221	8255	6825	5753	6785	26617	15604	7240	2115	468	9823

Marriages in the Prussian States.

In the years	Marriages in which the man had not exceeded his 45th year. The woman had not completed the 30th year,	passed the 30th, but not the 45th.	exceeded the 45th year.	Marriages in which the husband had exceeded the 45th, but not completed the 60th. Woman under 30.	between 30 and 45.	exceeded 45.	Marriage in which the husband exceeded 60. Woman under 30.	between 30 and 45.	above 45.	Number of Marriages. Favourable for children.	Late marriages.	Marriages of convenience.	Total number of marriages.
1816,	83688	19595	1976	2667	3667	1929	421	744	991	83688	25929	6061	115678
1817,	81919	17610	1723	3291	3807	1677	432	796	918	81919	24708	5546	112173
1818,	80682	16990	1749	3580	4005	1686	506	798	923	80682	24575	5662	110919
1819,	80801	17410	1615	3357	3952	1777	401	795	846	80801	24719	543	110954
1820,	80801	16812	1498	3362	3790	1687	348	708	911	80801	23964	5152	109917
1821,	78724	15761	1427	2844	3564	1687	337	719	824	78724	22169	4994	105887
1822,	80336	15462	1396	2520	3327	1488	288	628	696	80336	21309	4496	106141
1823,	77103	15070	1346	2464	3262	1401	302	554	729	77103	20796	4332	102231
Sum,	644054	134710	12730	24085	29374	13332	3035	5742	6838	644054	188169	41677	873900
Mean,	80507	16839	1591	3011	3672	1666	379	718	855	80507	23521	5210	109238

TABLE III.] View of the Births in the Prussian States.

Calendar year, 1st January.	Number of Inhabitants.	Number of marriages.	Number of Births. Legitimate. Males.	Legitimate. Females.	Legitimate. Total.	Illegitimate. Males.	Illegitimate. Females.	Illegitimate. Total.	Total. Males.	Total. Females.	Total.
1816,	10349031	115678	212462	200704	413166	16759	16373	33132	229221	217077	446298
1817,	10572843	112173	215877	204729	420606	17036	16549	33585	232913	221278	454191
1818,	10796874	110919	222234	209239	431473	15632	15284	30916	237866	224523	462389
1819,	10981934	110954	235921	222340	458261	17262	16819	34081	253183	239159	492342
1820,	11274482	109917	232056	218525	450581	17206	16736	33942	249262	235261	484523
1821,	11480815	105887	241453	226756	468209	18041	17494	35535	259494	244250	503744
1822,	11663177	106141	240242	226395	466637	18260	18028	36288	258502	244423	502925
1823,	11842942	102231	238669	224649	463318	18078	17247	33325	256747	241896	498643
Total,	88962098	873900	1838914	1733337	3572251	138274	134530	272804	1977188	1867867	3845055
Mean for one year,	11120262	109237	229864	216667	446531	17284	16816	34100	247148	233483	480631
There were therefore	10000000	98234	206708	194840	401548	15543	15121	30664	222251	209961	432212

TABLE IV.]

View of the Deaths in the Prussian States.

Years.	Number of Inhabitants.	Number of Births.	Deaths.	Deaths in the months of January, February, March.	April, May, June.	July, Aug. Sept.	October, November, December.	Excess of Births over Deaths.
1816,	10349031	446298	285382	84012	73840	57413	70117	160916
1817,	10572843	454191	306728	84726	78292	64529	79181	147463
1818,	10796874	462389	312923	91001	78837	64755	78330	149466
1819,	10981934	492342	334156	96531	78810	73215	85600	158186
1820,	11274482	484523	297004	97416	71190	57366	71032	187519
1821,	11480815	503744	287318	87865	68996	59731	70726	216426
1822,	11663177	502925	314513	88148	75897	72946	77522	188412
1823,	11842942	498643	318878	104649	81274	61816	71139	179765
Sum,	88962098	3845055	2456902	734348	607136	511771	603647	1388153
Mean,	11120262	480632	307113	91794	75892	63971	5456	173519
For each,	10000000	432213	276174	82546	68247	57527	67854	156039

TABLE V.] Proportional number of Marriages in the Prussian States, according to the ages of the parties married.

In 100,000 Marriages there were from which

Years. 1st Jan.	Children might be expected, because the Husband did not exceed 60, and the Wife 45. Marriages at favourable ages. Husband not exceeding 45. Wife not exceeding 30.	LATE MARRIAGES. On the side of the Husband. Husband who exceeded 45.	LATE MARRIAGES. On the side of the Husband. Wife who exceeded 30.	LATE MARRIAGES. On both sides. The Husband exceeding 45, and the Wife exceeding 30.	Total.	Total of all marriages from which children may be expected.	Children may not reasonably be expected, because either the Husband or the Wife, or both, are too old. Because only one is too old. The Husband exceeded his 60th year. The Wife not completed her 30th year.	The Husband exceeded his 60th year. The Wife having completed the 30th, but not exceeded her 45th.	Total of both cases.	Because the Wife has exceeded her 45th year. The Husband has not completed his 45th year.	The Husband has completed his 45th, but not exceeded the 60th year.	Total of both cases.	The Husband exceeded his 60th year, and the Wife her 45th year.	Total of all Marriages from which the Children are not to be expected.	Total of all Marriages.
1816,	72346	2305	16939	3170	22414	94760	364	643	1007	1708	1668	3376	857	5240	115678
1817,	73029	2933	15699	3395	22027	95056	385	710	1095	1535	1495	3030	819	4944	112173
1818,	72740	3228	15317	3611	22156	94896	455	720	1175	1577	1519	3096	833	5104	110919
1819,	72825	3026	15691	3561	22278	95103	362	716	1078	1456	1601	3057	762	4897	110954
Mean of 4	72731	2867	15923	3431	22221	94952	391	697	1088	1570	1572	3142	818	5048	112431
1820,	73511	3060	15295	3448	21803	95314	316	644	960	1363	1534	2897	829	4686	109917
1821,	74347	2686	14885	3365	20936	95283	318	680	998	1347	1593	2940	779	4717	105887
1822,	75687	2375	14567	3135	20077	95764	272	591	863	1316	1401	2717	656	4236	106141
1823,	75420	2410	14741	3191	20342	95762	295	542	837	1317	1370	2687	714	4238	102231
Mean of 4.	74725	2638	14877	3287	20802	95527	301	615	916	1336	1476	2812	745	4473	106044

Years.	Marriages favourable in which the Husband had not completed 45, and his Wife had not completed the 30th year.	On the side of the Husband who exceeded the 45th year.	Wife who exceeded the 30th year.	On both sides. Husband exceeded 45th year, Wife exceeded 30th year.	Total of late marriages.	Total of all the marriages from which children might be expected.	The Wife had not yet completed her 30th year.	Exceeded the 30th, but not completed 45.	Total of both cases.	But the Husband had not completed his 45th.	The Husband had passed his 45th, but not completed his 60th year.	Total of both cases.	Because the Husband had completed his 60th, and the Wife had passed the 45th year.	Total of all the marriages from which children are not to be expected.	Total of *all* the marriages.
1816,	83688	2667	19595	3667	25929	109617	421	744	1165	1976	1929	3905	991	6061	115678
1817,	81919	3291	17610	3807	24708	106627	432	796	1228	1723	1677	3400	918	5546	112173
1818,	80682	3580	16990	4005	24575	105257	506	798	1304	1749	1686	3435	923	5662	110919
1819,	80801	3357	17410	3952	24719	105520	401	795	1196	1615	1777	3392	846	5434	110954
	327090	12895	71605	15431	99931	427021	1760	3133	4893	7063	7069	14132	3678	22703	449724
1820,	80801	3362	16812	3790	23964	104765	348	708	1056	1498	1687	3185	911	5152	109917
1820,	78724	2844	15761	3564	22169	100893	337	719	1056	1427	1687	3114	824	4994	105887
1822,	80336	2520	15462	3327	21309	101645	288	628	916	1396	1488	2884	696	4496	106141
1823.	77103	2464	15070	3262	20796	97899	302	554	856	1346	1401	2747	729	4332	102231
Total of 4 years.	316964	11190	63105	13943	88238	405202	1275	2609	3884	5667	6263	11930	3160	18974	424176
Total of 8 years.	644054	24085	134710	29374	188169	832223	3035	5742	8777	12730	13332	26062	6838	41677	873900

TABLE VII.] In the Prussian States.

Years beginning 1st Jan.	Legitimate Births.			Number of those (legitimate) who died in the first year, including the still-born.			Number of those surviving the first year.			Proportion for 100,000 of those born legitimate.		
	Males.	Females.	Total.	Males.	Females.	Total.	Males.	Females.	Total.	Males.	Females.	Total.
1816,	212462	200704	413166	44339	34969	79308	168123	165735	333858			
1817,	215877	204729	420606	45010	35787	80797	170867	168942	339809			
1818,	222234	209239	431473	43564	34532	78096	178670	174707	353377			
1819,	235921	222340	458261	49426	39308	88734	186495	183032	369527			
Total of 4 years.	886494	837012	1723506	182339	144596	326935	704155	692416	1396571			18969
1820,	232056	218525	450581	45563	35541	81104	186493	182984	369477			
1821,	241453	226756	468209	46470	36299	82769	194983	190457	385440			
1822,	240242	226395	466637	50738	40336	91074	189504	186059	375563			
1833,	238669	224649	463318	49113	39103	88216	189556	185546	375162			
Total of 4 years.	952420	896325	1848745	191884	151279	343163	760536	745046	1505582			18562
Total of 8 years.	1838914	1733337	3572251	374223	295875	670098	1464691	1437462	2902153	20350	17070	18758

TABLE VIII.] In the Prussian States.

years beginning 1st Jan.	Total number of births.			Number of those dying in the first year, including the still-born.			Number of those surviving the first year.			In 100,000 there died in the first year.		
	Males.	Females.	Total.	Males.	Females.	Total.	Males.	Females.	Total.	Males.	Females.	Total.
1816,	229221	217077	446298	49465	39479	88944	179756	177598	357354	21580	18186	19929
1817,	232913	221278	454191	50220	40484	90704	182693	180794	363487	21562	18296	19970
1818,	237866	224523	462389	48116	38666	86782	189750	185857	375607	20228	17222	18768
1819,	253183	239159	492342	54500	43850	98350	198683	195309	393992	21526	18335	19976
	953183	902037	1855220	202301	162479	364780	750882	739558	1490440	21224	18012	19662
1820,	249262	235261	484523	50215	39688	89903	199047	195573	394620	20146	16870	18555
1821,	259494	244250	503744	51488	40736	92224	208006	203514	411520	19842	16678	18308
1822,	258502	244423	502925	55997	48104	101101	202505	199319	401824	21662	18453	20103
1823,	256747	241896	498643	54155	43441	97596	202592	198455	401047	21093	17958	19572
	1024005	965830	1989835	211855	168969	380824	812150	796861	1609011	20689	17495	19138
	1977188	1867867	3845055	414156	331448	745604	1563032	1536419	3099451	20947	17745	19391

TABLE IX.]

In the Prussian States.

Year beginning 1st Jan.	Legitimate births. Both sexes.	of which Males.	of which Females.	In 100,000 children there were Males.	In 100,000 children there were Females.	Illegitimate births. Both sexes.	of which Males.	of which Females.	In 100,000. Males.	In 100,000. Females.	Total number of births. Both sexes.	of which Males.	of which Females.	In 100,000. Males.	In 100,000. Females.
1816,	413166	212462	200704	51423	48577	33132	16759	16373	50582	49418	446298	229221	217077	51360	48640
1817,	420606	215877	204729	51325	48675	33585	17036	16549	50725	49275	454191	232913	221278	51281	48719
1818,	431473	222234	209239	51506	48494	30916	15632	15284	50563	49437	462389	237866	224523	51443	48557
1819,	458261	235921	222340	51482	48518	34081	17262	16819	50650	49350	492342	253183	239159	51424	48576
Total of 4 years.	1723506	886494	837012	51735	48565	131714	66689	65025	50632	49368	1855220	953183	902037	51378	48622
1820,	450581	232056	218525	51501	48499	33942	17206	16736	50692	49308	484523	249262	235261	51454	48555
1821,	468209	241453	226756	51569	48431	35535	18041	17494	50770	49230	503744	259494	244250	51813	48487
1822,	466637	240242	226395	51483	48516	36288	18260	18028	50320	49680	502925	258502	244423	51400	48600
1823,	463318	238669	224649	51513	48487	35325	18078	17247	51177	48823	498643	256747	241896	51489	48511
Total of 4 years.	1848745	952420	896325	51517	48483	141090	71585	69505	50737	49263	1989835	1024005	965830	51462	48538
Total of 8 years.	3572251	1838914	1733337	51478	48522	272804	138274	135530	50686	49314	3845055	77188	1867867	51422	48578

TABLE X.]

In the Prussian States.

	Illegitimate Births.			Deaths in the 1st year, including the still-born, (all illegitimate.)			Survivors of the first year. Illegitimate.			Out of 100,000.		
	Males.	Females.	Total.	Males.	Females.	Total.	Males.	Females.	Total.			
1816,	16759	16373	33132	5126	4510	9636	11633	11863	23496			
1817,	17036	16549	33585	5210	4697	9907	11826	11852	23678			
1818,	15632	15284	30916	4552	4134	8686	11080	11150	22230			
1819,	17262	16819	34081	5074	4542	9616	12188	12277	24465			
	66689	65025	131714	19962	17883	37845	46727	47142	93869			
1820,	17206	16736	33942	4652	4147	8799	12554	12589	25143			
1821,	18041	17494	35535	5018	4437	9455	13023	13057	26080			
1822,	18260	18028	36288	5259	4768	10027	13001	13260	26261			
1823,	18078	17247	35325	5042	4338	9380	13036	12909	25945			
	71585	69505	141090	19971	17690	37661	51614	51815	103429			
	138274	134530	272804	39933	35573	75506	98341	98957	197298	28880	26442	27678

Table XI.

Christians during the years 1820, 1821, 1822, 1823, 1824.

	MARRIAGES.	BIRTHS. Males.	BIRTHS. Females.	BIRTHS. Total.	DEATHS. Males.	DEATHS. Females.	DEATHS. Total.
Prussian Poland, Posen,	41351	112766	106737	219503	63726	60069	123795
Marienwerder,	20824	55267	51961	107228	30514	28498	59012
Upper Silesia, Oeppeln,	33061	83996	79833	163829	52465	49978	102443
Breslaw & Lignitz, Lower Silesia,	71098	171561	161219	332780	115416	110616	226032
Brandenburg, Pomerania,	99830	226740	212997	439737	131335	122739	254074
Provinces of the Rhine,	75922	193564	181833	374397	123941	120037	243978
Total,	342086	842894	794580	1637474	517397	491937	1009334

Jews during the same years.

	MARRIAGES.	BIRTHS. Males.	BIRTHS. Females.	BIRTHS. Total.	DEATHS. Males.	DEATHS. Females.	DEATHS. Total.
Prussian Poland, Posen,	2193	6516	5730	12246	3528	3204	6732
Marienwerder,	356	1118	975	2093	501	437	938
Upper Silesia, Oeppeln,	379	1127	959	2086	482	404	886
Breslaw & Lignitz, Lower Silesia,	354	769	763	1532	502	493	995
Brandenburg, Pomerania,	345	1202	1053	2255	701	529	1230
Provinces of the Rhine,	562	1722	1549	3271	1030	863	1893
Total,	4389	12454	11029	23483	6744	5930	12674

Table XII.

Kingdom of Naples.

Births.

	Legitimate. Males.	*Legitimate.* Females.	*Illegitimate.* Males.	*Illegitimate.* Females.
1819,	110,341	105,763	5606	5213
1820,	108,607	102,660	5323	5254
1821,	98,537	95,288	5068	4970
1822,	109,128	104,418	5063	4895
1824,	114,625	109,688	5058	4860
Total,	541,238	517,817	26,117	25,192

.E XIII.—The kingdom of Westphalia, comprising the ›artments of Aller, Elbe, Fulda, Hartz, Leine, Oker, ıle, and Werra.

Births. Males.	Births. Fem.	Total.	Still-born. Males.	Still-born. Fem.	Deaths. Males.	Deaths. Fem.	Tot.	Died above 100
		Year ending 1st Jan. 1811.						
·7,023	35,819	72,842	1508	1108	27,887	27,934	55,821	10
		Year ending 1st Jan. 1812.						
·0,404	38,123	78,327	1790	1263	29,379	29,175	58,554	6

	Illegitimate.	*Still-born.* Males.	*Still-born.* Females.
1810,	2876 Males 2845 Females.	1300	988
1811,	3039 Males. 2995 Females	1508	1108
1812,	4029 Males. 4066 Females.	1790	1263
		4598	3359

?.—The greater part of the still-born occur in the marshy provinces, the moist parts of the department of the Elbe.

In 1811.	*Legitimate.*	*Illegitimate.*
Göttingen,	355	132
Hanover,	649	207
Celle,	319	96
Magdeburg,	1135	279
Brunswick,	1123	256
Halle,	658	161
Quedlinburg,	369	55
Wolfenbuttel,	219	34
Hilderheim,	412	62
Halberstadt,	554	90
Cassel,	1022	110
	4598	3359

TABLE XIV.—*Foundling Hospital Dublin.*

ımber of infants received into the Foundling Hospital : the age of twelve months, from 1st January 1800, to December 1826, a period of 27 years.

	Males.	Females.	Total.
›ars ending Dec. 31, 1814,	15,586	17,655	33,241
1822,	5,788	6,506	12,294
1826,	913	1,008	1,921
Totals	22,287	25,169	47,456

During the first period the admissions were unlimited.

During the second partially limited.

During the last a particular certificate is required from the minister and churchwardens, stating that the child was deserted and in danger of perishing.

J. FINLAY.

FOUNDLING HOSPITAL,
September 26, 1827.

ART. XIV.—*Notice respecting a Method of producing an intense heat from Gas for various purposes in the Arts.** By DAVID BREWSTER, LL.D., F.R.S.L. and E.

HAVING been occupied for many years in the examination of flames of different colours, with the view of obtaining one perfectly homogeneous for microscopical purposes, I succeeded in constructing a monochromatic lamp, by which a yellow flame of considerable intensity was produced. In this lamp, which was submitted to the Royal Society of Edinburgh in April 1822, I connected with the top of the burner a frame of wire gauze, which, by moving vertically round a hinge, or by a motion to one side, could be placed in a horizontal position above the sponge wick. As soon as it had become red hot, it was made to descend into contact with the sponge, when it converted the alcohol rapidly into vapour, and produced an abundant discharge of yellow light.

In the beginning of this winter when I lighted my house with oil gas, I was desirous of examining the modifications which the flame of the gas experienced when burned through wire gauze. For this purpose, I took the wire gauze frame of the monochromatic lamp, and having fixed it about two inches above a single jet burner, I found that it burned with much agitation, but with almost no light. The inner flame was of a bluish green colour, and the outer one of a pale blue, slightly tinged with red, which always became of a most beautiful homogeneous yellow when any body placed in it became

* Read before the Society of Arts, February 21st 1826.

sufficiently heated. In examining the nature of the flame thus produced, I found that it differed in the most essential manner from all other flames. The flame of a candle, of gas, and of spirit of wine is entirely a superficial film of a conical form, there being no oxygen in the interior of the cone to promote combustion, as has been proved by Mr Sym and Mr Davies. Hence these hollow flames produce comparatively very little heat. The flame, on the contrary, produced above the wire gauze is a solid mass, similar to that of the oxihydrogen blowpipe, and therefore it produces a very intense heat. The term flame, indeed, cannot with any propriety be applied to it. It is in reality a succession of explosions of the explosive mixture formed by the gas and atmospheric air. Having thus ascertained the cause of the intense heat, generated by this method of using gas, I conceived that it might be advantageously applied to various purposes in the arts, particularly as no smoke is generated during the combustion. At this period of my inquiry, I learned that Dr Andrew Duncan Junior had, a considerable time ago, been in the habit of burning gas above wire gauze in his pharmaceutical experiments, chiefly with the view of diffusing the heat over a greater surface; and I have since found, that an ingenious American gentleman, Mr Samuel Morey, made numerous experiments for obtaining heat by burning the vapours of different substances through wire gauze. The following interesting experiment deserves to be quoted.* "If the vapour of spirits of turpentine be made to pass through a tube covered at the upper end with fine wire gauze, it burns with much smoke: If a quantity of atmospheric air be allowed to mix with it, the smoke arises, but the flame continues white. If more still be added, the flame lessens and becomes partly blue. By adding still more and more it will burn with a very small flame, entirely blue, and with a singular musical sound. If still more be added, the flame and every ray of light ceases, but that the combustion still continues is certain, from the explosive detonating noise or report continuing to be distinctly heard."

The object which I had principally in view was to produce

* Letter to Professor Silliman, May 4, 1819.

an intense local heat, and in procuring it I have been led to new methods, which I trust will be found of some value in practical science.

As a very great quantity of heat is carried off by the wire gauze, I endeavoured to produce an explosive mixture of oil gas and atmospheric air above the burner, without the interposition of any substance whatever. After many fruitless attempts, I succeeded in effecting this, but any slight agitation of the air either blew out the exploding gases, or converted them into a regular flame. This defect was too serious to be overlooked, and it required some consideration to remedy it. It occurred to me that if a small flame were permanently kept up about two inches above the burner, it would maintain a constant explosion of the mixed gases, independent of any accidental agitation of the air, and upon making the experiment, I found it to exceed my highest expectations. In constructing the burner permanently for use, a small gas tube *a b c*, (Plate II. Fig. 3.) arising from the main tube M N of the gas lamp, should terminate above the burner, and have a short tube *d e* moveable up and down within it so as to be gas light. This tube *d e*, closed *A e*, communicate with the hollow ring *f g*, on the inside of which four apertures are perforated in such a manner as to throw their jets of gas to the apex of a cone, of which *f g* is the base. When we allow the gas to flow from the burner M, by opening the main cock A, the gas will flow at the tube *a b c d*, and issue at the four holes in the ring *f g* in very small flames. The size of these flames is regulated by the cock *b*. If the ignited gas were now to issue at M its inflammation will be sustained by the four little subsidiary flames, independent of any agitation of the air.

Besides the great loss of heat occasioned by the wire gauze, it has another defect equally injurious. A great quantity of soot collects on its lower side, and this soot is carried up into the flame, by the gas rushing through the meshes. The purity of the flame, which in many experiments is essential, is thus contaminated, and a portion of the heat lost by the ignition of the particles. This defect is entirely removed in the method which has been described.

Having succeeded to this extent with gas issuing under its dinary pressure, I expected to produce still higher heats with compressed gas. A limit, however, appeared to be set to such experiment, by the velocity of the issuing gas, which, at certain point, never fails to blow out the flame; but I have found that when the gas is discharged with a velocity much greater than what will extinguish the flame, the explosion may be kept up to any extent by the small subsidiary burner, which have already described. This result removed every difficulty which seemed to be opposed to the generation of high heats by exploding the gases; and I have no hesitation in saying, that the portable gas lamp will become one of the most valuable instruments that has ever been presented to the arts. When either the philosopher or the artisan requries a powerful heat for any specific purpose, he must kindle a fire or light a furnace, and thus obtain what he wants, with much trouble and expence; but by the present method of fitting up a portable gas lamp, he can obtain the heat of a furnace in a second, and extinguish it as speedily.*

For various purposes in domestic economy such a lamp will be equally useful, particularly in summer, and under circumstances where heat could not be conveniently obtained in the ordinary way.

The preceding observations furnish us with what I conceive is the true theory of the common blowpipe. The intense heat generated by the blowpipe is ascribed by chemists to a concentration of the flame by the blast,—words which have no very definite meaning; but I apprehend that the heat in question is the heat generated by the repeated explosions of the explosive mixture formed by the unconsumed gas of the flame and atmospheric air.

I hope to be able to show some of the preceding experiments to the Society, through the kindness of Mr Gordon, secretary to the Portable Gas Light Company, who has kindly favoured me with a lamp for this purpose. †

* Iron wire was speedily melted by this lamp.

† The above lamp, fitted up with a subsidiary burner, was exhibited at the public lectures on chemistry delivered by Dr Hope and Dr Turner in 1826 and 1827.

Art. XV.—*Account of a New Monochromatic Lamp depe-ing on the combustion of compressed Gas.* * By Da Brewster, LL. D. F. R. S. L. and E.

On the 21st of February 1826, I exhibited to the Society Arts, the experiment of converting the exploding flame o portable gas lamp into a mass of homogeneous yellow light crossing it with a platinum wire or a film of mica; and occurred to me, that a permanent monochromatic flame mig be produced by causing a spiral of platinum wire to revo in the lower part of the flame by means of its impulsive fo As the spiral wire, however, required to have its surface su plied with a thin coating of a soapy or greasy fluid, and w besides liable to go out of order, I constructed a broad coll with coarse cotton wick, which could be placed either upon o above the ring *f g* of the subsidiary burner, shown in Plate II Fig. 3, and already described. This collar was soaked in saturated solution of common salt.

When the gas is allowed to escape at M, with such forc as to produce a long and broad column of an explosive mix ture of gas and atmospheric air, the bluish flame produced by the explosion is made to pass through the saturated collar, and is converted by it into a mass of homogeneous yellow light The collar will last a long time without any fresh supply o salt, so that the gas lamp will yield a permanent monochro-matic flame, during the longest series of optical experiments. The effect of this instrument is quite surprising. The inten-sity of the yellow light is very great, and may be readily in-creased for microscopical purposes by condensing it with mir-rors or lenses.

In place of a collar of cotton wick, a hollow cylinder of sponge with numerous projecting tufts may be substituted; or a collar may be similarly constructed with Asbestos cloth; and, if thought necessary, it might be supplied with a saline solution from a capillary fountain.

* Exhibited to the Society of Arts, May 1st, 1826.

ıT. XVI.—*On the Law of the Colours seen by transmission through Grooved Surfaces.* By M. Babinet.

ıe paper by M. Babinet, of which we propose to lay before r optical readers the most important part, was read to the ilamothic Society on the 8th December 1827, and has been lished in the *Ann. de Chimie* for February 1829. The nomena of which our author investigates the law, are e which have been so accurately measured and described Fraunhofer, and which were made by transmitting the light narrow aperture through systems of equidistant parallel s of very small diameter, or through systems of grooves le upon glass by a diamond point. As these phenomena already well known to the readers of this *Journal*, we shall ceed to M. Babinet's explanation of them.

To conceive this law, and to give an explanation of it, let suppose that MN, Plate II. Fig. 1, represents this system grooves of which LP, QA, KB, RN, are the full or opaque rts not permeable to light, and HL, PQ, AK, BR, the ansparent parts. The phenomena depend on the width of equal intervals HP, PA, AB, BN, composed of one opaque d one transparent part. Let us take one of these inter- ls AB, so situated that to the eye placed at C, the differ- nce of the lines BC and AC may be equal to the length of an tire undulation for one kind of light. The incident rays H, SA, SB, &c. being perpendicular to the plane of the ate MN, and radiating from a point sufficiently distant, d the lines AC and BC being sensibly parallel on account f the extreme smallness of AB, the arc AG described round as a centre (so that BG $=$ BC — AC $= \lambda$) may be considered s a perpendicular common to the lines AC and BC, and BG qual to λ will express the retardation of a ray which follows e line SBC, compared with a ray which follows the line SAC. et us suppose for a moment the interval AB to be quite open, nd through I, the middle of AB, let us draw SIC, the retar- ation of which will consequently be one-half that of BG, that s a semi-undulation in relation to the ray SAC. On this upposition, it is obvious that the ray which goes from A to C, The same figure shows the case of oblique reflexion, the rays proceeding from F and reaching the eye at C by reflexion. If from the mean of the common perpendiculars AD, BE, we take away equal parts in the paths of the rays FAC, FBC, the tint will depend on the difference between the semicircles AE, and DB, which may be easily replaced by the sines of the angles in the expression

$$AE - DB = m\lambda.$$

The difference which exists between the colours and systems of grooves and those produced by narrow apertures, though the same principles of interference apply for the two cases, will become more sensible if we observe in Fig. 1, that if AB is a narrow aperture, for which $BG = \lambda$ the eye will not receive from this aperture a single ray, while in the system of grooves, it is from this part which the eye receives most intensely this species of light. Hence we must not refer the colour of the interval AB of the grooves to the transparent part AK considered as a narrow aperture, since the tint which would arrive at C ought to vary with the width AK of the aperture, which it does not do.

If any additional proof was required that *light has no tendency to propagate itself according to any determinate direction*, it might be easily found in the great obliquity of the rays transmitted by grooves in relation to the plane of the system. If we receive on MN, Fig. 2, a solar ray FA, the eye being placed at C, we may make the rays FA so oblique to MN, and the direction AC so inclined on the other side to the same plane, that the rays FA almost parallel to MN will be obliged to take a direction nearly retrograde to arrive at C, which almost completely verifies the hypothesis of Huyghens.

ART. XVII.—*Theory of the colours observed in the experiments of Fraunhofer.* By THOMAS YOUNG, M. D. F. R. S.

As this *Journal* is the only English work in which a very full account of the discoveries of Fraunhofer have been published, we hasten to lay before our readers the following paper by Dr THOMAS YOUNG, which at the present moment derives a fresh interest as being the last production of that distinguished philosopher. At any period of our history the loss of such a man would have been deemed irreparable, but in the present declining state of the arts and sciences in England, it cannot but be regarded as a national calamity. In less than one year we have lost two of the greatest ornaments of English science since the days of Newton,—Dr Wollaston and Dr Young; and the grief which such events inspire, is embittered by the recollection that no national honour attended the triumphs of their genius, and with the fear that none will be paid over their tomb. That country must be indeed degenerate, where its rewards are conferred only on feats of animal courage, and where the flower of its intellectual chivalry is allowed to live and die unhonoured.

"The following note forms a very simple corollary to the law of interference, by which I succeeded, some weeks ago, after having read the excellent treatise by Mr Herschel on Light, in explaining the character of the perfect spectrum formed by diffraction in the fine experiments of the late M. Fraunhofer.

"It has been long ago observed, and Dr Brewster, if I am not mistaken, made the remark, in treating of the superficial colours of mother-of-pearl, that the images seen in this case of multiplied diffraction approached nearer the solar spectrum formed by refraction than the prevalent colour of ordinary diffraction, or those of the rings analyzed by Newton. But it is to M. Fraunhofer that we owe the most precise experiments on these colours.

"The following is the principle by which I propose to explain this phenomenon. If there is a series of parallel lines capable of furnishing by pairs the ordinary colours of diffraction, the union of a considerable number of these lines ought to have the effect of narrowing extremely the fringes formed by homogeneous light, so that after the brilliant central line there ought to be some total darkness; that is to say, in place of the second brilliant band of the narrowest fringes, which the two parallel lines the most remote would have formed; and this darkness will be followed only by paler bands of light, which will go on diminishing to half the distance of the second principal brilliant line of any single pair of adjacent lines.

is destroyed by its interference with the ray which goes from to C, and which differs from it half an undulation. Besides, the successive elementary rays which would have for their origi the different points from A to I, will be destroyed by the ray emanating from points similarly situated from I to B, and re spectively retarded half an undulation in relation to the ray emanating from points situated between A and I. The poi AB, therefore, will appear completely deprived of light. B if we now conceive the opaque part KB of the interval A and if we take IL equal to AK, the rays whose origin is b tween I and K will no longer be destroyed by those which a between I and L, and which would have differed from the by half an undulation, since these last rays are suppressed b the opacity of LB. The first rays will then subsist and wi convey to C a light, the more or less, as AK approaches i equality to AI or to the half of AB. But we must no increase beyond I the interval AK, or if, for example, AI were the transparent and LB the opaque part of the system In this case, indeed, a certain portion of the rays near A wi be destroyed by the rays whose origin would have been be tween I and L, and which the opaque part of the syste would not have suppressed. This particularity, which relate to the intensity of the light emanating from AB, has escape Fraunhofer, and deserves to be confirmed by precise experi ments.

As the tint for which the length of an undulation is λ ought to subsist in the part AB of the system in which we have $BG = \lambda$, it is easy to determine the angle HCA or rathe HCB, which the direct ray SC makes with the ray AC or BC which propagates this tint to the eye at C. The two right angled triangles HCB and BAG have the angle at B com mon, and consequently are similar. The ratio of HB to BC or the sine of the angles HCB, which we shall call δ, wil therefore be equal to the ratio of BG to BA, that is, to the ratio of λ to the quantity AB, which we shall call c. We shall then have $\sin. \delta = \frac{\lambda}{c}$

In like manner it may be shown that this same tint whose length of undulation is λ will still subsist for the intervals more remote from H than AB is, and for which BG will be equal to *twice*, *thrice*, four times the quantity λ. We shall then have the angle of deviation δ by the expression $\sin.\delta = \frac{m\lambda}{c}$, m being the whole number which marks the order of the spectrum.

By examining the simple relation which exists between the deviation of a ray and the length of an undulation λ, on which the tint depends, that the least refrangible rays for which λ is greatest will also be the most deviated: Thus in each spectrum the red will be exterior, and the violet nearest to the disc of image. We see also that the spectra nearest the direct image, for which δ is not too great, will be equidistant on account of the proportionality of the arc to its sine. All the other circumstances of the phenomenon are equally deducible from the formula which express its law. * * *

If we receive upon the system of grooves MN, rays such as S′A′, S′I′, S′B′, so that the eye placed in C may receive by reflexion the rays which it gives, it is easy to see that the differences of the paths of the rays being the same as in the preceding case, the same tints should be observed at the same part of MN, which is conformable to experiment.

If we suppose the rays not to be parallel, but to proceed from F, Fig. 2, then the colour in AB will depend on EB+BG, for if this quantity is equal to one or to several undulations of a certain tint, this tint will be seen in this direction by the eye placed at C. Let δ, as formerly, denote the angle HCB or HCA, and α the angle HFA or HCA, we have

$$\frac{BG}{AB} = \text{Sin.}\,\delta; \quad \frac{EB}{AB} = \text{Sin.}\,\alpha$$

Whence $BG = c\,\text{Sin.}\,\delta$; $EB = c\,\text{Sin.}\,\alpha$

Whence $BG + EB = c\,\text{Sin.}\,\delta + c\,\text{Sin.}\,\alpha$

but this quantity ought to be a multiple of λ. Hence

$$m\lambda = c\,\text{Sin.}\,\delta + c\,\text{Sin.}\,\alpha$$

$$\text{or } \frac{m\lambda}{c} = \text{Sin.}\,\delta + \text{Sin.}\,\alpha$$

Figure 3 shows the case where the plane of the system of grooves is oblique to the rays which form the direct image. When the rays proceed from G and reach the eye at C from the part AB, we shall have $GAC - GBC = m\lambda$.

The same figure shows the case of oblique reflexion, the rays proceeding from F and reaching the eye at C by reflexion. If from the mean of the common perpendiculars AD, BE, we take away equal parts in the paths of the rays FAC, FBC, the tint will depend on the difference between the semicircles AE, and DB, which may be easily replaced by the sines of the angles in the expression

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" It has been long ago observed, and Dr Brewster, if I am not mistaken, made the remark, in treating of the superficial colours of mother-of-pearl, that the images seen in this case of multiplied diffraction approached nearer the solar spectrum formed by refraction than the prevalent colour of ordinary diffraction, or those of the rings analyzed by Newton. But it is to M. Fraunhofer that we owe the most precise experiments on these colours.

" The following is the principle by which I propose to explain this phenomenon. If there is a series of parallel lines capable of furnishing by pairs the ordinary colours of diffraction, the union of a considerable number of these lines ought to have the effect of narrowing extremely the fringes formed by homogeneous light, so that after the brilliant central line there ought to be some total darkness; that is to say, in place of the second brilliant band of the narrowest fringes, which the two parallel lines the most remote would have formed; and this darkness will be followed only by paler bands of light, which will go on diminishing to half the distance of the second principal brilliant line of any single pair of adjacent lines.

"There are two methods by which we may calculate the properties of a combination of several undulations: The one is, to confine ourselves to a given instant to find the sum of the motions which take place for any corpuscle, and to calculate after the maximum and minimum for every possible instant. The other is, to determine for every differential, or individual addition of motion the properties of the whole resulting undulations. M. Fresnel made use of the latter, which is perhaps the most general and elegant; but the first is the most simple, and is applicable without difficulty to the instants of maximum and minimum by the aid of the general principles of variable quantities.

"Let us suppose, for example, that there are 100 parallel diffracting lines traced at the distance of the 5000dth of an inch from each other, and all equally distant both from the source of light, and from the card or lens which receives the images, and let us consider only the homogeneous green light whose undulations are nearly the 50,000dth of an inch. It follows from experiments which I published in 1801, that each pair of the lines will exhibit brilliant bands at angular distances from the middle point, whose series are the numbers $\frac{1}{10}$, $\frac{2}{10}$, $\frac{3}{10}$, to $\frac{10}{10}$. The lines which I had then at my command not being equidistant, I was unable to draw from them the remarkable consequence which the experiments of M. Fraunhofer have since given,* and which I had observed also on the Iris buttons of Mr Barton; that is, that each colour is contained within limits as well marked as those of the solar spectrum produced by refraction; whereas, with a single pair of lines, or with a single narrow wire, we distinguish only confused and mixed colours, as in the reflected rings of Newton.

* This result was established so long ago as 1813 by Dr Brewster's experiments on a mother-of-pearl spectrum, which he found to be capable of being corrected by the opposite action of a prism of flint glass of 65° "*a large secondary spectrum being left, having the uncorrected green towards the vertex of the prism.*" *Phil. Trans.* 1814. By this experiment, not only was the general resemblance of the spectra established, but also their specific difference, or the fact, that the least refrangible spaces were *more expanded*, and the most refrangible ones *more contracted* in the mother-of-pearl than in the glass.—Ed.

"There are, indeed, only single places marked by the precise middle of the largest common fringes, where the 100 supposed systems of undulations are capable of mutually supporting themselves, and of co-operating together. In these places the elementary oscillations are exactly contemporaneous, in virtue of the equality of their paths, like those of the middle: They follow at intervals equal to an entire undulation, for it is evident that in the second brilliant band, where the undulations of the second system have lost an oscillation relatively to those of the first, the undulations of the third system will have lost two of them, since the third line is distant from the first double that of the second, and the undulations of the 101st line will have lost 100 undulations; the third band will be still one greater, but the effect will be the same throughout, and each band will have the united force of all the 100 centres of diffraction; whilst, at a very small distance from the middle line, and such that the light coming from the most remote points has lost only an undulation by the difference of paths, the oscillations will follow at distances almost equal throughout the circumference of the circle which represents them, and the respective velocities which are proportional to the positive or negative cosines will mutually destroy one other, so that their sum will be *zero*. This distance is the 100dth of the tenth of the radius, or the 1000dth of the whole distance of the card; and if this whole distance is 100 inches, the width of the brilliant band will be one-tenth of an inch on each side, or one-fifth altogether, including the space imperceptibly illuminated by the enfeebled light; the precise brilliant band being probably much narrower.

"Without knowing the law according to which the primitive impulsions ought to diffuse themselves in all directions, it is impossible to calculate exactly the illumination of the different points of the space on the card; but we may suppose these elementary impulsions equal in all directions, as Huyghens has done, or at least in all directions near the rectilineal one, as M. Fresnel has done. It is also more convenient to presume that they follow the law of the sines and cosines, which seems to be that of the greater number of small natural vibrations.

Then calling x the distance of any point of the card from the middle of the nearest brilliant band, and supposing that x

becomes 360° for the interval of the two bands, we may always represent the velocity of the oscillations which unite these at any given instant, by the sum of a series of the cosines of the number n of arcs of a circle whose common difference is x; for example, $\cos. 0 + \cos. x + \cos. 2x \ldots + \cos. nx$, for the instant when the velocity of the first oscillation alone is at its maximum; but it is evident that the entire sum will be a maximum when the mean term is a maximum; it is easy to see also, that for a considerable number of divisions we may find the sum of the series by representing each term by the narrow space given by the division of a figure of the sine, and the sum of $\cos. 0 + \cos. x + \cos. 2x \ldots + \cos. nx$, will be nearly $n \frac{\text{Sin}. nx}{nx} = \frac{\text{Sin}. nx}{x}$; a quantity whose differential vanishes when $nx = \text{Tang}. nx$; and to find the *maxima* of light, we must take the values for $\frac{n}{2}$ or the half of the lines, and for x positive or negative.

"For the two first *maxima* beyond the middle, the angular values are 256° 27′ = 4.4936, and 442° 37′ = 7.725, the intermediate dark lines being at 180° and 360° of the same scale, and for the intensity of the light, we have $\frac{1}{21.18}$ and $\frac{1}{60.7}$ in comparing the squares of the velocities with that of the total velocity of the middle point.

"This calculation becomes more exact in proportion as the diffractive lines are closer and more numerous, the quantity $\frac{\sin. nx}{x}$ representing always a finite velocity, though n becomes infinite by this multiplication, which happens when we wish to calculate the diffraction of a very narrow pencil of light which enters a dark room by a narrow aperture. But when this pencil is sufficiently long, so that there is a sensible difference between the paths of its different parts with regard to the middle of the card, then the experiment comes under the circumstances of the problems so successfully resolved by M. Fresnel."—*Ann. de Chim. Fev.* 1829.

Art. XVIII.—*Notice of a Remarkable Electrical Cloud.* By the Reverend John Macvicar, A. M. Lecturer on Natural Philosophy in the University of St Andrews. Communicated by the Author.

On the evening of the 23d May, about 8 p. m., when I was returning from Strathmore, and had gained an eminence on the Sidlaw range, I rested to admire the contrast in the aspect of the sky over the highlands which bounded the horizon in the region of the sun, and over Fife and the ocean which lay in the opposite quarter. The western and northern sky was very clear and serene, and almost destitute of colour, though the sun was not far from setting. Between that horizon and the zenith there were several small cumuli of their usual indigo tint, with red on their aspects facing the sun. Their number increased towards the region opposite the sun, so that the canopy in that quarter might be said to be covered with cloudy matter, much in the state of cirro-cumuli at that elevation. Beneath this stratum there were nimbi, a slight one over my head, and some very heavy ones passing slowly from the west over the hills of Fife, which form the southern bank of the Tay, and tending towards the north. Where I was, there was no sensible wind, but the cottage smoke was bending from the east. In the nimbus over head (in its rain) a rainbow was developed, and its southern limb, which was formed upon a very dense nimbus over the Tay, was extremely vivid, but the colours were very much blended. In this vivid region a secondary arch was developed. To the westward of the secondary arch there was a heavy cloud, whose under aspect was strangely illuminated, so that the cloud seemed as if actually formed of rectilineal pencils of aqueous vapour, like the streamers of the aurora inverted; and what makes me trouble you with this description at all, is the circumstance that these illuminated pencils of cloud lengthened and shortened, and changed their form (though not their place) as fast and as distinctly as the streamers of a moderately active aurora. They seemed to be directed towards the loftiest hills. The large one represented in Plate II. fig. 9, which represents as dark the portions which ought to be bright, was the most active, and the cloud which surmounted it was drawn down in a similar manner, though not so remarkably. These clouds moved towards the east, and when they passed through the region where the rainbow was formed, they had the effect of insulating the colours much more perfectly than when they were developed in an ordinary dark nimbus. When these clouds passed away, the upper stratum of aqueous matter presented that mottled appearance (like tin-plate mottled by a very weak acid,) so often observed when a cloud is giving off lightning, or insulated in a different electric state from the ground beneath, or possessed of that quantity which constitutes the natural equilibrium of a cloud. I mention this phenomenon, not as if it were the index of an unusual state of a cloud, but rather an ocular evidence of what probably occurs always during a silent discharge of electricity from a cloud without being perceived. The elevation, my distance from the cloud, and the lowness of the sun, enabled me to look upon it in circumstances very favourable for observing changes in it by changes in its action upon light. I should suppose, that these illuminated beams were portions rendered more highly symmetrical by their electric state, and capable of reflecting light which was quenched in other regions. The superior symmetry of the whole to an uniform dense nimbus, seems to be indicated by the fact, that the rainbow or coloured ring formed in it when it traversed the region in which such a display was possible, had its colours far more completely insulated and defined. The phenomenon was singularly like the aurora; and this much perhaps may be inferred respecting both, that as those who were in the region of this cloud could certainly not see its changing beams, so those who were in the region of an aurora could not see its streamers possessing the aspect which they exhibit to those who see a vertical projection of them.

DUNDEE, *May* 26, 1829.

Art. XIX.—On the atomic constitution of the Cyanide of Mercury. By J. F. W. Johnston, M. A. Communicated by the Author.

The admirable researches of Gay-Lussac have long ago shown the true constituents of the cyanide of mercury,—that it consists of the metal combined with cyanogen, and that when resolved into its ultimate elements by direct analysis, the gaseous products, with the exception of a little hydrogen derived from moisture or prussic acid retained between the plates of the salt, are carbonic acid and azote in the proportion of two volumes of the former to one of the latter. But though its constituents be thus correctly made out, I am not aware that any chemist has determined by experiment the atomic constitution of this salt. It is usually called the Cyanide of mercury; but I find it nowhere stated whether the constituents exist in it atom to atom, or in what other ratio they are combined. The following experiments clear up this point, and show the salt to be a *Bi*-cyanide :—

1. Five grains of the dry salt in fine powder mixed with peroxide of copper and heated to redness in a glass tube by the flame of a spirit lamp till gas ceased to come over, gave in four experiments the following results :—

	Carbonic acid.	Azote.	Cyanogen.	Atomic proportion.
No. 1,	3.99 inches.	1.8	1.995	7.0
2,	3.73 ——	1.77	1.865	6.4
3,	3.7 ——	1.7	1.85	6.37
4,	3.73 ——	1.74	1.865	6.4
			Mean atomic proportion,	6.54 grs.

The third column containing the volume of cyanogen whose elements are given off, is simply half the volume of carbonic acid obtained. For the volume of azote being always less than half that of the carbonic, probably from the formation of some nitrous compounds, this mode of estimating the cyanogen has the less chance of error.

The fourth column indicating the weight of cyanogen combined with 25 grains or one atom of mercury according to these results, is obtained for No. 1, by the following ratio :—

1.995 inches cyanogen = 1.097 grains.

∴ 3.903 : 1.097 : : 25 : 6.7, and so on for the rest.

Now, 6.5 being the weight of two atoms cyanogen, No. 1 errs in excess; the three others are a little deficient. The errors, however, are very small; for even No. 3, which is the most incorrect of those deficient, would have given 6.5 for the atomic proportion had the carbonic acid collected been only one-twentieth of an inch greater. This error may be due either to measurement or to a minute portion of the cyanide remaining undecomposed. No. 1 is so much (half a cubic inch) in excess, that I fear there must have been some cause of error which I could not discover.

2. Five grains of cyanide heated in like manner with 50 peroxide of mercury till gas ceased to come over, gave in three experiments

	Carbonic acid.	Azote.	Cyanogen.	Atomic proportion.
No. 1,	3.84 inches	1.865	1.92	6.69
2,	3.882	1.8	1.941	6.7
3,	3.83	1.926	1.915	6.65
			Mean atomic proportion,	6.68

These results agree in being all in excess. The quantity of azote is also greater than by the peroxide of copper, and in No. 3 is almost exactly half the volume of the carbonic acid.

3. When the cyanides, the sulpho-cyanides, the ferro-cyanides, or the new salts called the *red* * ferro-cyanides, are mixed

* The Cyan Eisen Kalium (Rothes) of Gmelin; the Cayanure Rouge de Potassium et de Fer of Robiquet, is the only one of these hitherto employed for chemical purposes. Robiquet disputes with Gmelin the right of discovery, because he knew nothing of Gmelin's published experiments till he had made his own. On the same ground I might advance a similar claim, as I obtained the potash salt three years ago in beautiful crystals, and was indebted to Dr Thomson for directing me to Gmelin's paper. But my mode of forming the salts leading me to infer the presence of chlorine, I

with chlorate of potash, they detonate by heat, by friction, and in some cases by percussion. The sulpho-cyanide of potassium rubbed in a mortar in this way detonates with a purple flash, and with much greater ease and violence than sulphur in the same circumstances. The same salt, the crystallized ferro-cyanic acid, and the acid of the red ferro-cyanides mixed with chlorate, detonate under the hammer, while all the salts of cyanogen, excepting the oxy-cyanides of Wöhler, explode by a very gentle heat, or by merely scraping together the parts of the powder in a glass mortar with the broken end of a glass rod. The exception of the oxy-cyanates shows that it is the affinity of the carbon for oxygen which determines these rapid decompositions. If the parts of the powder be separated by a sufficient admixture of pounded glass, the decomposition may be so regulated as to admit of the gaseous products being collected with perfect precision. In the following experiments the mixture was introduced into a glass tube of from three to five-tenths of an inch in diameter, connected by a small bent tube with the mercurial trough. The flame of a spirit lamp was then applied for a short time to the extremity of the powder nearest the open end of the tube. It speedily ignited, when, the lamp being removed, the ignition and decomposition proceeded gradually along the tube till it reached the sealed end, when gas ceased to be given off. The flame of the lamp was now passed along the tube to insure the entire decomposition of the whole substance operated upon.

This mode of analysis is peculiarly elegant, and, from the little heat required and the very short time necessary to perform an experiment, is admirably adapted for public exhibition. The following results show that it admits also of nearly as much accuracy as the other methods.

Mixed with an equal weight of chlorate of potash and 50 grains of pounded glass, five grains of cyanide in four experiments gave

considered them to be chloro ferro-cyanides, and as such have described some of their properties in a short paper inserted in the last Fasciculus of the *Edinburgh Transactions*.

	Carbonic acid.	Azote.	Cyanogen.	Atomic proportion.
No. 1,	3.8 inches	1.78	1.9	6.6
2,	3.75 ——	1.88	1.875	6.5
3,	3.62 ——	1.78	1.81	6.21
4,	3.67 ——	1.9	1.835	6.32
		Mean atomic proportion,		6.407

These results agree as much among themselves, and come about as near the truth as by either of the former methods. The azote also differs a little from half the volume of carbonic acid.

4. Let us now take the mean result of the whole three methods, and we shall probably not be far from the truth.

Mean result by peroxide of copper = 6.54
—————————— mercury = 6.68
—————— chlorate of potash = 6.407

—————— of the whole = 6.54

That is to say, 25 grains of mercury when converted into cyanide are combined with 6.54 grains of cyanogen by experiment, it is obvious, therefore, that the true composition of the salt is

Mercury one atom = 25
Cyanogen two atoms = 6.5

And the atom of bi-cyanide weighs 31.5

This result leads us to another analogy between chlorine and cyanogen. The *bi*-chloride like the *bi*-cyanide of mercury is a soluble salt, while the proto-chloride (calomel) is nearly insoluble. It is probable, therefore, that there is also an insoluble *proto*-cyanide not hitherto met with. In a note to a paper *on the carburets of azote*, published in this *Journal*, I have mentioned a series of insoluble compounds, which may possibly prove to be *proto*-cyanides.

5. This constitution of the salt may be verified by estimat-

ing the volume of cyanogen given off when it is decomposed by heat. If two atoms of cyanogen be given off, then, from 100 grains of the pure dry salt, we should obtain 37.642 inches of pure gas.

For 31.5 : 6.5 :: 100 : 20.603 grains = 37.642 inches.

But though the volume of cyanogen given off is pretty uniform, yet it falls very considerably short of this quantity.

Thus 20 grains gave	6.3 inches	= 31.5 from 100 grains.
23.2	7.08	= 30.5
30	9.3	31
21.15	6.5	= 30.7
Mean gas given off by 100 grains cyanide		= 30.92 inches.

And 37.642 — 30.92 = 6.722 inches of deficiency. That is, more than a fifth part of the whole cyanogen remains in the tube. Now, as the whole cyanide is decomposed, and there remains in the tube only a charcoal looking substance, either the salt is not a *bi*-cyanide, or the elements of the cyanogen deficient must be contained in the black substance that remains behind. To determine this, what remained in the tube was detonated as above with chlorate of potash, and gave from the first three

	Carbonic acid.	Azote.	Equivalent Cyanogen.
No. 1,	3.2 inches	1.791	1.6
2,	2.99	1.72	1.5
3,	4.58	2.29	2.29

In the two former of these results the azote is more than half of the carbonic acid; in the third it is exactly one-half, as the results of many other experiments on this substance show that it should be.

Add the equivalent cyanogen in the third column to that already obtained by heat in these three cases, and we have

	Cyanogen.	Its elements.	Sum.	Atomic proportion.
20 grains gave	6·3 inches	1.6	7.9	6.93
23.2	7.08	1.5	8.58	6.36
30	9.3	2.29	11.59	6.74
			Mean atomic proportion,	6.676

This result comes surprisingly near 6.5, when we consider the round-about method by which it is obtained, and amply confirms that already obtained by direct analysis.

6. To sum up the results contained in this paper, we have found,

First, That the salt analyzed is a *bi*-cyanide.

Second, That 100 grains of the salt give off by heat about 31 cubic inches of cyanogen.

Third, That what is wanting to make up the whole two atoms of cyanogen is converted into a black carbonaceous substance, consisting of carbon and azote in the same proportions.

It is possible that the volume of cyanogen given off, though nearly constant in the four experiments above stated, may at times vary. According to these experiments, about one-sixth of the whole is converted into the black solid compound, or from every three atoms of the salt we obtain one atom in this state.

This solid *bi*-carburet of azote I have described in the paper on the Carburets of Azote, above alluded to.

PORTOBELLO, *30th May* 1829.

ART. XX.—*Physical Notices of the Bay of Naples.* By JAMES D. FORBES, Esq. Communicated by the Author.

No. IV.—*On the Solfatara of Pozzuoli.*

> ————————" Neapolim inter
> Et Cumas, locus est multis jam frigidus annis
> Quamvis æternum pinguescat ab ubere sulphur."
> CORN. SEVERUS.

THE next object which demands our attention in a survey of the Phlegræan fields is the Solfatara of Pozzuoli, generally considered after Vesuvius the most important feature of the Bay of Naples. So much, however, has been written on the subject, that, had not its importance required a separate article, I should willingly have passed it over more slightly; for it would be difficult, without a continued residence on the spot, to add materially to our information on its phenomena; but I should set too high a value on any original observations I have made, or such theoretical considerations as I have casually proposed, if I were not fully aware, that any interest which the present series of papers may excite, either in the general or scientific reader, must be almost entirely due to the condensed and epitomized view I have endeavoured to take of the labours of my predecessors on this interesting field, who seem for the most part to have taken an insulated survey of some facts, without attempting to embody the results of previous experience, or to furnish the physical inquirer with a statement of such facts as he is naturally desirous to be possessed of, without a reference to the bulky and unconnected works from which alone such a body of information can be derived.

From the nature of the phenomena of the Solfatara, we can best treat the subject by considering, first, its situation and external characters, and afterwards its productions, which are extremely varied and important.

The Solfatara* is the crater of a volcano which can hardly be called entirely extinct. Its connection with the surrounding hills of Capomazza on the west, Astroni on the north, and the "Colles Leucogæi," extending between Agnano and the sea on the east, is so complete as to disprove the assertion of Ferber, that this crater is an insulated one. The rock of which it is composed is a dark and hard one, which will be more particularly noticed afterwards; but from the action of the vapours with which this spot abounds, the whole is decomposed at the surface into a white argillaceous matter, which gives the characteristic colour to this tract of country. The crater itself has a nearly oval form, its greatest diameter being in the direction of S. E. to N. W., having a length of 2337 French feet; the smaller one extending from N. E. to S. W. is 1800 feet, and the circumference of the whole 6850. The southern edge is lower than the rest, coinciding, as Breis-

* This name is sometimes spelled Solfaterra, which perhaps presents the most obvious etymology. Solfatara, however, is much the most usual expression, and may be a corruption, as Eustace supposes, of *Sulphurata*.

lak remarks*, with most of the craters of the Phlegræan fields in that peculiarity of structure. The height of this plain, which occupies the interior of the crater, is 291 French feet, = 310.2 English, above the sea, of which the bounding walls are steep towards the inside, particularly in the eastern quarter, where the Monte Olibano boldly rises and stretches downwards to the sea, forming a point of great boldness, through which the road has been cut a short way from Pozzuoli, between that town and Naples.

The exterior flanks of the crater are less abrupt, joining, as we have said, in some places with the ridges which surround it. From the spring of the Pisciarella on the side next the lake Agnano the ascent through the ravines there formed by torrents is rather fatiguing. Before we advert more particularly to the structure and phenomena of the Solfatara, we may take a glance of its previous history, and the probable changes which it has experienced within the memory of man.

As far back as the time of Strabo it was known by the name of Ηφαιςυ Αγορα, or "Forum Vulcani," and his description corresponds very well with its present condition. Pliny,† in speaking of the qualities of sulphur, mentions among the sources whence it is procured one, which can only refer to the Solfatara; he says "in Italia invenitur in Neapolitano Campanoque agro, collibus qui vocantur Leucogæi;" the spot before us being the only one in this range of hills which affords sulphur in a commercial quantity. A famous passage of *Petronius Arbiter* commencing "Est locus exciso penitus demersus hiatu," is too well known as a poetical picture of this scene in describing the infernal regions to require quotation; but the most satisfactory account of the ancient condition of this crater is in the short passage of Cornelius Severus, placed as a motto to this paper, in which the term "multis jam frigidus annis" cannot be supposed to exclude the idea of the production of sulphur, but merely in allusion to the formerly

* *Essais Mineralogiques sur la Solfature de Pozzuole.* 8vo. Nap. 1792, p. 17. The measurements are also taken from this work, as Breislak had the best opportunity of determining the size. Soulavie makes it only 1500 feet by 1000, and Ferrari gives 1100 palms for the greatest length.

† *Hist. Nat.* lib. xxxv. cap. 15.

active condition of the crater, when it discharged the lava composing Monte Olibano, to which its character of a Solfatara might well be considered "frigidus." Silius Italicus and some other ancient writers seem to allude in different passages of their works to the phenomena of this spot; but in all these records, whether poetical or historical, we have no intimation of an actual eruption of the crater, though it is sufficiently evident that such must have taken place. It is such an event, and that only, which breaks the silence of the middle ages regarding the Solfatara. It is on tradition that an eruption took place in the year 1198, during the reign of Frederic II. emperor of Germany, though the authority for this important event is very obscure. Indeed writers on the subject seem to have copied one from another without any reflection; but on investigation I find that the earliest authority given for the fact appears to be Capaccio, quoted in the *Terra Tremante* of Bonito, and it appears to be overlooked by the Italian historical writers of note, as Muratori, who does not mention it in his *Annali d'Italia* for that year, and Giannone in his *Storia di Napoli;* nor does it appear in the Modern Universal History; yet though unsupported by very sufficient testimony, it would be extremely bold to deny the occurrence of an event which must have been of the most striking notoriety at the time, and which it is not possible to conceive any old Italian writer to have invented, though we may well imagine how no circumstantial detail should have reached us from that time, when more than usual gloom benighted the literature and the records of Europe, and Italy was subjected to all the miseries of foreign and intestine wars, more calculated to absorb attention even than the ravages of volcanic eruption.

The fact is also confirmed by natural appearances. It may safely be said that the Solfatara has a more modern appearance as a volcanic crater than any of the surrounding hills, Astroni not excepted; and its actual activity is also far greater, so that no writer has hesitated to consider it the object of the bay most nearly approaching to the condition of Mount Vesuvius, although Monte Epomeo in Ischia was in a state of activity in 1302. The uppermost formation, too, which we observe in the crater of the Solfatara, particularly the loose scoriform matter which surmounts the mass of trachyte to the eastward has every appearance of recent origin.* Farther, the Temple of Jupiter Serapis at Pozzuoli, the phenomena of which I intend to consider in my next paper, having been buried by volcanic ashes in the middle ages, there seems no agent so likely to have produced this effect as the crater of the Solfatara. The Monte Nuovo, which is the only other plausible source, being *three times as distant*, and besides, not having appeared till 1538, it seems not probable that all memory of the temple should have been entirely lost after an age so advanced in civilization, till disinterred during the last century. From all these reasons, therefore, it seems natural to adopt the received opinion of authors, though certainly it has been copied generally without examination, that the Solfatara was in a state of eruption in the year 1198.†

To this eruption, however, we can impute nothing more than a discharge of scoriaceous matter, for the only true lava stream has, as we have already mentioned, the true trachytic character, and probably had its origin during the most remote ages of tradition; for various circumstances lead us to believe that a state of inflammation then existed of a far more serious nature than any which has occurred there since history became a science. The Greek root of the name "Campus Phlegræus," which had its most legitimate application to this spot,‡ besides the appellation of Strabo already quoted, marks the fact; but I would more particularly remark, that in this spot, the scene was laid by Diodorus Siculus and other ancient writers, of one of the contests of Hercules with the giants,§

* For this remark I am indebted to a paper of Mr Scrope's on the volcanic district of Naples, which has appeared in the *Geological Transactions*, (N. S. vol. ii. part 3.) which came to my hands since writing the last of these notices. I am glad to find many of my ideas confirmed in this paper, which also affords me the occasion of some new remarks which I shall introduce in the progress of these Notices.

† Mr Scrope in the paper just cited says 1180; but this seems to be an entire mistake.

‡ Cluverius, *Italia Antiqua*. fol. vol. ii. p. 1144.

§ Tradunt Herculeâ prostratos mole gigantes
Tellurem injectam quærere et spiramine anhelo
Torreri late campos quotiesque minantur
Rumpere Compagene impositam expallescere cœlum.
Sil. Ital. xii. And Strabo, Lib. v.

fact which, though trifling in itself, when viewed in connection with Dr Daubeny's ingenious and learned remarks on the Typhæus of the Greeks, * is an important analogical illustration of the earlier periods of volcanic inflammation.

The comparative activity of this crater as a mere sulphureous emissary, at different periods, is of difficult estimation. One fact, however, seems pretty definite, that till within the last few centuries water must have been a more abundant production of the crater than it is at present. This we might infer from the expression of Petronius Arbiter, " Cocyta perfusus aqua;" but we have the most distinct testimony of the fact in later ages. Elisio, the physician of Ferdinand of Arragon, a respectable writer of the 15th century, informs us that in his time there was a boiling spring which spouted to the height of even 3 *canne*, or 19 French feet. This remarkable account Breislak (at least when he wrote his " Essais Mineralogiques sur la Solfatare") seems to have distrusted.† However, upon consulting the work of the accurate Cluverius, ‡ I find a description extremely similar. Speaking of the Solfatara he says, " Habet passim lacunas calidorum fontium qui instar bullientis aheni perpetuò fervent, aquasque igne ac sulfure mixtas ad vi. sæpe cubitorum altitudinem eructant." It is impossible to construe this into an account of the " fumeroles" as they now exist; nor can we imagine that a man of such high geographical authority as Cluverius, who flourished in the commencement of the 17th century, and who examined most of the countries of Europe, should have taken such a fact from so old an author as Elisio just quoted. Even eighty years since, Nollet observed a basin of water on the east side nearly full, and having a temperature of 34° R. || We must therefore come to the conclusion first stated, that water must formerly have been far more abundant than at present, when it is with great difficulty that a supply can be procured for the purpose of lixiviating the salts with which the soil abounds.

The general aspect of the Solfatara is undoubtedly striking, though its wonders have perhaps been exaggerated by some writers. The wall of the crater is low on the west side, or

* *Description of Volcanos*, p. 440, &c.

† Compare " *Solfatare*," p. 59, and *Campanie*, ii. 70.

‡ *Italia Antiqua*, vol. ii.

|| *Mem. de l'Academie*, 1750.

that next Pozzuoli, and gradually rises with rocks of a more massive form to the opposite one. The general whiteness produced by the disintegration of the mineral substances by the action of emitted gases gives the whole rather a dazzling than an imposing appearance. At the east side rise the emissaries of steam and vapour, which show the igneous action to be still in a condition of great activity, and the substances which surround them, are coated with the party-coloured salts which are contained in the "fumerole." Some authors, and particularly Della Torre,* have asserted that flames are to be seen during the night; but from the account of the best authors, this seems to have been a mistake, at least they were not observed at the close of the last century, when we have the most authentic account of this spot. These rise at short distances from one another through the flat crust of which the bottom of the crater is composed, which in almost every part is warm, and in some so much so, as to afford the means of evaporating the aluminous solutions. The water required to form these, we have just noticed is rare, and it was the Abbé Breislak who first devised the means of procuring a sufficient quantity, as we shall presently explain; but from Sir William Hamilton's account, we are led to believe that the water of La Pisciarella, a spring I have formerly described,† and which lies in the direction of Agnano, on the exterior side of the crater, was transported here for that purpose.

The reverberation heard on striking the ground violently has excited some difference of opinion among authors; many considering it an evidence of a subterranean vault; but the greater part imputing it to the porous nature of the ground, which, by the approximation of its parts from a sudden blow, may produce the effect. I cannot, however, think the former opinion futile; and I have the satisfaction of having Dr Daubeny to support me. Mr Scrope, in his paper just published,‡ and also in his *Considerations on Volcanos*, § thinks he has proved that such a cavity can never exist; yet let us consider how it might be formed in the case of the Solfatara: The apex of the inverted cone or crater, which is truncated by the

* *Storia del Vesuvio.* 4to. Napoli.

† See this *Journal*, last No. p. 261. and No. xiv. p. 265.

‡ *Geol. Trans. ut sup.* p. 345. § P. 267.

flat plain so often mentioned, let us suppose to have been filled up with a solid mass to the level of that truncation at the period of the last eruption ; here we have every thing most favourable to Mr Scrope's opinion. Now we would ask, whence comes the vast bulk of mineral matter annually brought to the surface ? We have it on the authority of Breislak, * that he never found a vein, or even a particle, of sulphur in the natural soil or crust of the Solfatara in the deepest pits he had occasion to make; and he shows that the whole sulphur of commerce is derived from the decomposition of the sulphuretted hydrogen gas, (the mode of which chemists have more lately satisfactorily pointed out, as we shall explain shortly) Now Sir William Hamilton † tells us that even in his time, when the mode of working was confessedly imperfect and dilatory to the last degree, that 273 quintals, or near 30,000 English pounds of sulphur were annually prepared from the deposits of the "fumerole." It therefore becomes a question, whence the millions of pounds were drawn, which for centuries have been deposited in this form? The conclusion I conceive is obvious, that this alone must have formed a chasm corresponding to our ideas of magnitude, without any hypothetical considerations whatever, but which, according to my ideas of volcanic action, are equally tenable both in modern and extinct craters.

The disintegrated soil of the crater is in general unfavourable to the growth of plants, probably from the large quantity of gaseous matter it contains, the abundance of sulphurous acid, and the various acrid salts which it produces. It would appear, however, that Ferber has been too hasty in his remark, that the *Arbutus unedo* and *Erica carnea* are the sole possessors of the soil. There are considerable spaces of vegetable soil in which vegetation is luxuriant, such as the vine and chestnut when planted, as indeed we might expect wherever the potash of the felspathose lavas in a state of disintegration is abundant. The *Erica* and *Spartium junceum* succeed the lichens in such spots. The *arbutus* and *erica* have indeed the appearance of peculiar richness, especially the former, when covered with

* *Campanie*, ii. 120.

† *Campi Phlegræi*. Folio. Vol. ii. Explanation of Plate xxv.

fruit, as it was when I visited the spot in the beginning of December 1826.

The communication of the Solfatara with Vesuvius is a point of great interest, but which unfortunately we have insufficient means of deciding upon, nor has it sufficiently excited the examination of observers. Breislak, who certainly had the best means of judging, wholly denies it, though I feel convinced that his expressions are too strong on the subject, and even contrary to some of the now most received opinions of the laws which regulate the position of volcanic emissaries.—He says, "Beaucoup de physiciens ont voulu etablir une communication entre la Solfatare et le Vésuve, et d'autres entre la Solfatare et la mer. Pour ce qui regarde la premiere je puis assurer qu'elle n'existe pas. J'ai fait sur cela beaucoup d'observations de suite, dont le resultat est que, soit que le Vesuve vomisse des torrents de lave, soit qu'il laisse echapper une epaisse colonne de fumée, soit qu'il soit parfaitement tranquille, les fumeroles de la Solfatare sont constamment dans le meme etat. Ces vapeurs n'ont d'ailleurs aucun rapport avec celles de Vesuve. Dans les premiers domine l'acide sulphureux, dans les secondes le muriatique. La communication de la Solfatare avec le mer n'est pas moins imaginaire."* This is certainly sufficiently conclusive in its terms, yet from what I heard during my residence at Naples, I cannot help doubting the accuracy of the statement, so directly urged in support of a theory now nearly abandoned, that volcanos have no connection with the sea, which Breislak more amply defends in his notes to the larger description of the Solfatara. The very intelligent guide to Vesuvius, a true philosopher, and diligent observer, informed me, that during commotions in the state of Vesuvius, the Solfatara displays much less activity than at other times. Again, when visiting the Solfatara, we were informed that the "fumerole" were unusually quiescent, owing to the agitated state of Vesuvius at that period. With regard to the variation in the nature of the gases in the two localities, it is a partial and incorrect statement of facts, at least according to the best modern authors. Not only does muriatic acid occur at

* *Campanie*, ii. 70.

the Solfatara, but sulphurous acid is a frequent production of Vesuvius; and I find it stated as a general fact, unconnected with the present subject of debate, on the authority of Sig. Monticelli and Covelli, as cited and compared by Dr Daubeny, that the gases evolved by Vesuvius are similar to those of Etna, Volcano, and Solfatara. We have not room to enter farther upon this curious topic.

The rock of which the mountain of La Solfatara is composed is a compact lava, approaching in its characters to those of trachyte. It is for the most part porphyritic, and contains silex and iron in great quantity; in some places the former predominates so much, as to give the lava the characters of hornstone, and in general it affects the magnetic needle. It can serve no good purpose to detail particularly the varieties of this rock, which may be seen at great length in the travels of Spallanzani; the only exception of importance to the features just noticed is the lava stream already mentioned, forming the Monte Olibano, which consists *essentially* of felspar, frequently in a crystallized condition, and combining augite as an accidental ingredient, and chiefly in the upper part of the current. Its fracture is uneven and the colour ash gray, which is lighter than that of most other lavas of the Solfatara; it is covered by fragments of scoriaceous tufa, * probably of the modern formation to which we have already alluded; and we may now add, that in the seams of this substance are various vegetable impressions, which appear to be nearly carbonized. These were carefully examined by Spallanzani, who pronounced them to be undoubted species of *alga marina*, a very curious fact, which, as far as I know is unexampled.

The process of disintegration in all these solid rocks is carried on chiefly by the action of the sulphurous acid vapours, commencing by a removal of colour, then abrading the softer portions, leaving a honey-combed appearance; and when the whole has crumbled to dust, which in a great measure consists of silex, it gives the characteristic colour to the plain, and tends to defend the interior rock from the farther influence of the exhalations. In this condition it affects, according to Mr

* Daubeny, p. 171.

Scrope *, a peculiar concretionary form, which he attributes to a play of chemical affinities. It is ascertained, too, that here, as at the spring of La Pisciarella, the calcareous particles form an oolitic concretion named pisolite or peastone, well known as a production of the hot spring of Carlsbad. It has been a subject of remark that the disintegrated matter of the Solfatara much resembles tripoli, and might probably be employed for the same purposes.

We have now considered in sufficient detail the general characters of the Solfatara, and we may proceed to the second object of the present paper, by giving a very short account of the products of this curious spot, by which we mean such as are daily forming by the action of subterraneous volcanic agency.

The "fumerole," or emissaries through which these are emitted, rise on the eastern side of the plain. The temperature of one of them, during a series of observations made by Breislak in the month of June, remained within the extremes of 75° and 78° Reaumur. The humidity contained in them is very great, and of course rapidly condenses on reaching the external air, which the same observer employed as a means of procuring the requisite supply of water for the solution of the salts used in commerce.

United with the steam of the fumerole, we find sulphuretted hydrogen and a small quantity of muriatic acid gas, and accompanying it, nitrogen and carbonic acid. We shall very briefly state some facts regarding the origin and effects of these elastic fluids, borrowing chiefly from the excellent work of Dr Daubeny, professor of chemistry at Oxford, and from Breislak's detailed account of the Solfatara.

If we adopt the theory that volcanic action is superinduced by the affusion of the metallic alkaline bases by sea water, these effects are easily explained. The oxygen of the water rapidly uniting with the potassium and sodium disengages its hydrogen, which combines with the sulphurous deposits undoubtedly existing at a great depth below the surface of the earth, and appears with the steam produced by the calorific

* *Geolog. Trans.* ut sup. p. 346.

agency in the form of sulphuretted hydrogen gas. The small quantity of muriatic acid accompanying it is easily explained by the decomposition of the muriate of soda in the sea water. The nitrogen may be accounted for by the accidental access of atmospheric air to the seat of volcanic oxidation. Carbonic acid, as we had occasion to mention when speaking of the Grotto Del Cane in the last number of these Notices, probably owes its origin to the effect of internal heat upon calcareous strata, and its prevalence in those volcanos only which are nearly extinct has been ingeniously explained by the fact, that potassium, and probably the other alkaline and earthy bases when heated, decompose this gas. Such is one of the most modern and most elegant explanations of the origin of these elastic fluids, though I shall not venture here to give an opinion upon the plausibility of the great chemical theory upon which they are built. The subsequent action of these gases, however, is sufficiently obvious, and accounts for all the varied products of this crater. The principal of these we shall shortly notice.

The sulphuretted hydrogen combining with the various substances contained in the rock of the Solfatara forms the class of hydrosulphurets, which being decomposed by the union of carbonic acid with the bases, the sulphuretted hydrogen is separated into hydrogen, which forms water with the oxygen of the atmosphere, and sulphur, first forming hyposulphates of the earthy bases, and finally sulphates, but a great part is precipitated into the natural forms of *sulphur.*

This substance occurs either crystallized or compact; but it is a curious fact that it seems to be entirely the production of the "fumerole," as no sulphur has been detected in the natural plain of the Solfatara. For a long time the manufacture of sulphur was continued during the last century with little profit, and the usual inattention to the economy of labour, and management in general, which too often characterizes manual operations in Italy: the product was annually only 270 cwt., selling at sixteen livres per cwt.,* which was found so little profitable, that about fifty years ago, when the Solfatara

* Lalande, *Voyage en Italie,* vii. 329.

was put under the direction of the Abbé Breislak, it appears to have been stopped. * At present, however, it is carried on, and apparently in the very same method as formerly, which we find described by Fougaroux de Bondaroy in the *Memoirs of the Academy of Sciences.* † The following was the apparatus employed when I visited the spot. Figures 7 and 8 of Plate II. represent the horizontal section and the elevation endways of the furnaces; the impure sulphur is deposited in the earthen jars B, which are filled from the top *e*, and the mouths then luted. When fire is placed in the receptacle at A, the sulphur is sublimed through the tubes *a a* into the receivers *c*, also of earthenware, and kept cool by an opening *f* to the open air. By this simple operation the sulphur is extracted in a commercial state from the earth of the plain lying nearest to the "fumerole."

The next production of the Solfatara which we shall notice is the Sulphate of Alumina, the presence of which is easily accounted for by the union of the sulphuric acid, of which the origin has been already noticed, with the base of aluminous earth, so abundant in felspathose lavas; its external character is generally filamentous or fascicular, and it is extracted in considerable quantities by lixiviating the soil of that part of the plain where it abounds. 37 cwt. used to be annually prepared. ‡

The Muriate of Ammonia is one of the most important commercial products of the crater. Its occurrence is thus accounted for. When muriatic acid combines with a hydrosulphuret, a portion of hydrogen is disengaged after the deposition of a sulphureous oily matter; uniting with the compounds of the atmosphere it forms water and ammonia, and the latter combining with part of the muriatic acid is sublimed from the "fumerole" in the form of sal-ammoniac. Previous to the superintendence of the Abbé Breislak, only two hundred weights

* Spallanzani's *Travels*, i. 83.

† For 1765, 12mo edit. p. 418.

‡ Some curious particulars of the alum of the Solfatara and a comparison with that of La Tolfa, near Rome, will be found in the *Annales des Mines.*

of this salt * were annually procured. By an adaptation of a number of tubes of baked earth united in one large recipient, he succeeded in obtaining it in larger quantity. By inspecting the account of this salt in his "*Essais Mineralogiques sur la Solfatara*," we may form an idea of the extreme uncertainty of chemical science at a period not very distant, when that work was written.

Sulphate of Lime or Gypsum in an earthy state is an extremely abundant production of this crater, particularly on the exterior side next the lake Agnano, where the Monte Secco, from which the water of La Pisciarella flows, is chiefly composed of it. It seems difficult to account for the abundance of this mineral; for it would be hard to believe that the vast beds which now appear on the surface should have been solely produced by the filtration of spring waters bringing from the deep-seated limestone strata particles of the rock from which the carbonic acid has been expelled by the superior affinity of the sulphuric. It has been suggested that the origin of these gypseous depositions is owing to the calcareous masses ejected by the volcano when in a state of activity. This certainly is rather hypothetical, nor, judging from the example of Vesuvius, can we suppose it very adequate to the effect. I should think it would be more plausible to suppose that a bed of Apennine limestone had once cropped out from beneath the volcanic formations, which, as this is one of the most active *foci* in the neighbourhood, is rendered more probable from the theory of the parallel lines of eruptive energy. The sulphate of lime, which can be derived from the decomposition of the lavas of the Solfatara (in which only 1 per cent. of lime occurs) is quite insufficient to account for the effect; they only produce some small radiated specimens, which are occasionally met with.

The Sulphates of Magnesia and of Soda each occur but in a single part of the Solfatara, and the rarity of the former as a volcanic mineral is rather to be wondered at, since it forms about $\frac{1}{25}$ of the mass of the surrounding rocks. The origin of the latter salt has excited some debate; but if we admit that sea-water is an agent in the production of volcanic phases,

* Lalande.

and if the acid be, as we have already supposed, disengaged by a chemical affinity exerted at a high temperature with the clay or sand present, the base will afterwards be in a condition to unite at the surface of the plain with the sulphurous and sulphuric acid vapours.

The only other sulphate we have to notice is that of Iron, which occurs generally in greenish acicular crystals under four lines in length, along with the native alum in the vicinity of the "fumerole." The sulphuret of this metal is here abundant, which is the more remarkable, as, though long considered the most approved prime mover in the theories of volcanic action, Dolomieu notices but one specimen in Mount Etna, and Gioeni none in Vesuvius, notwithstanding the erroneous statement of Sir William Hamilton; nor could Spallanzani detect it in Stromboli and Volcano. In the Solfatara it occurs in two forms, not only incorporated in the rocks of the crater, or lining drusy cavities, but in a state of sublimation from the active emissaries. Since sulphur only accumulates for a long period, and becomes an important feature in volcanos when half extinct, perhaps we should not be surprised to find its compound with absolute iron ore, which abounds in almost all lavas, in a case like the present. The octohedral magnetic iron ore is here very abundant, both in the solid rocks of the mountain and in the ferruginous sand where the sea washes its base.

These and some minor products of the Solfatara give the spots which they incrust a very peculiar, and often beautiful appearance. The shades of the sulphur softening from the deep-orange formed by the combination of arsenic, through all tints to the palest straw-colour, from the mixture of the various salts which have been enumerated, have an attractive appearance; nor less so the more unusual colour of green vitriol, similarly diversified; and both the yellow and green merging at last as we retreat from the immediate action of the "fumerole" into the monotonous white which characterizes the whole plain, rendered here and there more conspicuous by the siliceous sinter which was first discovered by Dr Thompson, and in some places forms a white crust of two or three lines in thickness.

Such being an account of the Solfatara and its productions, as far as our limits permit us to dwell upon this part of our subject, we may, in conclusion, notice those volcanic craters occurring in different parts of the world, in a state similar to the one before us. The general congruity of such phenomena, however, the small stock of information which we comparatively possess relative to inter-tropical volcanos, and the superior interest of the Solfatara of Pozzuoli, as well as the care with which it has been examined, admit only a moment's attention to this subject.

Since sulphur is converted into vapour at 290° of Fahrenheit, the habitual temperature of any spot which exhibits it as a characteristic product must be below that point, and, it is hardly necessary to add, must be free from those paroxysms which would destroy the nature of the crater, and probably change the emanating gases. Now, as it is known that sulphurous acid and sulphuretted hydrogen gases mutually decompose each other, the portion of either emitted from any crater is understood only to be the excess of the one above the other.* But in all active volcanos the sulphurous acid appears to predominate, and does not lead to any direct and extensive deposition of native sulphur, while, on the other hand, sulphuretted hydrogen becomes the characteristic gas of extinct emissaries. Thus, to take only Italy and its neighbourhood, whilst at the Solfatara, at Sermoneta, at Terracina, at Castelamare, at Acerra, at Jaci Reali, (Sicily;) among the least active of the Lipari Isles, and other examples almost innumerable, the hydrogen abounds; at Etna, Vesuvius, and Volcano, the only true modern emissaries, it is wholly unknown. Such then are the general conditions of the formation of a Solfatara.

The most accurate examples of Solfataras with which we are acquainted are in the West Indies, and I shall notice no others, for the Lipari Isles, the crater of the Peak of Teneriffe,† and other more imperfect examples, can hardly be classed under this denomination. Among the Carribbee Islands the most important is Guadaloupe, of which we have some account in the "*Mémoires de l'Academie*,"‡ in the *Annales des*

* Daubeny. † Humboldt, *Pers. Nar.*

‡ For 1750, *Histoire*, p. 48, 12mo Edit.

Mines," by Dupuget,* and in the *Geological Transactions*. In the first we have an account of one of the active eruptions of the principal mountain in the island in the middle of last century, which appear to take place from the sides, leaving a plain on the top in the form of a Solfatara. In 1797 an eruption took place at the height of 4800 feet. Dupuget is most particular in his account of the sulphureous part, which he describes as extremely active and filled with vapours. He describes minutely three caverns he observed on the mountain, the first of which is 45 feet by 25, and of difficult access, through which vapour rises of a temperature of 32° R. = 104° Fahr. and the walls are abundantly lined with green and white crystals, thus presenting phenomena apparently identical with those which we have been endeavouring to describe. In the plain at top springs occur, having a temperature of 73° R. = 198° Fahr.

Another of the most remarkable of these islands is St Vincent, the great mountain of which is called Le Souffrier, a name which it probably received before 1718, when it changed its character and became an active volcano, and again in 1812 desolated the island by a most awful eruption. In Martinique, though the volcanic formations are overlaid by limestone, yet in some places form lofty hills, particularly La Montagne Pelée, which is a Solfatara, and 736 toises high. There has been no eruption of the Peak, at least since America was discovered; but there are several craters on the side, one of which opened January 22, 1792, and discharged much sulphur and black sulphuretted water. Hot springs occur in different parts of the island. †

Several other Solfataras occur in the same group; but the particulars which have reached us serve only to confirm the general fact of the similarity of this volcanic phase wherever it occurs. Montserrat is mentioned as possessing beautifully crystallized porphyritic rocks which have in many places sufferred decomposition from sulphureous vapours, as we have explained in the Solfatara of Pozzuoli.

I have not even mentioned the Solfatara in the Campagna di Roma near Tivoli, nor the Lagunes of Tuscany. The for-

* Vol. iii. p. 44, &c.

† *Annales des Mines.* vol. iii.

mer, indeed, is unworthy of its name, being a mere emissary of sulphuretted hydrogen, without even a deposition of sulphur beds. * The latter presents many phenomena similar to the Solfatara of Pozzuoli, including the emission of sulphuretted hydrogen and steam at a high temperature, with the deposition of sulphur and several salts. In the opinion of some, the phenomena are even more interesting than the more frequented display in the Bay of Naples, but they have excited comparatively little attention, especially among English writers. † At all events they deserve at present particular observation, since it is the opinion of Breislak, and perhaps not devoid of plausibility, that while the Solfatara of Pozzuoli is becoming gradually extinct, the phenomena of the Tuscan Lagunes tend towards a state of perfect inflammation.

Art. XXI.—*Researches on the Elasticity of regularly crystallized Bodies.* By M. Felix Savart, Member of the Academy of Science.

This very able and interesting memoir was read before the Academy of Sciences at Paris on the 29th January 1829, and has been printed in the *Ann. de Chimie* for January and February. The object of the author is to determine the distribution of elasticity in solid bodies, and consequently their structure, by cutting plates out of them in various directions, and ascertaining the sound which they emit while vibrating, and the modes in which they divide themselves, as rendered visible by the figures formed by sand or lycopodium strewed on their surface. A condensed abstract of this curious paper is all that our limits will permit us to give.

M. Savart's first experiments were made upon wood, the structure of which he has analyzed by means of sonorous vibrations. The phenomena are here related to three rectangular axes of

* While this sheet was passing through the press, I accidentally read the Abbé Nollet's account of this place in the *Memoires de l'Academie*, 1750, p. 65, 4to edit., where I find a true sulphureous deposition noticed. In fact, there are two emissaries of sulphuretted hydrogen in this locality, the Solfatara, properly so called, and the Lago dei Tartari.

† A full account will, I believe, be found in "*Santi, Viaggii d'Istoria Naturale*," 3 vols. 8vo, and in the work of a German, named Prystanowski.

elasticity, the axis of greatest elasticity corresponding with the axis of the branch, and the other two axes, which are equal, being perpendicular to the annual layers of the wood. When the branch is nearly cylindrical, the elasticity is sensibly uniform in all the diameters of a section perpendicular to the axis of the branch.

The following are some of the leading results obtained by using plates of beech :—

1. When one of the axes of elasticity is in the plane of the plate of wood, one of the nodal figures (viz. those taken by the sand,) is always composed of two straight lines at right angles, one of the lines being in the direction of the axis of elasticity. The other figure is formed by two curves, like the branches of a hyperbola, having their convex summits towards each other, and equi-distant from the centre of the plate.

2. When the plate does not contain any of the axes of elasticity in its plane, the two nodal figures are always hyperbolic curves.

3. The number of vibrations which accompany each mode of division is generally as much higher as the inclination of the plate to the axis of greatest elasticity becomes less.

4. The plate which emits the most acute sound, or which is susceptible of producing the greatest number of vibrations, is that which contains in its planes both the axis of greatest elasticity, and the axis of mean elasticity.

5. The plate perpendicular to the axis of greatest elasticity is that which emits the gravest sound, or produces the smallest number of vibrations.

6. When one of the axes is in the plane of the plate, and when the elasticity in a direction perpendicular to this axis is equal to that of the axis itself, the two nodal systems are similar. They are each composed of two straight lines at right angles, and the one system is inclined 45° to the other. In bodies with these inequal axes of elasticity, there are only two planes which enjoy this property.

M. Savart next proceeds to the analysis of *rock crystal* by means of sonorous vibrations, and the following are the leading results :—

1. The elasticity of all the diametral lines of any plane what-

ever perpendicular to the axis of a prism of rock crystal, (the axis of double refraction) is sensibly the same.

2. All planes parallel to the axis *do not possess the same elastic state*; but if we take any *three* of these planes, so that the angles which they form with one another are equal, their elastic state is the same.

With regard to light, all the planes parallel to the axis possess exactly the same properties, so that the information respecting structure thus given by sonorous vibrations, is not of the same order as that given by means of light. M. Savart is of opinion that the first method indicates more particularly the elastic state, and the force of cohesion in the different directions of all the planes of the integrant particles, whilst the phenomena of light, depending more especially on the form of the particles, and the position which they affect round their centre of gravity, they are to a certain point independent of the mode of junction of the different laminæ of which the crystal is formed.

3. The transformations of the nodal lines of a series of plates cut round one of the edges of the base of the prism, are quite analogous to those which take place in a series of plates cut round the intermediate axis in bodies which possess three unequal and rectangular axes of elasticity.

4. The transformations in a series of plates perpendicular to any one of the three planes which pass through the opposite edges of the hexaedron, are in general analogous to those of a series of plates cut round a line, which divides into two equal parts the plane angle contained between two of the three axes of elasticity, in bodies where these axes are unequal and rectangular.

5. By means of the acoustic figures on a plate cut nearly parallel to the axis, and not parallel to two faces of the hexaedron, we may always distinguish which are the faces of the pyramid which are susceptible of cleavage. We may also obtain the same results by the arrangement of the modes of division of a plate cut nearly parallel to one of the faces of the pyramid.

6. Whatever be the direction of the plates, the optic axis or its projection on their plane, always occupies there a position which is closely connected with the arrangement of the acoustic lines. For example, in all the plates cut round one of the

edges of the base of the prism, the optic axis or its projection corresponds constantly with one of the two straight lines which compose the nodal system formed of two lines at right angles to each other.

Though there is a great analogy between these phenomena and those observed in wood, yet rock crystal cannot be numbered among bodies which have three rectangular and unequal axes of elasticity, and still less in the number of those, all of whose parts are symmetrically arranged round a single straight line. The same phenomena, indeed, are constantly reproduced in three different positions, and it would appear that every thing is related to the different directions of cleavage—to the faces and to the edges of the primitive rhombohedron. Thus all the plates cut parallel to the natural faces of the hexaedron enjoy exactly the same properties, and these properties are very different from those of plates equally parallel to the axis, but which are perpendicular to two opposite faces of the octohedron. Those plates, too, which are parallel to the cleavable faces of the pyramid, emit the same sounds, and produce the same acoustic figures; whilst plates parallel to the three other faces present figures different from those of the preceding. It would appear, therefore, to follow from this identity of phenomena for three distinct positions, that there is in rock crystal three systems of axes or principal lines of elasticity.

By comparing the phenomena in rock crystal with those in wood, M. Savart concludes, that the individual axes of each of these three systems are as follows: The shorter diagonal of each face of the primitive rhombohedron is the axis of greatest elasticity for each system, while the great diagonal of the face of the rhombohedron is the intermediate axis of elasticity. The axis of least elasticity is perpendicular to the axis of intermediate elasticity, and is inclined 57° 40′ 13″ to the axis of greatest elasticity, this angle being the inclination of the rhombohedral face upon the plane passing through the great diagonal of the same face.

"The transparent carbonate of lime, and the carbonate of lime and iron appear to possess elastic properties, in general analogous to those of rock crystal. Like it, they possess three systems of principal lines of elasticity similar to each other; but the extreme facility with which carbonate of lime cleaves, permits us to discover a peculiarity which does not appear in rock crystal."

"It is known that the rhomb of carbonate of lime is *often susceptible of mechanical division, in directions parallel to its diagonal planes;* but as these planes cut one another perpendicularly in pairs, the intersections of each pair with the rhomboidal faces of the crystal form the great and the small diagonal of each face, so that if we imagine a plane which turns round the great diagonal, it ought always to remain perpendicular to the supernumerary joint which passes through the small one. From this it follows, that if we cut a series of plates round this same line, their structure, considered in the direction of their plane, will be different in different directions at right angles to each other, whence arises the production of nodal lines crossed at right angles, as in plates cut round one of the axes of elasticity, in bodies where these axes are rectangular. We may therefore conclude, that *rock crystal possesses, like carbonate of lime, supernumerary planes of cleavage parallel to the diagonal planes of its primitive rhomboid,* and that it is to the existence of these supernumerary joints that we must ascribe the principal peculiarities of the elastic state of this substance.

"The only marked difference which appears to exist between these two minerals is, that in the carbonate the small diagonal of the rhomboidal face is the axis of least elasticity, while in quartz it is the axis of greatest elasticity. This result is curious, as the former is a crystal with *negative* double refraction, and the latter with *positive* double refraction.

"The preceding researches are doubtless far from forming a complete work on the elastic state of rock crystal and carbonate of lime; nevertheless we hope, that they will be sufficient to show, that the mode of experiment we have used may yet become a powerful means of studying the structure of solid bodies, whether regularly or irregularly crystallized. The relation for example, which exists between the modes of division, and the primitive form of crystals, allows us to hope, that by means of sonorous vibrations we may determine the primitive form of certain substances, which do not admit of mechanical division. It is equally natural to suppose, that less imperfect notions than we at present possess of the elastic state and cohesion of crystals, may throw light on many of the peculiarities of crystallization. It is not impossible, for example, that the degrees of elasticity of a given substance may not be exactly the same, and in the same direction relative to the primitive form, whilst from another cause the secondary form is different; and if this is the case, as some facts induce me to suppose, the determination of the elastic state of crystals will lead to the explanation of the most complicated phenomena of the structure of these bodies. It appears to me indeed, that the comparison of the results obtained by means of light respecting the constitution of bodies, and also by means of sonorous vibrations, will necessarily unite in advancing the science of optics as well as that of acoustics."

Observations by the Editor.

With the greatest deference to the distinguished talents of M. Savart, we suspect that he is mistaken in his views respecting the cleavages of carbonate of lime. We have not only never heard of any cleavage in the direction of the lesser diagonal of its rhomboidal faces, but we have sought for it in vain by processes which could not fail to have exhibited it. The cleavage too in the direction of the greater diagonal, and of which M. Savart observes carbonate of lime is *often susceptible*, is, as we have often shown, a face of composition, though in crystals not compounded there is a weakness of cohesion, or what may be called a secondary cleavage in that direction, as rendered visible by the method explained in a former number of this *Journal.* * Carbonate of lime is *always susceptible* of this secondary cleavage, and only sometimes susceptible of the composition cleavage, which must be that referred to by M. Savart. If his analysis, therefore, has been made with compound crystals, we hope he will repeat it with plates of crystals which he has determined to be simple ones by optical examination.

We would beg to suggest to M. Savart the following topics for investigation:—

* See this *Journal,* No xviii. p. 311.

1. To examine the right and left handed crystals of *quartz*.

2. To examine the structure of *amethyst*, and compare it with that of *quartz*.

3. To examine the pyramidal *sulphate of potash*, and compare it with the uniaxal sulphate.

4. To examine *sulphate of lime* and *glauberite* at different temperatures, so as to determine if the axes of elasticity change while these crystals pass from their biaxal to their uniaxal state.

5. To examine plates of rapidly cooled *glass* possessing regular axes of double refraction.

6. To examine *apophyllite*, *analcime*, and other crystals in which the doubly refracting structure is so singularly distributed.

7. To examine crystals such as *ice*, or in which no cleavage planes have been discovered, and compare them with crystals having the same doubly refracting structure, but possessing regular cleavage planes.

8. To examine the two classes of *pyramidal crystals*, in one of which the double refraction is *negative*, and in the other *positive*,—properties which seem to be related to the existence or non-existence of cleavage parallel to the base of the pyramid.

Art. XXII.—*On the Art of forming Diamonds into Single Lenses for Microscopes.* By Andrew Pritchard. Communicated by C. R. Goring, M. D.

Of the various improvements in microscopes originated by Dr Goring, that which he conceives to be the most important is the construction of single magnifiers from adamant. The details relative to this novel class of instruments I have been induced to lay before the public. Single microscopes, naturally aplanatic, or at least sufficiently so for practical purposes, possess an incontestable superiority over all others, and must be recognized by the scientific as verging towards the ultimatum of improvement in magnifying glasses. The advantages obtained by the most improved compound engiscopes over single microscopes, resolve themselves into *the attainment of vision without aberration, with considerable angles of aperture*; but against this must be set the never-to-be-forgotten fact, that they only show us a ***picture of an object instead of nature itself.*** Now a diamond lens shows us our real object without any sensible aberration like that produced by glass lenses; and we are entitled, I think, to expect new discoveries in microscopic science, even at this late period, ***from very deep single lenses of adamant.*** *

* It seems generally admitted that, within a certain range of power, not exceeding that of a lens of 1-20th of an inch focus, the beauty and truth of the vision given by the new compound microscopes cannot be equalled by that of any single instrument, at least of glass. It is no less true, however, that the *picture* of the compound, however perfect, is not like a real object, and will not admit of amplification beyond a certain point with advantage. Under the action of very deep eye-glasses, the image of opaque objects especially, first loses its strong well-determined outline, then grows soft and nebulous, and finally melts away in shadowy confusion. Let the experiment be made of raising the power of a compound up to that of a 1-60th inch lens, then try it against the single microscope of that power, (having of course the utmost opening the nature of the object viewed will permit.) The observer, if open to conviction, will soon be taught the superior efficacy of the latter, for it will show the lines on the dust of Menelaus with such force and vivacity that they will always be apparent *without any particular management of the light, nor can their image be extinguished by causing the illumination to be directed truly through the axis of the lens, (as it always may in the compound.)* A due consideration of the teeth and inequalities on the surface of a human hair, together with the *transverse connecting fibres between the lines on the scales of the Curculio imperialis,* viewed as opaque objects, will suffice to complete the illustration of the subject; though the last object is not to be well seen by that kind of light which is given by silver cups, and a single lens of 1-60th inch focus can of course have no other. The effectiveness and penetrating faculties of simple magnifiers are invariably increased by an accession of power, however great; that of compounds seems to be deteriorated beyond certain limits. An opinion may be hazarded that the achromatics and reflectors yet made *do not really surpass the efficacy of equivalent single lenses, even of glass, when their power exceeds that of a 1-20th lens*; from 1-20th to 1-40th, the vision may be about equal, but from 1-40th upwards infinitely inferior. The superior light of the single refraction can need no comment, and it is evident that there must be a degree of power at which that of the compounds will become too dim and feeble for vision, while that of the single instrument will still retain a due intensity. For these reasons it is conceived, that the close and penetrating scrutiny of lenses of diamonds of perhaps only the 1-200th inch focus, and an equal aperture (which their very low aberration would easily admit of,) must enable us to see farther into the arcana of nature than we have been empowered to do. Glass globules of 1-200th inch focus, and indeed much deeper,

I shall not fatigue my readers by describing the difficulties which were encountered in the prosecution of the design of making diamond lenses. Nature does not seem to permit us to produce any thing of surpassing excellence without proportional effort, and I shall simply say, that in its infancy the project of grinding and polishing the refractory substance of adamant was far more hopeless than that of making achromatic glass lenses of 0.2 of an inch focus. I conceive it just to state, that Messrs Rundell and Bridge, of Ludgate Hill, had, at the time of the commencement of my labours, many Dutch diamond cutters at work, and that the foreman Mr Levi, with all his men, assured me that it was impossible to work diamonds into spherical curves; the same opinion was also expressed by several others who were considered of standard authority in such matters.

Notwithstanding this discouragement, in the summer of the year 1824, I was instigated by Dr Goring (at his expence) to undertake the task of working a diamond lens. For this purpose Dr Goring forwarded to me a brilliant diamond, which, contrary to the expectation of many, was at length ground into a spherical figure, and examined by Mr Levi, who expressed great astonishment at it, and added, that he was not acquainted with any means by which that figure could have been effected. Unfortunately this stone was irrecoverably lost. Mr Varley having returned from the country, becoming now thoroughly heated with the project, permitted me to complete another diamond which had been presented to me by Dr Goring. This is a plane convex of about $\frac{1}{20}$th of an inch focus. It was not thought advisable to polish it, more than sufficed to enable us to see objects through it, because several flaws, before invisible, made their appearance in the process of polishing. In spite of all its imperfections, it plainly convinced us of the superiority which a *perfect diamond lens* would possess by its style of performance, both as a single magnifier, and as the object lens of a compound microscope. After the lapse of a short interval of a few months, I devoted some time to the formation of a per-

have been executed; but the testimony of lenses of diamond would certainly be far more respectable, and is at least worthy of trial and examination.—C. R. G.

fect diamond lens, and have at length succeeded in completing a double convex of equal radii, of about $\frac{1}{25}$th of an inch in focus, bearing an aperture of $\frac{1}{30}$th of an inch with distinctness on opaque objects, and its entire diameter on transparent ones. It was finished at the conclusion of the year 1826. The date of its final completion has by many been considered a remarkable epoch in the history of the microscope, being the first perfect one ever *made*, or thought of in any part of the world. * I think it sufficient to say of this adamantine lens, that it gives vision with a trifling chromatic aberration, but in other respects exceedingly like that of Dr Goring's Amician reflector, but without its darkness; for it is quite evident that its light must be superior to that of any compound microscope whatever, acting with the same power, and with the same angle of aperture. The advantage of seeing an object *without aberration* by the interposition of but a single magnifier, instead of looking at a picture of it (however perfect) with an eye-glass, must surely be duly appreciated by every person endowed with ordinary reason. It requires little knowledge of optics to be convinced that the simple unadulterated view of an object must enable us to look farther into its real texture than we can see by any artificial arrangement whatever; it is like seeing an action performed instead of scenic representation of it, or being informed of its occurrence on the most indisputable and accurate testimony.

Previous to grinding a diamond into a spherical figure it is absolutely necessary that it should be ground flat and parallel on both sides, (if not a larke or plate diamond,) so that we may be enabled to see through it, and try it as opticians try a piece of flint glass. Without this preparatory step it will be extremely dangerous to commence the process of grinding, for many dia-

* In Dr Brewster's *Treatise on New Philosophical Instruments*, Book v. chap. 2, page 403, Account of a new compound microscope for objects of Natural History, is the following passage:—"We cannot, therefore, expect any essential improvement in the single microscope, unless from the discovery of some transparent substance which, like the diamond, combines a high refractive with a low dispersive power." From which it seems certain that the Doctor did not contemplate the possibility of working on the substance of the diamond, though he must have been aware of its valuable properties.—A. P.

monds give a double, or even a species of treble refraction, forming two or three images of an object. This property of course totally unfits them for making lenses. I need not observe, that it must be chosen of the finest water, and free from all visible flaws when examined by a deep magnifier. It was extremely fortunate for diamond lenses, that this substance is free from the defect of double vision, otherwise diamonds *en masse*, might at once have been abandoned as unfit for optical purposes.

The cause why some stones give single vision, and others several peculiar refractions, may also arise from different degrees of density or hardness occurring in the same stone.* Diamond cutters are in the habit of designating stones male and female; sometimes a *he and a she*, as they have it, are united in the same gem. Their *he* means merely a hard stone, and their *she* a soft one. When a diamond which gives several refractions, is ground into a spherical figure, and partially polished, it is seen by the microscope to exhibit a peculiar appearance of minute shivering crystallized flaws, sometimes radiated, and sometimes in one direction, which can never be polished out. I believe I could distinguish with certainty a bad lens from a good one without looking through it. Precious stones from their crystalline texture are liable to the same defects for optical purposes as diamonds.

Having ascertained the goodness of a stone it must next be prepared for grinding. It will in many cases be advisable to make diamond lenses plano-convex, both because this figure gives a very low aberration, and because it saves the trouble of grinding one side of the stone. It must never be forgotten that it may be possible to neutralize the naturally low spherical aberration of a diamond lens by giving it an improper figure, or by the injudicious position of its sides in relation to the radiant. When the lens is to be plano-convex, cause the flat side to be polished as truly plane as possible, without ribs or scratches; for this purpose the diamond should be so set as to possess the capability of being turned round, that the proper direction with respect to the laminæ may be obtained. When the flat side is completed, let the other side be worked against another diamond, so as to be brought into a spherical figure by the abrasion

* See *Edinburgh Transactions*, vol. viii. p. 160.—Ed.

of its surface. When this is accomplished, a concave tool of cast iron must be formed of the required curve in a lathe, having a small mandril of about $\frac{2}{10}$ths of an inch in diameter, and a velocity of about 60 revolutions per second. The diamond must now be fixed by a strong hard cement (made of equal parts of the best shell-lac and pumice stone powder, carefully melted together without burning,) to a short handle, and held by the fingers against the concave tool while revolving. This tool must be paved by diamond powder, hammered into it by an hardened steel convex punch. When the lens is uniformly ground all over, very fine sifted diamond-dust, carefully washed in oil, must be applied to another iron concave tool. (I may here remark, that of all the metals which I have used for this purpose, soft cast iron is decidedly to be preferred.) This tool must be supplied with the finest washed powder till the lens is completely polished. During the process of grinding, the stone should be examined by a magnifying lens, to ascertain whether the figure be truly spherical; for it sometimes will occur that the edges are ground quicker than the centre, and hence it will assume the form of a conoid, and thus be rendered unfit for microscopic purposes. The spherical aberration of a diamond lens is extremely small, and when compared with that of a glass lens the difference is rendered strikingly apparent. This diminution of error in the diamond arises from the enormous refractive power possessed by this brilliant substance, and the consequent increase of amplification, with *very shallow curves*. The longitudinal aberration of a plano-convex diamond lens is only 0.955, while that of a glass one of the same figure is 1.166; both numbers being enumerated in terms of their thickness, and their convex surfaces exposed to parallel rays. But the indistinctness produced by lenses arises chiefly from every mathematical point on the surface of an object being spread out into a small circle; these circles, intermixing with each other, occasion a confused view of the object. Now this error must necessarily be in the ratio of the areas of these small circles, which being respectively as the squares of their diameters, the lateral error produced by a diamond lens will be 0.912, while that of a glass lens of like curvature is 2.775; but the magnifying power of the diamond lens will be to that of the glass as 8 to 3, their curves being similar; (or, in other words, the superficial amplification of an object, with the perfect diamond lens before-mentioned, is 225000 times, while a single magnifier made of glass, amplifies only 3136 times, reckoning 6 inches as the standard of distinct vision.) Thus the diamond will enable us to gain more power than it is possible to procure by lenses of glass; for the focal distance of the smallest glass lens which I have been able to grind and polish is about the $\frac{1}{80}$th of an inch focal length, while that of a diamond, worked in the same tools, would be only the $\frac{1}{200}$th of an inch.

If we wish to compare the aberrations of the two lenses when of equal power, the curvature of the glass must be increased; and as it is well known the lateral aberrations increases inversely as the square of the radius, (the aperture and position remaining the same,) the aberration of the diamond lens will only be about $\frac{1}{20}$th of that produced by the glass one, even when their thickness is the same; but as the curvature of the diamond is less, the thickness may be greatly diminished. The chromatic dispersion of the adamant being nearly as low as that of water, its effects in small lenses can barely be appreciated by the eye, even in the examination of that valuable class of test objects, which require enormous angles of aperture to be rendered visible, which it is evident must be of easier attainment by diamond magnifiers than by any other sort of microscope.

A mathematical investigation of the spherical aberration of the diamond when formed into lenses, I hope to lay before the public at a future opportunity. The comparative numbers here taken from the longitudinal aberration are, I believe, sufficiently accurate for practical purposes.

Andrew Pritchard.

312, Strand, *opposite Somerset House.*

Postscript.—Since writing the above paper, my attention has been steadfastly devoted in search of a substitute in place of the diamond, which might rank next to that invulnerable substance, and superior to glass, so that I could procure superior amplification over lenses formed of the latter, combined with the most important property of the diamond, viz. that of obtaining a given power with shallow curves. For the difficulty of working that substance, and its expence, which is greatly enlarged by the previous process of determining its goodness for optical purposes, or if we risk the stone (as I have often been tempted to do) its entire loss when the lens is completed, should it happen to possess polarity in the direction of its axis, which render the discovery of a substitute highly important.

It has long ago been determined by Dr Brewster, in his work on new philosophical instruments, that the precious stones offer the best known materials for the formation of magnifiers on account of their feeble dispersion, combined with the high refractive indices of those substances. It will at first appear easy to select that stone which has the strongest refractive power; but here we have to contend with another property which can be avoided in the diamond, viz. the *colour of the stone*, and although this will be very little in deep magnifiers, yet it then becomes more necessary to avoid those stones that do not transmit the rays found by experience most essential for examining the intimate structure of very delicate and minute bodies. Now, the substance of which I form my lenses can be selected nearly free from any colour, while the rays which remain appear, from the experiments of Dr Wollaston, to be admirably suited for viewing the most minute if not the ultimate organization of animal and vegetable tissues; for, in the beautiful and effective method of illumination adopted by him, of separating by means of a convex lens the white light, and adjusting the focus in such a manner that the object shall only be illuminated by the VIOLET *rays*, he was enabled to command at pleasure the vision of the most delicate markings of different test objects, a thing extremely difficult even with the best microscopes and the ordinary illumination. A short time before his decease he showed me several objects, both with glass doublets and my sapphire lenses, illuminated by his method, which certainly exhibited them in a very satisfactory manner, and without any uncommon management, but which objects require great care for their developement in the ordinary mode of viewing them. The sapphires I employ are almost colourless, retaining only a tinge of *violet*, which greatly adds to their value, as the complementary colour would diminish, while at the same time it is less fatiguing to the eye than looking through the latter. Indeed, the advantages of employing sapphire magnifiers is so well established by experience, that nothing is now wanted as a medium between the diamond and glass.

May 1829. A. P.

Art. XXIII.—*Remarks on the structure of the Gibbons, a subgenus of the Orangs or Pitheci.* By Dr Knox, Lecturer on Anatomy. Communicated by the Author.

Two specimens of Gibbons, apparently the species or variety (for there is great confusion in all these matters as regards the higher order of the Quadrumana,) called by naturalists *Pithecus leuciscus*, were put into my hands by J. Robison Esq. Secretary to the Royal Society, (to which they were sent by George Swinton, Esq.) with a request to prepare them in whatever way I thought most beneficial to science. They came originally from Assam, and having been long preserved in spirits, it was not easy to determine very precisely the colour of the head and other external marks, in which the naturalist is of course much interested; but they were Orangs of the subgenus Gibbon, and corresponded tolerably well to the *Pithecus leuciscus* of Schreber and Geoffroy; but known also by a variety of other denominations, such as, Orang, Wou Wou, Simia lar, Gibbon cendre, &c. They were stated to be "mother and son," but they proved both females, the one seemingly perfectly adult; the other quite young. The youngest was much the darker, so that had they not arrived together, and been designated as young and old of the same species, they might well have passed for different species; and indeed, when I describe these specimens as belonging to the species called *leuciscus*, I do not pretend but what they may really be after all the proper Orang Gibbon, the *Simia lar* and *longimana* of Linné and Schreber. The description given of these Gibbons by naturalists, seems to me extremely imperfect, and not warranting their division into species. The *Simia longimana* of Desmarest was described from a single specimen dissected (?) by Daubenton, which obviously must have been quite young, since it weighed only nine pounds. The *Pithecus variegatus* of the same excellent naturalist (Desmarest,) was described by Daubenton from a single specimen, which no longer exists in the French Museum.

The soft parts of the specimens examined by me were not in a good condition, or at least not so as to admit of very nice examination. I observed that the interosseal ligaments between the bones of the fore-arm and of the leg were not present; the larynx is simple, and without those sacs described by Camper in the Red Orang or *Simia Satyrus*, an animal which is now very generally considered merely as the young of the Pongo, and not a distinct species. I did not observe any thyroid gland, though I can scarcely believe it to be altogether wanting. The stomach and intestines had a strong resemblance to the same parts in human structure; the same remark may be made as to the form of the uterus. The kidneys were much rounded, instead of being oval-shaped as in man. The chief peculiarities as to the muscles of the extremities, consisted in the weakness and even absence of certain of these powerful muscles, which bend, extend, and rotate the human thumb; but such peculiarities must have been already described by most systematic writers on comparative anatomy.

In the skeleton I observed that the facial angle of the adult was not superior to what we meet with in the ordinary *cynocephali* or baboons, and this remark, I imagine, will ultimately be found applicable to all the quadrumana: the cristæ, with the exception of the supra-orbital, are not apparent; the circumference of the head, as may be seen by a reference to the table of measurements, is comparatively small. The canine teeth in the upper jaw project very considerably beyond the line of the other teeth. The pelvis extends considerably further than the human does, beyond the level of the coccygeal bones, so that a straight line passing through the pelvis, immediately above the symphysis pubis, would not touch any part of the coccygeal bones, unless a very considerable degree of obliquity were given to it.

The measurements of the skeleton and of the individual bones of the skeleton, compared with the adult male and female human structure, are as follows :—

	Human male adult skeleton.	Human female. adult.	Gibbon female. adult.	Do. young.
Cranial circumference, inches	$21\frac{1}{2}$	$20\frac{1}{8}$	$9\frac{5}{4}$	$8\frac{1}{2}$
Spinal column,	30	28	$14\frac{2}{8}$	$7\frac{2}{8}$
Humerus,	12	$11\frac{1}{2}$	9	$4\frac{6}{8}$
Radius,	9	$8\frac{8}{10}$	11	$4\frac{6}{8}$
Ulna,	$9\frac{4}{8}$	$9\frac{1}{2}$	11	5
Hand,	8	7 nearly	$6\frac{1}{2}$	
Femur,	18	16	8	$4\frac{2}{8}$
Tibia,	15	$12\frac{11}{2}$	7	$3\frac{4}{8}$
Foot,	10	9	$6\frac{1}{8}$	
Total height,	69	$63\frac{1}{2}$	32	19

Art. XXIV.—ZOOLOGICAL COLLECTIONS.

1. *Notice regarding the Osteology and Dentition of the Dugong.* By Dr Knox.

A more detailed account of the facts connected with Dr Knox's observations as to the anatomy of the Dugong, will be laid before the readers of this *Journal* in a future number. The *results* as given in a communication from the author are as follows:—

1st, No complete skeleton of this remarkable animal exists in any of the European museums. If it exists, it has not been properly described by an anatomist competent to the task.

2dly, The incisive teeth in the upper jaw, exclusive of the fang-like incisives, are thrown off or shed at an early period and not replaced by others; an extremely firm horny-looking substance, seems to supply the place of incisive teeth. It encrusts that remarkable sloping portion of the upper jaw, which, together with a corresponding and opposite one in the lower jaw, (also encrusted with a dense horny covering,) forms an extraordinary feature in the general appearance of the face of the Dugong.

3dly, The incisive teeth in the lower maxillary bone remain imbedded in their sockets throughout life; they are neither shed nor replaced. They seeem to be eight in number.

4thly, The teeth termed milk-fangs by Sir E. Home in his paper on this subject in the *Philosophical Transactions*, cannot be temporary teeth, because they are found in the head of an apparently adult specimen, or a least in a head larger, heavier, and denser considerably than another, which, having the tusks formed like those described as permanent by Sir E. Home, must be considered as an adult specimen; and because there are not the slightest appearances of any approaching change in the form of the tooth, or indicative of the approach of another or permanent tooth. Dr Knox considers them therefore as *permanent teeth, not as temporary*; and to reconcile these contradictory statements on the part of anatomists, he supposes it not unlikely that the differences in the form of these tusks may originate not in a difference of age, but in their belonging to distinct varieties or species of the Dugong.

2. *Baron Cuvier's great work on Fishes.**

Two volumes of this work, for which the celebrated author has been collecting materials for upwards of forty years, have appeared. The first volume contains a historical view of the progress of Icthyology, drawn up with all the critical accuracy which the most intimate knowledge of the subject, and of the original writers on it enabled him to display, from the earliest notices of this class of animals among the Egyptians, Phenicians, and Carthaginians down to the present time. Then follows the different scientific classifications which have been proposed; a general idea of the nature and organization of Fishes, and minute details of their external and anatomical characters. This volume is accompanied by nine plates in folio, to illustrate the anatomical details; and as the common Perch is one of the fishes most extensively diffused over the world, and belongs to by much the largest group of fishes, the *Acanthopterygii*, it has been adopted as the example most easily accessible for detailing the leading external and internal characters of the class. We may return to the contents of this volume for some of the interesting information it includes; but

* *Histoire Naturelle des Poissons*, par M. le Baron Cuvier et M. Valenciennes, vols. i and ii. Paris, 1828.

at present we extract from the second volume, which commences with the history of the *Percoides* or Perch family, some of the particulars regarding the *Perca fluviatilis*, or common Perch.

The common Perch, the best known of the osseous fishes of Europe, is one of the most esteemed and beautiful of the fresh water species. The Greeks knew this fish well, and gave it the name which it still retains; for it is evidently the πέρκη which Aristotle describes as depositing its ova in long threads like the frog among aquatic plants. This name, however, has been sometimes extended to fishes which inhabit the sea by Pliny, Oppian, Athenæus, and even by Aristotle himself; but Ausonius seems to have restricted it to its original signification in comparing the Perch with the marine fishes.

> "Nec te delicias mensarum Perca silebo,
> Amnigenos inter pisces dignande marinis."

From this period the same term, more or less altered, has served to designate the common perch in most of the languages of Latin or Teutonic derivation.*

The Perch occurs in all the temperate parts of Europe and in a great part of Asia. It is found from Italy to Sweden, and in Great Britain it is particularly plentiful. In some islands of the North Sea, however, it does not appear to be met with, as it is not mentioned in the Faunas of Orkney and Greenland. It is fished, according to Pallas and Georgi, over all the Russian empire in Europe and Asia; in the rivers which empty themselves into the polar sea, the Baltic, the Black and Caspian Seas. And if the common perch exists not in the North American rivers and lakes, one species is there found so nearly resembling it as to be taken for a variety by many naturalists.

Lakes, rivers, and rivulets are indifferently the habitation of the perch; but it has been observed that it inclines rather to rise towards the sources of rivers than to descend to their outlets in the ocean, and that it avoids salt waters. It is seldom found at a greater depth under water than from two to

* *Persega*, in Italian; *Peixe persio*, in Portuguese; *Perca, persico*, in Spanish; *Barsch, bersig*, in German; and *Perch* in English.

three feet, and often among the rushes and reeds in ponds, particularly at spawning time.

The habits of the perch are not very social. It does not swim in groups or flocks like other fishes, but each has its separate attraction. Its motion in swimming is by bounds or leaps; and it is often seen in still waters darting forward with great rapidity to some distance, and afterwards remaining in its customary immobility. The perch rarely leaps out of the water, and comes seldom to the surface but in warm weather to seize the gnats or their larvæ. It feeds generally on worms, insects which swim or fly on the water, the smaller Crustacea, and fishes; and as its voracity is extreme, it sometimes chooses its prey without sufficient precaution. Thus the stickleback often occasions its death, by erecting its sharp dorsal spines at the moment the perch is about to swallow it, which stick in the palate or throat. Salamanders, small vipers, and young frogs, also serve as food to the perch; and M. de Lacepede has assured Baron Cuvier that they even seize young water-rats.

The perch spawns when about six inches long and three years old, but it is not known how long a period is required for attaining its greatest size. In the environs of Paris it scarcely exceeds 15 or 18 inches in length, and rarely attains two feet. Its weight is then from three to four lbs. This remark applies to those of the lake of Geneva; but Mr Pennant relates, though not from his own knowledge, that a perch weighing nine lbs. was taken in the Serpentine river.

In the Seine the perch spawns in April, and Bloch remarks that in Brandenburgh, in shallow waters, it spawns about the same time; but as the waters are deeper the spawning season is proportionally later. The great size of the ovary or roe at this season, makes it desirable for them to disembarrass themselves of the load. In a perch of two lbs. it weighs about seven or eight ounces, and the number of ova, according to Harmer, is about 281,000, and according to M. Picot nearly a million. This difference may arise from estimates made at different ages; for the large and old fish appear to have a larger ovarium than the smaller ones, though the ova of both are of the same magnitude.

When the period of deposition has arrived, the female perch rubs herself against hard bodies; it is said that she even contrives it so that the point of a rush or reed enters the oviduct, and attaches the glairy fluid which envelopes the ova. With one point fixed thus, or to aquatic plants, she withdraws herself by sinuous movements, keeping entire the connection of the gelatinous thread, and spinning, it may be said, this thread into a long line similar to that of the ova of a frog. This line is sometimes more than six feet in length, but it is folded or laid one part of the thread above another in such a manner as to form a kind of network of little heaps or balls. When this is observed with a lens, four or five ova are always found united in one pellicle; and the little clusters or balls are so arranged, that the ova appear to be contiguous in square or hexagonal cells.

At Paris the male perch is the least numerous, and the fishermen assert that they scarcely take one male for fifty females. It is perhaps a consequence of this that many of the ova are not fecundated, and this may also serve to explain why in an animal so prolific the species is not more multiplied. But this inequality in the number of individuals of each sex is not the same everywhere. In the lake of Harlem there are so many males, that the village of Lisse is famed for a dish which is prepared from the milts of perches.

The perch is better armed against the attacks of its enemies than most of the fresh water fishes. Its spines, when it attains any considerable size, protect it from the voracity of other fishes, and when full grown even the pike dares not attack it, though the very young perches are its favourite food. Several species of water-birds, however, pursue the perch with avidity. It fears thunder, is afraid of frost and ice, and has internal enemies in intestinal worms, of which, according to Rudolphi, no less than seven species are found in the body of the perch. This fish is very tenacious of life, and Pennant asserts that it may be carried in dry straw for sixty miles without much danger. They are brought to Paris from a distance of sixty leagues by water carriage in well-boats.

It happens in certain circumstances that perches acquire a kind of protuberance or hunch, which renders them deformed.

The perches at Fahlun in Sweden, according to Linnæus, and those of a lake in Merionethshire in Wales, according to Pennant, are of this strange variety; and Sir W. W. Wynn, Bart. the proprietor of this lake, sent Baron Cuvier some individuals of this kind, which are now in the Royal Museum. This malformation Cuvier attributes to the nature of the waters they inhabit.

In the lake of Geneva during winter, when the cold hinders them from approaching the surface, it happens sometimes, when fishing at a depth of 40 or 50 fathoms, that many perches are seen floating at the surface of the water with the stomach inflated and projecting from the mouth; and these perish in a few days if this be not perforated with a needle. This is occasioned by the dilatation of the air in the swimming-vessel; but it never happens in places where the water is of less depth, and of course where the contained air cannot be so much compressed. The fishermen say, that if the fish be touched by the fishing line at this depth, they experience this revulsion of the stomach; and in truth fear may be a sufficient cause for the animal rising too rapidly to the surface. As M. Jurine remarks, at 50 fathoms the fish is under the pressure of more than eleven atmospheres; and when this weight is instantaneously removed, the air is dilated in the vessel more quickly than it can be absorbed. In this species, as in the greatest part of the Acanthopterygious fishes, there is no outlet in this vessel either towards the œsophagus or stomach.

ART. XXV.—HISTORY OF MECHANICAL INVENTIONS AND OF PROCESSES AND MATERIALS USED IN THE FINE AND USEFUL ARTS.

1. *Account of an Improved Air Pump.* By the Reverend JOHN MACVICAR, A. M. Lecturer on Natural Philosophy in the University of St Andrews. Communicated by the Author.

THE toleration of the scientific world for the many projects to improve the air-pump which prove abortive shows that a want is felt; and as I have thought over the instrument occasionally for several years, and despair of simplifying it farther, I ake the liberty of sending you a notice of the ultimate state to which I have brought it.

As soon as I became practically acquainted with the admirable working of steam engines, I thought of forming a double stroke air-pump ; and I believe, since mechanics have turned their thoughts to air-pumps and such like matters, they all wonder why such an instrument is not used. In 1823, I showed Mr Adie one which I had contrived, the mechanism of which was such, that it worked as a double stroke pump until the exhaustion was carried as far as possible in that way, and then, by turning two stop-cocks, it was worked as a single stroke pump, with a vacuum above the piston, into which the rarified air from below was forced previously to expulsion to the air. Mr Adie's opinion was, that the instrument was too complicated for use ; and I soon came to be persuaded that that gentleman was right, and moreover, that there was no occasion for such affectation towards a vacuum, so long as it cannot be denied, that though there were no barrier whatever opposed to the free expansion of the air in the receiver, it would still remain full of air of uncompressed density, and, for anything that could be affirmed to the contrary, might contain as many atoms of oxygen and nitrogen as there are stars in the Milky Way ; and that therefore experiments in Boyle's vacuum, however perfect the removal of the pressure, were always conducted in the presence of oxygen and nitrogen, unless the oxygen had been withdrawn by chemical means. The value of the air-pump, however, is so very great, that if I have the good fortune to secure your approbation for mine, I am sure you will forgive me for troubling you with this letter. The piston rod, see Plate II. Fig. 10, is hollow, and the two parts of the piston (which must be for receiving the collars of leather, which, by the way, when screwed in, ought to be turned upon a lathe,) form a capsule for the lower extremity of the tube, having an inversely conical opening cut in the region below. In this conical portion a common free conical valve (Scotticé jumping valve) plays in oil. One side of the piston and its collars of leather is drilled to receive a hollow tube with its extremities bevelled to a conical form, the collars of leather acting as a stuffing-box, and also occasioning a necessary friction for the hollow tube. The conical ends of this hollow tube are received into hollow cones in the bottom plate and top of the cylinder, (in which there is of course a conical valve and a stuffing-box,) and the conical opening in the bottom plate is merely an expansion of the aperture of the tube from the receiver. The length of the hollow tube is a small fraction of an inch less than that of the cylinder. In the figure the piston is rising. The receiver and air in the hollow tube are rushing in below the piston and getting equally rarified, while the air above the piston is getting expelled by the jumping-valve in the cover and no otherwise ; and when the piston is fairly at the top, not a bubble of air of any density ought to remain if the valve be properly made. When the piston descends, its first effect is to carry down the hollow tube by a fraction of an inch, till it is prevented from being carried farther by pressing on its negative cone in the bottom plate. This movement closes the connection of the region below the piston with the receiver, and the air escapes by the jumping-valve of the piston, and no otherwise ; and when the piston reaches the bottom, not one bubble of air ought to remain lodged anywhere. But all the while that the piston has been descending, the air has been rushing in from the receiver through the hollow tube in the piston, above the piston, and so on. These rods running through the collars of the piston work so well, that in an air-pump which had no advantage besides by the use of them, an ingenious friend of mine, since dead, preferred working his valves in this way ; and I recollect seeing in the *Annals of Philosophy*, some years ago, an air-pump proposed, in which no fewer than four such solid rods were introduced into two barrels to do the work here effected by one hollow one. I have besides seen one internal one, and one external hollow one proposed, and two internal solid ones, and have imagined all sort of things before arriving at that which is so provokingly simple compared with the others, that I had almost rather not have discovered it, than have had to accuse myself so much for stupidity in not discovering it long ago. I have thought over it occasionally for several months, and cannot simplify it ; and having shown the design to some innious practical men, they cannot find any objection to it; so am emboldened to send you a description of it.

On the evaporation of Wines, Alcohol, and other fluids by means of bladders. By M. Sœmmering of Munich.

M. Sœmmering, in a memoir in the Academy of Sciences of Munich, states that alcohol, in a vessel covered with bladder, he latter not being in contact with the fluid, loses, when exposed to a dry atmosphere, much of its water and becomes tronger. But if the vessel thus closed be exposed to a damp ir, the alcohol attracts humidity and becomes weaker.

In a second memoir the author states more particularly the ffect of bringing the alcohol into immediate contact with the nembrane. If a bladder be filled with 16 ounces of alcohol t 75°, and be well closed and suspended over a sand bath, or laced near a warm stove, so as to remain at the distance of nore than an inch from the hot surface, it becomes in a few lays reduced to a fourth of its volume, and is nearly or quite nhydrous.

M. Sœmmering prepares for this purpose calves or beeves ladders, by steeping them first in water, washing, inflating nd cleansing them from grease and other extraneous matters, ying the ureters carefully, and then returning them to the vater in order to clear off more fully the interior mucosity. After having inflated and dried the bladders, M. S. covers hem with a solution of Ichthyocolla, one coating internally nd two externally. The bladder thus becomes firmer, and he alcoholic concentration succeeds better.

It is better not to fill the bladder entirely, but to leave a mall space empty. The bladder is not moist to the touch, nd gives out no odour of alcohol. If the latter be below 16° Baume, the bladder then softens a little and appears moist to he touch.

Bladders prepared as above may be employed more than a undred times, though they at length acquire a yellowish-rown colour and become a little wrinkled and leathery. The swimming bladder of the salmon is not fit for these experiments. Alcohol of 72° was put into one of them, and after n exposure of thirty-two hours it had lost more than one-third of its volume and was weakened 12°. The alcoholic vapour was perceived by the smell.

Into two bladders of equal size was put, into one eight ounces of water, and into the other eight ounces of alcohol. They were placed side by side, exposed to a slight heat. In four days the water had entirely disappeared, while the alcohol had scarcely lost an ounce of its weight. Mineral waters and that of wells evaporate and deposit on the interior of the bladders the saline matters which they contain.

If the heat be conveniently managed, absolute alcohol may be obtained in from six to twelve hours. Solar heat even is sufficient to produce an anhydrous alcohol.

Wine placed in prepared bladders contracts no bad odour; it assumes a deep colour, acquires more aroma, and a milder taste, and becomes generally stronger. Spirits of turpentine of 75° contained in a cylindrical glass closed with a bladder, lost nothing in four years. Concentrated vinegar lost the half of its volume in four months, the other half acquired more consistency, and had no longer an acid taste. The liqueur of orange flowers, was about one-third evaporated in a few months, appeared to have a stronger odour, and consequently to have lost nothing of its volatile principle.—Ferussac's *Bulletin*, *Mai* 1828.

3. *On the employment of Iodine as a Dye.*

It appears from a note by Pelletier that he ascertained, during a recent journey in England, that a large quantity of periodurет of mercury is sold in that country under the name of English vermilion, which is employed principally in the preparation of paper hangings. Learning also that iodine was used in printing calico, he analyzed a specimen of the colouring material from Glasgow, and succeeded in forming a compound which was a perfect imitation of the English salts. The proportions which he found to succeed best were the following:

Hydriodate of potash - - -	65
Iodate of potash - - - - -	2
Ioduret of mercury - - -	33
	100

This salt appeared to have cost in England one hundred francs the kilogramme, (2 lbs. 3 oz.) but could be prepared in France for thirty-six francs, reckoning the iodine to cost forty francs.

"It appears to me (observes this skilful chemist) that this salt ought to be applied to the stuff before it is passed through metallic solutions. Among the latter, those which give the most beautiful colours are the solutions of lead and mercury. This salt may be applied with advantage to stuffs by the aid of a solution of starch which becomes a beautiful violet, (a known effect of iodine and starch.) The starch appears also to contribute to fix the salt on the stuffs.

"There is another salt also much employed, it is said, in Glasgow, in calico-printing, which I ought also to mention, because it appears not to be much used in France. This is a triple acetate of lime and copper, prepared in the large way by Ramsay, at Glasgow, for the printers. This salt is of a very beautiful blue. It crystallizes in straight prisms with square bases. The summits of the prisms are often replaced by facets, whence result prisms with six or eight planes, according to the extension which the secondary faces acquire.

When this salt is decomposed by a fixed alkali, the oxide of copper and lime are precipitated combined, because they meet in the nascent state and in definite proportions. It is certain that the precipitate turns green but little in the air, even in drying, and in its application it is a kind of *ash blue* which becomes fixed on the stuff. I call the attention of cotton printers to this salt, which may furnish very beautiful dyes, and which cannot become very expensive.—*Bulletin d'encouragement*, Sept. 1828.

4. *Account of M. Gersdorff's manufacture of Packfong.*

This substance, as analysed by M. Brewster, * is composed of 31.6 parts of nickel, 25.4 of zinc, 40.4 of copper, and 2.6 of iron. It is employed in China in the fabrication of a great number of utensils, such as vases, teapots, goblets, &c. It has the lustre, colour and sound of silver.

M. de Gersdorff, desiring to introduce into Europe so va-

* Not being able to refer to the original of this article, we cannot correct this typographical error, which is likely to be Berthier.

luable an alloy, has established at Vienna a manufactory, in which he prepares this substance in large quantities. His process is as follows:

After breaking the nickel into pieces of the size of a small nut, and dividing the copper and zinc, the three metals are mixed, and put into a crucible, in such a manner that copper may be both at bottom and top; the whole is covered with pulverized charcoal, and the crucible is heated in a wind furnace. It is necessary frequently to stir the mass, in order that the nickel, which is difficult to fuse, may combine with the other metals and the alloy be homogeneous; it must also be kept a long time in fusion, even at the risk of separating a small portion of the zinc by volatilization.

The relative proportion of the three metals which compose the packfong, should vary according to the use which is to be made of it. That destined for the fabrication of spoons, forks, &c. ought to contain 0.25 nickel, 0.25 zinc, 0.50 copper. When it is to be used in ornamenting knives, snuffers, &c. it should contain 22 nickel, 23 zinc, and 55 copper. The packfong most suitable for plating consists of 20 nickel, 20 zinc, and 55 copper. For objects which are to be soldered, as candlesticks, spurs, &c. the best alloy is one of 20 nickel, 20 zinc, 57 copper and 3 lead.

The addition of .020 to .025 of iron or steel, renders packfong much more white, but at the same time more brittle. It is necessary that the iron should be previously melted with the copper.

Packfong cannot be rolled without the greatest precaution. Every time it is passed through the rollers, it must be heated to a cherry red and slowly cooled. When the sheets present any rent, it must be hammered out before it passes again through the rollers.

The goldsmiths apply the pumice-stone to packfong, as to silver. Colour is given to it, by dipping it in a mixture of 100 parts water and 14 sulphuric acid.

When the turnings and filings of packfong are remelted, it is best to add .03 to .04 of zinc, to replace that which has been volatilized.

M. Gersdorff sells his packfong at five francs per pound; nickel being sixteen francs.—*Idem.*

ART. XXVI.—ANALYSIS OF SCIENTIFIC BOOKS AND MEMOIRS.

Specimen Geographiæ Physicæ Comparativæ. Auctore Dr JOACH. FRED. SCHOUW, in Universitate Hauniensi Botanices Prof. Cum Tab. Lithograph. 3. Hauniæ, 1828. Pp. 65.—*Specimen of Comparative Physical Geography.* By Dr JOACH. FRED. SCHOUW, Professor of Botany in the University of Copenhagen. With three Lithographic Plates. Copenhagen, 1828.

THE science of physical geography is yet, it must be admitted, in a very imperfect state. Nor is this wonderful. The vast multiplicity of objects it must embrace, even in the survey of a small district, require much patience and general information; and the attempt to classify our researches over any considerable portion of the globe, requires a comprehensiveness of mind and power of generalization, of which very few are capable.

Unlike the distinctive branches of knowledge, such as geology, botany, and meteorology, any one of which is usually considered a fit engagement for a philosophic mind, physical geography demands the union of *all*; and nothing requires more acuteness, as well as profound knowledge, than to compare different countries and climates, analytically to discriminate between the points of similarity and discrepancy, and by investigating the primary causes of each, to point out where results which are alike are produced by circumstances essentially distinct. For example, on the great ranges of the Armenian Caucasus, we have at different elevations examples of every climate, from the tropics to the pole; we have at the base the region of the tree-fern and palm, then the chestnut, the oak, the beech, the pine, the stunted shrub, the creeping lichen, and finally, the region of eternal snow. If we compare the zone of pines with our Scotch mountains, we must consider two essentially distinct conditions before a proper comparison can be instituted, the soil, and the climate. In estimating the former, we must know the geology of the two districts, and the relations of the vegetable physiology to soil in general; and in the latter, two distinct functions must be kept in view, the isothermal lines, as depending on the latitude, and the decrease of heat by altitude, varying also irregularly in different parts of the globe. This shows that even in the simplest cases great knowledge and experience are indispensable to the physical geographer, and we cannot be surprised that conclusions so important, and of such a general nature as the problems of this study unfold, should be pursued with success by few of our physical inquirers.

At the head of the list, Humboldt undoubtedly stands,—a man who is certainly a philosopher *sui generis*, the boldness and generality of whose investigations are only equalled by his own enlarged conceptions and extensive experience. His "*Personal Narrative*," with all its voluminous appendages, is an effort of a single mind, (for M. Bonpland acted a very subordinate part), which is truly surprising, nor less so the zeal of its author, who, though now past his grand climacteric, after surveying and comparing the constitution, productions, phenomena, and inhabitants of Europe and America, is now about to extend his researches to the almost unknown central regions of Asia. We are happy to observe, that the Lectures on Physical Geography by this great man, which last year were delivered at Berlin to an immense assembly of all ranks, are about to appear in the English language, and cannot fail to be a important donation to science.

In speaking thus pre-eminently of Humboldt, we would be far from desiring to exclude the various philosophers, who, by their observations or writings, have contributed to advance our acquisitions in physical geography, and Professor Schouw of Copenhagen, whose pamphlet is now before us, though best known as a botanist, bids fair to hold, by a continuation of his labours, a very high rank in that class in which Humboldt has taken the lead. M. Schouw, besides, has an opportunity of affording important information on a great district almost as little known with respect to natural history as the equinoctial regions before the publication of the "Relation Historique." Scandinavia, the vast and rugged high land of the north of Europe, will, it is to be hoped, not much longer remain unknown to the European naturalist after the extended researches of M. Schouw; and as his present paper is literally merely a "Specimen" of an extensive work, it affords us a favourable earnest of the results of his investigations. His aim is a judicious, though a limited one. He undertakes to compare the physical peculiarities of three great Alpine districts of Europe, those of Scandinavia, the Pyrenees, and the Alps, which he does in a very methodical manner under the various heads we shall presently enumerate.

We are not aware in what method the author proposes to execute his larger work, and whether this pamphlet is meant to form as it stands any integral part of it; but we are inclined to think, that, if this is the case, the plan is too formal, and the data, propositions, and corollaries of which it is in fact composed, though very distinct, are too far carried for a work of this nature. Some positions which are hypothetical are too slightly treated, and too positively ranked among the results, while others are such obvious matters of reality, that it appears trifling to methodize them in so regular a manner. These, however, are minute errors in a paper of sixty pages like this "Specimen," in which so much is included; and though in a larger work a little more generalization would be desirable, we are here the more easily enabled to lay the principal contents of it before our readers.

Our author, after his preliminary remarks, presents us with a very copious list of works which he has consulted. We reckon *twenty-nine* on Scandinavia alone, among which the names of Wahlenberg and Von Buch are the most conspicuous, and many of the others are probably known to few but the northern philosophers. The part of M. Schouw's researches contained in this fasciculus is entitled "Comparatio Alpium, Pyrenæorum, et Montium Scandinaviæ."

The first section treats of the natural limits of the three ranges; under which we need only observe, that the author does not include in the Scandinavian high land, the part of Sweden to the east of the Gulf of Bothnia, but merely the great peninsula of which the isthmus is contained between the northern extremity of that gulf and the Icy Sea. Of the latitude and longitude, the geographical position and the extension which occupy the succeeding sections, we need say nothing at present; but the sixth section enters into some interesting details upon the relative height of the three groups, and with regard to Scandinavia the measurements, though none of them seem to be original, are, we presume, but little known. The highest mountain recorded is between 7600 and 7700 French feet, two of 7100, two of 6800, one of 6400, two of 6200, one of 6000, and others less.

As the subject is curious, we may give an analysis of the results.

District.	N. Lat.	Direct.	Mean A. H.	Summits.
North Lapland,	71° —68°½	NE—SW	1000—2000	3000—4000
South ———	68½ —67	SSE—NNW	2000—3000	5000—6000
Kiölen, -	67 —63	SSE—NNW	1500—2000	3000—4000
Dovre, -	63 —62	ENE—WSW	2500—3500	5000—7000
Langfield & Sognefield,	62 —58	NNE—SSW	4000—5000	6000—7600
Filefield,			3500—4000	5000—6000
Hardangerfield,			4000—5000	5000—5200

Next follows a copious detail of the heights of the Alps, which have been collected with great pains from a variety of authorities, and the different results of observers placed together, forming a very complete detail of these interesting facts. The following is a synopsis of the heights:—

District.	Mean.	Summits.	Passes.
Between S.W. limit and Monte Viso, -	5— 7000	7—12000	3— 6000
Bet. Monte Viso and M. Blanc,	7—10000	11—13000	6— 7000
Bet. M. Blanc and M. Rosa,	10—12000	11—15000	8—10000
Bet. M. Rosa and Brenner,	8—10000	10—12000	6— 9000
Bet. Brenner and Glochner,	5— 8000	8—12000	4— 5000
Bet. Glochner and N.E. limit,	3— 6000	5—10000	3— 5000

Of the Pyrenees, the principal observations are by Reboul and Vidal, and Charpentier. The western ranges have summits at 3—8000 French feet in height; the central Pyrenees have a mean height of 7800, and an extreme of 9—11000 feet. The eastern district, 6—7000 at a mean, rising to 9—10000 feet.

From these facts the results are easily drawn. We may mention, however, M. Schouw's estimate of the ratios of extreme altitude to extension in the three ranges, which is rather remarkable:—

Pyrenees.	-	1 : 117
Alps, -	-	1 : 231
Scandinavia,	-	1 : 721

In the eighth section, our author speaks of the declivities of the ranges; but his observations amount to little more than that the mountains in Scandinavia are steepest to the north, the Alps and Pyrenees to the south. Some interesting transverse sections are given in illustration of these facts. The next section treats of the mountain ridges, and here we have the same results as in the ratios of altitude and extension. The Pyrenees are the most abrupt. Then the Alps, and the Scandinavian mountains, according to M. Schouw, can hardly be said to have ridges at all, the breadth of the summits being usually 8 to 10 miles, and the passage of the mountains occupying one or two days instead of a few hours, as in the Alps.

In speaking of the vallies, and then of the rivers, little illustration is required. Of the former, those which lie longitudinally to the range are frequent in the Alps, more rare in the Pyrenees, and in the Scandinavian range almost unknown, with one considerable exception, Elv Dal *, between the high land and the Gulf of Bothnia. Of rivers, it is needless to enter into particulars; one of the most remarkable in Lapland is the Lulea, on which are said to occur the most magnificent cataracts in the world, and which we are surprised M. Schouw does not mention. In the next section, we have a copious detail of the heights of lakes connected with the mountain ranges above the sea. We shall give some of those of the Alps, taking a mean of their heights given by different authors where they occur, instead of the round numbers of M. Schouw, as they seem to have been taken with great care. It is unfortunate that no estimate of their surfaces has been given.

Southern.	French feet.	Northern.	French feet.
Maggiore -	641	Geneva, -	1152
Varese, -	800	Neufchatel, -	1317
Lugano, -	881	Thun, -	1783
Como, -	649	Lucerne, -	1352
Guarda, -	239	Zug, -	1226
		Zurich, -	1262
		Constance, -	1089

The lakes of the higher Alps are few and small. Cenis, 6031, that on Mount Pilatus, 5625 feet. Scandinavia abounds much in lakes; three of those mentioned are above an elevation of 3000 feet, and six above 2000. on the Pyrenees they are wholly awanting on the sides of the chain, and on the summits are few and small; their altitude, however, is very great; for instance Lac de Loubassou, 6786, Lac du Mont Perdu, 7881, Lac Glacé, 8232, Lac d'Albe, 6810.

The twelfth section treats of the geognostical relations, and it is to be regretted that it is so very short. We shall, therefore, translate almost the whole of it.—" Primitive mountains occupy the central portions of the Alps and Pyrenees, and on all sides extend great districts of newer formations, both transition and secondary. The Scandinavian range is almost wholly composed of primitive mountains; the transition series is more con-

* It is worth remembering, that *dal* in Swedish means valley, as Herjedal, Dalarne, &c.; and *mark*, a plain, as Lappmarken, Tellemarken, &c.

fined than in the Alps; and the secondary wholly awanting. As to the particular rocks in Scandinavia, gneiss and mica-slate are frequent, whereas granite, primitive limestone, and clay-slate, so abundant in the Alps, are here rather uncommon. It is particularly observable, that limestone, transition, and secondary, as well as primitive, so prevalent in the Alps and Pyrenees, is of little importance in Scandinavia. The rocks in the Alps and Pyrenees are rugged and precipitous, but in Scandinavia have a rounder form, which may be accounted for either by the more slaty structure of the latter, or the greater horizontality of the strata. From the want of the secondary rocks, fossil remains are for the most part rare in Scandinavia; however, in transition districts, they are not unfrequent. Among the metals, iron, copper, and lead, in all these ranges, are abundant; all others are rather rare. In the Alps alone mercury is met with. The more metalliferous districts are the east and west portions of the Alps, (Styria, Carinthia, Savoy, and Dauphiny.) In the Pyrenees the same arrangement prevails. In Scandinavia the metals seem most abundant, especially in one valuable mineral, the magnetic iron ore. It is also worthy of notice, that thermal springs, so frequent in the Alps and Pyrenees, do not occur in the northern high land."

The subsequent section, which treats of Climate, is perhaps, the best in the work. It gives us, however, much room to regret the want of correct and authentic series of observations being carried on even at important stations. In Scandinavia the defects are so great, that we should feel diffidence in adopting all the conclusions which M. Schouw has drawn from them; and when in the various stations we find the most material circumstances unspecified which can alone render different results by different observers comparable, and when we find the whole crowned with one precious series, which we are disposed to think rather more than "satis dubia," from the following causes, "tempus brevissimum, observatores diversos, et horas non memoratas!" we gave up the idea of tabulating the results for our readers' inspection, with all the fair speciousness of two decimals of a Centigrade degree. It is not easy to believe that M. Schouw, who so much interests himself in these northern regions, and has travelled so much through their mountains and their coasts, should have been able to collect but *nine* thermometric registers of any description, and these so imperfect, that, with *two* exceptions, the hours of observation are unknown, and probably little attended to by the observers. We are surprised to see Provost Hertzburg of Ullensvang's observations among those wanting this essential postulate, that intelligent meteorologist being already known to the readers of this *Journal*. The observations made in the Alps and round their bases are more extensive and interesting; but being, we presume, all already known, and while the tables of Humboldt's isothermal lines may be referred to, it is unnecessary to insert results obtained in many important cities of Europe, and well known to most of our readers. In the Pyrenees the observations are extremely meagre, being only *four* localities from the old work of Cotte. We might have looked for some of Ramond's. To form general conclusions from the distribution of temperature from four ocalities, the altitude of two being unknown, of the third 600 feet, and the fourth 5000, would be obviously premature. It is discouraging to see how little even the most elementary branches of meteorology are extended, and how few conclusions can satisfactorily be drawn upon the simple element of temperature. We would, however, hope, that more is to be obtained in this department than M. Schouw has had access to, and that some physical geographer of more southern latitudes may use the same zeal in eliciting facts in the natural history of the Alps and Pyrenees, which our author has exercised in his almost unexplored and vast peninsula of Scandinavia. Without reporting the individual results on which M. Schouw builds his conclusions, we shall quote a few of the more established conclusions themselves.

The western division of Scandinavia is estimated to have a temperature in the same latitude, higher than the eastern by about a degree Centigrade (1.°8 Fahr.) at Stockholm and Christiania, but in some places rising to two and a half degrees. The difference of summer and winter temperature, which increases from south to north (and rises even to 30° Cent.) is much greater on the eastern side of the peninsula. These facts are easily explicable from the steady and milder temperature which the ocean washing the western shore maintains, and the exposure of the Gulf of Bothnia to the chilling blasts of the Siberian winter. 8°.1 Cent. = 14°.6 Fahr. may be taken as the range of mean annual temperature between the north and south portions of Scandinavia. The general results in the vicinity of the Alps will be pretty well anticipated. Our author gives an interesting series, showing how the difference of summer and winter temperatures decreases as we ascend higher in altitude; thus giving the heights in French feet, and the differences in Centigrade degrees.

Vienna,	480 feet,	22°.04.	Munich,	1629 feet	19°.38.
Tegernsee,	2263 —	17 .28.	Peissenberg,	3088 —	16 .11.
Gothard,	6440 —	14 .70.	Bernard,	7668 —	13 .20.

The range of mean temperature in latitude in the Alps is 6° Cent. = 10°.8 Fahr.; but the mean temperature of the highest summits being computed at —14°.9 Cent. = 5°.2 Fahr. the whole range in altitude extends to 29°.6 Cent. which in Scandinavia (where the summits are much lower, their mean temperature being as high as —9°.96 Cent.) rises only to 17°.0 Cent.

We must now give briefly M. Schouw's results on the quantity of rain falling among groups of mountains. To show the gradual increase of rain from the plains to the Alps we extract the following mean results:—

	Paris Inch.	Lin.
Lombardo-Venetian Plains near the Apennines, (8 places,)	27	7.8
Central plain of Lombardy, (7 places,) - -	36	7.3
At the bases of the Alps, and in the lower vallies, (20 places,)	54	10.8
{ To the E. of the Lago di Guarda, - - -	58	9.0
{ To the W. —— - -	39	6.0

Similar results are obtained in other approaches to the Alps:—

Mean fall of rain from lat. 43° to 44°, viz. at Toulon, Marseilles, Aix, Montpellier, Arles, Nismes, Cavaillon, and Avignon, - - - -	21	2.4

	Paris Inch.	Lin.
From lat. 44° to 46°, viz. at Orange, Viviers, Lyons, Villefranche, Bourg, and Geneva, - -	32	2.30
On the northern side: Carlsruhe, Manheim, Stuttgard, Wurtzburg, Augsburg, and Regensburg, - -	23	6.46
Zurich, Bern, Lausanne, Peissenberg, and Tegernsee, -	37	6.22
And the great St Bernard, the highest station in Europe, has an annual fall of no less than - - -	59	2.73

The fall of rain in the Alps appears to be most on the southern side, much less in the northern than the western, and least of all in an eastern direction.

There are only *four* registers of the depth of rain in Scandanavia, from which we can only deduce that much more falls on the west than on the east side. These observations are wholly wanting for the Pyrenees.

The fourteenth section is an interesting one, on the snow-line. And here we have a variety of authentic observations upon Scandinavia, many of which, we presume, are little known. By a calculation of means from the insulated observations given by Schouw, we obtain the following table:—

Latitude 59°	to 60°	5200 feet.
60	61	4747 —
61	62	5083 —
62	63	5142 —
63	64	4925 —
67		3600 —
70		3300 —
71		2200 —

The descent is not quite regular as we ascend northward, for this depends on the part of the chain on which the observations were made; the snow line being lower on the western than the eastern side by 1000 feet in lat. 67°, and 490 in 60°.* This is perhaps not exactly what we might have expected from the greater mean coldness of the eastern side, and our author does not very explicitly assign the cause. It would, however, appear to be from the greater range of temperature existing on the side most distant from the ocean, subjecting it to a high temperature in summer, which is the main instrument in the reduction of the snow line, notwithstanding the greater intensity of the winter frost. In the Alps the snow-line has been very accurately determined; on their south declivity, as on Mount Rosa, it is found to be at 9500, while on the north side of the range it is from 8500 to 8200, and in Styria, where it is lowest, at 8000. In the Pyrenees the southern may be taken at 8600, and the northern at 7800.

* We suspect a misprint in the memoir where the latter number is called 1400, which is not the difference of the heights as printed in the text, unless 4340, the height of the snow-line on the east side, is a mistake, as very possibly it may be, for 5340.

The Glaciers of course descend below the snow-line; in the Alps to 3000 feet; in southern Scandinavia to 1000; and in Lapland to the sea.

The succeeding section treats shortly of the limits of plants; whence the final limit of trees in Scandinavia descends to the northward from 2900 to 1500 feet, and similar differences are observed between the west and east sides as in the snow-line. The north and south sides of the Alps have the extreme limit of trees at 7000 and 5000 feet, in the Pyrenees from 6900 to 6500. The highest growing trees in Scandinavia are the *Betula alba*, and in the Alps and Pyrenees different kinds of the genus *Pinus. (Larix, Abies, Sylvestris, Cembra, Uncinata.)* In Scandinavia the highest limits of the *Pinus sylvestris* extends from 200 to 700 feet. The oak and beech extend on the Alps to the height of 4600–4800 feet on the south, and 4100 on the north side, and to 4900 on the south declivity of the Pyrenees. The limit of the chestnut is 2500 in the Alps, and 2800 in the Pyrenees.

In Scandinavia barley may be cultivated to 2000 feet in lat. 60° and 61°, to 800 in 67°, and in flat places at 70°. In the northern Alps, the limit of corn is 3400 feet, on the south side 4500, and the vine appears at 2500 feet, which on the north side is cultivated only in low places. In the Pyrenees, corn rises to 5200 feet on the south, and 4900 on the north exposure. M. Schouw justly remarks, that barley which is cultivated at an annual temperature of 0 Cent. in Scandinavia, cannot, from the less comparative warmth of the summer, reach maturity under that of + 5° 0 Cent. in the Alps.

Section sixteenth treats of the animals of the mountain districts. We need only mention the following as common to all. *Canis Lupus, Canis Vulpes, Felis Lynx*, and *Ursus Arctos*. Peculiar to the Alps and Pyrenees, *Antilopa rupicapra;* to the Alps only, *Capra Ibex* and *Marmota Alpina*, which live in the very highest Alps; peculiar to Scandinavia, *Cervus Tarandus, Cervus Alces, Gulo*. In domestic animals, the reindeer in Scandinavia takes the place of the ass and mule, which are awanting. The next section in due order proceeds to treat of *Man*; M. Schouw undertaking to prove the weakness of the theory, that human intellect dwindles as we advance from temperate to polar climes, and that eternal snows are agents calculated to "chill the genial current of the soul." The debate has been made one rather of mental than natural philosophy, and at all events is too important to be entered upon and concluded in half a page of wide print, as our author has done. We, therefore, decline entering on the controversy, and shall merely add, as the principal natural fact which M. Schouw states, that Cretinism, that dreadful visitation of many vallies of the Alps and Pyrenees, is unknown in Scandinavia.

A concluding section methodizes the whole results; and in closing M. Schouw's work, and in concluding our analysis, we can only strengthen our remarks at the commencement, and our hope, that, if the author is about to give to the world a larger treatise on this boundless subject, he will adopt a method somewhat less minute and tedious. His able *Essay on Ancient Climate*, and his present *Specimen* lead us to hope, that we shall be favoured with many more productions of his pen, and none we should more willingly hail, than an account of the climate and vegetation of Italy, which we observe he promises in his present pamphlet.

Art. XXVII.—PROCEEDINGS OF SOCIETIES.

1. *Proceedings of the Royal Society of Edinburgh.*

March 16, 1829.—Professor Russell in the chair.

The following communications were read :—

1. Observations on spontaneous emissions of Inflammable Gas, by R. Bald, Esq. Civil Engineer. Published in this Number, p. 71.

2. On the applicability of the Line of Continuity to created Beings, in their relations to time and space, and its *inapplicability* in reference to their relation to one another, by the Reverend Dr Fleming of Flisk.

April 6.—Dr Hope in the chair.

Bindon Blood, Esq. M. R. I. A., and M. R. D. S., was admitted an Ordinary Member.

The Keith Prize, consisting of a gold medal and a handsome piece of plate, lately adjudged to Dr Brewster for his Discovery of Two New Fluids in Gems and other Minerals, was presented to him with an appropriate address from the chair.

The following communication was read :

On the causes giving rise to the peculiar shape and figure of the land at the margin of the sea, illustrated by maps and charts, by R. Stevenson, Esq. Civil Engineer.

April 20.—Professor Russell in the chair.

The following communications were read :

1. On certain new phenomena of colour in Labrador Felspar, with observations on its changeable tints, by Dr Brewster.

2. On a new form of Cyanogen or its elements, by Mr J. F. W. Johnston, M. A.

This meeting closed the Society's 46th session. Adjourned till the general annual meeting in November next.

2. *Proceedings of the Society for the Encouragement of the Useful Arts in Scotland*

March 18, 1829.—A special general meeting of the Society was held to take into consideration the list of prizes prepared by the committee.

April 1.—1. A description and drawings of a Mangle of a new construction, invented by Mr James Brown, wright, India Street, were read and exhibited.

2. Mr Hume's Turnip Extractor and Mr Dunn's apparatus for showing the effect of steam issuing from an aperture were exhibited.

April 15.—1. A Committee was appointed to examine into the merits of Mr Brown's Mangle. Sectional drawings of the mangle were exhibited.

2. A notice of an Improved Cistern for Barometers, invented by Mr J. Adie, together with an Illustrative Sketch, were read and exhibited. From the superior mode of the adjustment of the mercury in the improved cistern, the measurement of heights, by barometrical observation, will be rendered much less liable to error.

3. A machine for producing alternate intervals of light and darkness of

any required duration, as applicable to light-house purposes, invented by ROBERT AYTOUN, Esq. S. S. A., was exhibited in operation, a description of it read, and a committee appointed to examine and report.

4. An instrument for enabling tailors to find the proper position of the "sye" or sleeve of coats, and the proper hanging of the "skirts," invented by Mr JAMES M'DONALD, tailor, West Register Street, Edinburgh, was exhibited, and a description of its nature and uses, illustrated by two drawings, was read, and the drawings presented to the Society by Mr M'DONALD. A committee was appointed to examine into the merits of the invention.

DAVID BOSWELL REID, Esq. and JOHN R. SKINNER, Esq. W. S. were elected Ordinary Members.

3. *Proceedings of the Cambridge Philosophical Society.*

March 31, 1829.—The Rev. Professor Cumming Vice-President, being in the chair,

A memoir was read by J. Challis, Esq. of Trinity College, on the vibrations of an elastic fluid, in which the author maintained that the discontinuous functions introduced into the investigations on this subject by Lagrange, were inconsistent with the analogies of mathematical reasoning, and unnecessary for the solution of the problem.

A paper by J. W. Lubbock, Esq. of Trinity College, was also read, on the comparative probabilities of life, as obtained from the recorded observations of London, Northampton, Carlisle, Chester, France, Paris, Montpellier, Holland, Amsterdam, Brussels, Breslaw; and on various other points in the calculation of such probablities, and of annuities depending upon them.

After the meeting, Professor Henslow gave an account, illustrated by a collection of coloured drawings, of the organization and classification of ferns.

May 4.—Dr Frederick Thackeray, the treasurer, being in the chair,

Some observations were made by Professor Whewell, on the systems of mineralogical classification, recently proposed by Nordenskiöld, Bonsdorff, Keferstein, and Naumann; and the preference was given to the latter, as the one which best answers the condition of establishing a correspondence between the classes founded on chemical constitution and those connected by physical resemblances.

After the meeting the Rev. Leonard Jenyns gave a description, illustrated by drawings, of the construction of feathers, their uses and the mode in which these are provided for; and the manner of their origin and growth.

May 18.—Dr F. Thackeray, the treasurer, being in the chair, a paper by W. H. MILLER, Esq. of St John's college, was read: "on the caustics produced by successive reflections at a spherical surface."

A memoir was also read by the Rev. R. WILLIS, "on the mechanism of the glottis," in which the author explained the conditions under which sound is produced by air passing between the edges of two membranes, and the manner in which the muscles of the larynx bring the organs into and out of the positions which are thus required. This communication was illustrated by various drawings, models, and apparatus, illustrating both the formation of the sound, and the means by which its pitch and quality are regulated.

May 19.—At the anniversary meeting of the Society held this day, the following officers were elected for the ensuing year:—

OFFICE BEARERS.

The Rev. Dr Turton, President.
The Rev. Prof. Farish, } Vice-Presidents.
The Rev. Prof. Sedgwick, }
The Rev. Temple Chevallier, }
Dr. F. Thackeray, Treasurer,
The Rev. Prof. Henslow, (re-elected) } Secretaries.
The Rev. Prof. Whewell, re-elected) }
The Rev. J. Lodge, re-elected. Steward of the Reading-room.

COUNCIL:

Dr. Haviland.
The Rev. H. Coddington, } Old Members.
The Rev. W. Maddy, }
The Rev. H. Farish, }
The Rev. W. L. P. Garnons, } New Members.
The Rev. J. Bowstead, }
The Rev. R. Willis, }

The Treasurer reported upon the state of the funds of the Society, and the Secretary upon the Society's proceedings for the past year.

It was announced that a new Part of the Society's Transactions was nearly ready for publication, and would shortly make its appearance.

4.—*Proceedings of the Royal Irish Academy.*

April 28, 1828.—The following gentlemen were admitted Ordinary Members:—

James Apjohn, M. D.; W. F. Montgomery, M. D.; John Hart, Esq.; Arthur Jacob, M. D.;—and Aylmer Bourke Lambert, Esq. V. P. of the Linnæan Society, an Honorary Member.

Professor Davy made some novel and interesting experiments on fulminating powders, viz. of copper, lead, &c.

May 26.—Rev. Richard Grier, D. D. admitted an Ordinary Member.

Sir William Betham exhibited two ancient seals, one found in Clonmethan, in the county of Dublin, and the other in Guisnes, France, with descriptions.

June 23.—The following gentlemen were admitted Ordinary Members:

Nicholas Edward Vigors, F. L. S. Secretary to the Zoological Society of London; Rev. Edward Geoghegan; Rev. Cæsar Otway; and Philip Cecil Crampton, M. D.

Nov. 29.—Captain Parry, R. N. admitted an Honorary Member.

The Cunningham Medal presented to John D'Alton, Esq. for his Essay on the Social and Political State of the People of Ireland from the commencement of the Christian era to the 12th century.

Feb. 23, 1829.—The following gentlemen were admitted Ordinary Members:—

Valentine Flood, M. D.; Very Rev. H. R. Dawson, Dean of St Patrick's.

March 16.—The following Members were elected council and officers for the ensuing year.

COUNCIL.

PRESIDENT.

The Lord Bishop of Cloyne.

COMMITTEE OF SCIENCE.

Archbishop of Dublin; Joseph Clarke, M. D.; Rev. Samuel Kyle, D. D. P. T. C. D.; Rev. Franc Sadleir, D. D.; Sir C. L. Gièsecke; Rev. R. MacDonnel, D. D.; Professor Hamilton.

COMMITTEE OF POLITE LITERATURE.

Rev. Jos. H. Singer, D. D.; Andrew Carmichael, Esq.; Samuel Litton, M. D.; Rev. W. Drummond, D. D.; Hon. and Rev. J. Pomeroy; Rob. J. Graves, M. D.; James Apjohn, M. D.

COMMITTEE OF ANTIQUITIES.

William Brooke, M. D.; Isaac D'Olier, LL. D; T. H. Orpen, M. D.; Hugh Ferguson, M. D.; Sir W. Betham; John D'Alton, Esq.; George Petrie, Esq.

OFFICERS.

Treasurer.—William Brooke, M. D.

Secretaries.—Rev. J. H. Singer, D. D.; Rev. Franc Sadleir, D. D.

Librarian.—Rev. W. H. Drummond.

Secretary of Foreign Correspondence.—Sir William Betham.

The following gentlemen were admitted Ordinary Members:—

Samuel O'Malley, Esq.; John Ryan, M. D.; James W. Cusack, M. D.; Francis Rynd, Esq.;—and Davies Gilbert, Esq. President of the Royal Society of London, an Honorary Member.

An ancient Brass Instrument, with a drawing of it, exhibited by the Bishop of Down, and the latter ordered to be lithographed.

April 27.—William West, M. D. admitted an Ordinary Member.

May 25.—Read part of two Prize Essays: one on the Change of Climate in Ireland, and the other on the Authenticity of the Poems of Ossian.

Professor Davy exhibited a specimen of Bromine.

ART. XXVIII.—SCIENTIFIC INTELLIGENCE.

I. NATURAL PHILOSOPHY.

ASTRONOMY.

1. *New Solar Tables, with Professor Airy and Professor Bessel's corrections.*—In consequence of the singular discordances between the place of the sun, as computed from the best Solar Tables, and its true place as actually observed and pointed out by Mr South in the *Philosophical Transactions* for 1827, the attention of various astronomers has been directed to that subject, with a view to a solution of the difficulty. Amongst these, Professor Airy and Professor Bessel have most distinguished themselves by their very laborious and minute examination of all the points that bear on the subject: and the results of this severe and rigid inquiry has led

he proposal and adoption of various corrections in the Solar Tables, ch it is presumed will lead to remove the discrepancies hitherto ob-ed. The following are some of the conclusions drawn from Mr Bessel's estigations. They are calculated for the time of mean noon at Greenh.

Mean. Long. of the Sun, January 1st 1801,	280° 39′ 13″,17
Long. of perigee - - - -	279 31 9,91
Eccentricity - - -	.0167918226
Mass of Venus - - -	$\frac{1}{401847}$
Mass of Mars - - -	$\frac{1}{2680337}$
Sidereal revolution of the Sun,	365.256374417 = 365^d 6^h 9^m 10^s 75
Tropical revolution of the Sun,	365.242220013 = 365^d 5^h 48^m 47^s 1

'he principal corrections therefore to be applied to the Solar Tables of ambre, will arise from the alteration in the epoch of the mean longi-e, and in the longitude of the perigee, the former of which is increased 65* and the latter 65″. Professor Airy makes these corrections equal - 5″.061 for the epoch, 1821.5 and + 46″.3 for the perigee: each sured from the equinoctial point adopted by Mr Pond in 1826. At the e time, he states that the greatest equation of the centre ought to be inished 90″.84, the mass of Venus reduced in the proportion of 9 to arly, and the mass of Mars in the proportion of 22 to 15 nearly. He iders the irregularity in the motion of the perigee, and of the equation he centre, as depending on a new expression which he has introduced, lving the longitudes of the Earth and of Venus, the period of which 40 years. See *Phil. Trans.* 1828, Part i. Mr Bailey's *Appendix*, p. 271.

Professor Struve's new observations on Saturn's Ring.—In No xi. 74 of this *Journal*, we have published the admirable observations of essor Struve of Dorpat, on Saturn and Jupiter. We are now enabled to efore our readers his new results, deduced from a much greater num-of observations. Their accuracy may be inferred from the slight dif-nce between the old and the new results.

r diameter of outer ring - - -	40″.095
r - - - - - -	35 .289
r inner - - - - -	34 .475
r - - - - - -	26 .668
atorial diameter of Saturn - - - -	17 .991

n which measurements we obtain the

* The proper quantity is 2″. 90; but Mr Bessel makes it 2″. 65 only because he ses to take the constant of aberration 20″. 25 instead of 20″. 00, as assumed by mbre.

Breadth of the outer ring	2″.43
——— space between	0.407
——— inner ring	3.903
Distance of the ring from Saturn	4.339
Equatorial radius of Saturn	8.995
The inclination of the ring to the plane of the ecliptic is	28° 5′ 54″

App. to Mr Bailey's Astronomical Tables, p. 272.

3. *Professor Struve's new measurements of Jupiter and his Satellites.*

Diameter of Jupiters, Equator		38″.327
	Polar axis	35.538
Compression	= 0.0728 or	$\frac{1}{13.71}$
Mean diameter of	1st satellite,	1″.015
	2d	0.911
	3d	1.488
	4th	1.277

Id. 271.

4. *Encke's Comet.*—This comet, which was first re-discovered in Great Britain on the 26th October, by Mr Dunlop, at Sir Thomas Brisbane's Observatory at Makerston, in Roxburghshire, was observed by Mr South on the 30th October, in R. Asc. 23^h 13′ and N. Decl. 25° 43′. On the 4th November, it was in R. Asc. 22^h 49′ and N. Decl. 23° 48′. On the 4th November, it was seen at Greenwich Observatory by Mr Richardson, and on the 5th at Slough by Mr Herschel.

5. *On the Constant of the Aberration of Light.*—Mr Richardson of Greenwich Observatory has found the constant of aberration to be 20″ 505, by Troughton's Circle, and 20″ 502 by Jones's Circle, from 4119 observations made during the years 1825, 6, 7, and 8.

OPTICS.

6. *Large Polyzonal Burning Lens.*—Our optical readers may remember, that we formerly proposed that a large burning lens built up of zones and segments should be constructed by the joint aid of public societies, or by individual subscriptions. This idea is now likely to be realised. Through the scientific zeal of George Swinton, Esq. and James Calder, Esq. and several other ardent friends of science at Calcutta, we have received nearly L. 150 towards defraying the expence of constructing such a lens. This sum alone is sufficient for executing a larger lens than any that has yet been made; so that there is now every probability that a splendid burning instrument will speedily be constructed in this country. This will be effected by a scientific committee, and by arrangements which will be fully described in our next Number.

MAGNETISM.

7. *Professor Hansteen's Magnetic Journey.*—Letters have been received from Professor Hansteen and his companions to the 18th February, —On the 12th of September the left Tobolsk and travelled on sledges,

the cold being at — 40° of Reaumur; so that the frozen quicksilver could be cut with a knife. On the 31st they arrived at Tomsk; on the 21st of January 1829, at Krasnojarsk; and on the 7th of February at Irkutzk, which is about 4000 versts from Tobolsk. They afterwards visited Kiachta, and crossed the frontier of China; but the most agreeable result is, that one of the desired objects of the journey is accomplished, as the observations have proved perfectly satisfactory—and the position of the magnetic pole is ascertained. Centuries may elapse before Siberia will be again so thoroughly observed. When the letters were dispatched, it was resolved that the journey should be extended to Neertschinsk, from which place Professor Hansteen would return to Krasnojarsk. His companion, Lieutenant Due, was to go alone to Jakutzk, 2,700 versts N. E. of Irkutzk, and perhaps proceed down the river Lena to the Frozen Ocean, and they intend to meet again at Jeniseisk in September or October.

ELECTRO-MAGNETISM.

8. *Law of the phenomena attributed to magnetism in motion.* By M. SAIGEY.—From a series of valuable experiments made with discs of copper, zinc, tin and lead, M. Saigey has found that their action on a magnetic needle may be thus expressed. Calling x the distance of the needle from the disc, and y the number of oscillations which it loses by the action of the disc, or the difference between the number of its oscillations while oscillating alone, and while oscillating under the influence of the disc, and a and b two constant quantities

$$y = a\,b^{1-x},$$

that is, the oscillations lost form a progression by quotients when the distances of the needle from the discs form a progression by differences.

Two numbers expressing the losses are necessary for calculating all the others, for we must determine the two constants a and b in the formula which expresses them, the first of these a indicating for example the loss at the unit of distance, and the second b the quotient of one loss divided by the following.

The constant a varies for different amplitudes in the oscillations; but the ratio b is invariable for all amplitudes.

The constants a and b increase in an inverse order not only for different metals acting on the same needle, but even for the same metal acting upon different needles.—*Annales d'Observation,* No. i. p. 48.

II. CHEMISTRY.

9. **Professor ERMAN *on the Phenomena of Liquefaction in different Bodies***

Water.	Alloy of { 2 Bismuth. 1 Lead. 1 Tin.	Phosphorus.
1. It dilates by congelation.	1. It is condensed by solidification.	1. It is condensed by solidification.
2. Its dilatability is *greater* after congelation than before it.	2. Its dilatability is sensibly *equal* before and after solidification.	2. Its dilatability is *less* after solidification than before it.

3. It attains its *minimum* of volume when *liquid*.	3. It attains its *minimum* of volume when *solid*.	3. It has *no minimum* of volume.
4. The *volumes* of the *fluid* indicated by the continuation of the progress of dilatation in the solid are *greater* than the observed results.	4. The *volumes of fluid* indicated by the continuation of the progress of dilatation in the solid are *equal* to the observed results.	4. The *volumes of the fluid* indicated by the continuation of the progress of dilatation in the solid are *less* than the observed results.

Ann. de Chim. p. 211, Feb. 1829.

III. NATURAL HISTORY.

MINERALOGY.

10. *Weissite, a new mineral species.*—This mineral is named in honour of the celebrated mineralogist, Professor Weiss of Berlin. Count Trolle-Wachtmeister of Stockholm has given this name to a mineral from the Erick-Matts mine at Fahlun, where it is found in chlorite-slate. It is massive, and has traces of a crystalline form, which seems to be prismatic. Cleavage parallel to the base of the prism, and parallel to the great diagonal. Colour grey, inclining to brown. Streak white. Lustre faint, between pearly and resinous. Scratches glass. Translucent. Specific gravity = 2.808. According to the analysis of Count Wachtmeister, the mineral consists of

Silica, - - -	53.69
Alumina, - - -	21.70
Magnesia, - -	8.99
Protoxide of iron, - -	1.43
——— manganese, -	0.63
Potash, - - -	4.10
Soda, - - -	0.68
Oxide of zinc, - -	0.30
Water with a trace of ammonia,	3.20
A trace of lime, - -	
	100.72

$$\text{Formula,}\ \left.\begin{matrix} M \\ f \\ mg \\ K \\ N \end{matrix}\right\}\ S^2 + 2\,A\,S^2 \text{ or: } r\,S^2 + 2\,R\,S^2$$

Poggendorff's *Annalen*, vol. xiii. p. 371.

11. *Analysis of the Hisingerite* or *Silicate of Iron from Riddarhyttan in Sweden.* According to Mr Hisinger, this mineral consists of

Silica, - - - -	36.30
Peroxide of iron, - -	30.63
Protoxide, - - -	13.76
Water, - - -	20.70

Formula, $f\,S^3 + F\,S + 4\,A\,q$

Poggendorff's *Annalen*, vol. xiii. 505.

12. *Analysis of the Thraulite or Silicate of Iron from Bodenmaïs in Bavaria.*—According to Dr Kobell, this mineral consists of

Silica, - - -	31.28
Peroxide of iron, - -	33.90
Protoxide, - - -	15.22
Water, - - -	19.12

Formula, $f S^2 + 3\ T S + 5\ A q$

Poggendorff's *Annalen*, vol. xiv. p. 467.

13. *Seleniuret of Silver found in Seleniuret of Lead.*—Professor G. Rose of Berlin has found in the specimens of seleniuret of lead from Tilkerode on the Harz, *seleniuret of silver.*—Poggendorff's *Annalen*, vol. xiv. p. 471.

14. *Hydrate of Silex.*—An opal found in the Graphite mine of Pfaffenreith has been found by M. Schwitz to contain an extraordinary quantity of water as an ingredient. It is of a grayish or bluish white colour. It is translucent, and when exposed to a strong light it displays a feeble opalescence. Before the blowpipe it instantly loses its transparency and decrepitates. It is composed as follows :—

Silex, - -	63.91
Water, - -	34.84
	98.75

Ferussac's *Bullet. Univers. Nov.*

15. *Quartz crystals containing Anthracite Coal and Liquids.*—The students of Rensselaer school have found many quartz crystals containing anthracite coal. There were two specimens with liquid, and one of them had a piece of coal floating in the liquid.—Silliman's *Journal*, No. 32. p. 362.

16. *Large crystal of Beryl.*—A crystal of Beryl received from Ackworth, New Hampshire, was nine inches across, and weighed forty-seven pounds. —Ib. p. 358.

17. *Anfangsgrunde der Mineralogie. Elements of Mineralogy, for the use of Students.* By William Haidinger, F. R. S. E. With 15 Copperplates. Leipzic, 1829.—This popular treatise of mineralogy is a German edition of a volume of the *Library of Useful Knowledge*, which will appear soon in London. It contains an introduction to the science, the characteristic of the newest system of Mohs, and a description of the most important mineral species. In general this little volume is one of the best works on mineralogy, and deserves to be recommended to all mineralogists who wish to study that science in a scientific manner.

18. *Handbuch der Mineralogie. Manual of Mineralogy.* By Professor Hausmann, Counsellor of his Royal Majesty the King of Great Britain and

Hanover. Second Edition, greatly enlarged. Vol. I. Gottingen, 1828.—This first volume of a very classical and important work contains the introductory part of the science, and is divided into two parts. The first treats of the external, physical, and chemical properties of minerals; and the second on the history and method in mineralogy. The succeeding volumes of the work will appear after the return of the author from a tour to London, Paris, and the Pyrenees, in the summer of 1829.

GEOLOGY.

19. *Account of the Explosion of Slickensides.* By White Watson, F.L.S.—Slickensides is a singular formation occurring in some perpendicular mineral veins, consisting of two imperceptible specular surfaces, joined together without cohesion; they are sometimes composed of a mixture of fluor, carbonate of lime, galena, blende, &c.; at others, these surfaces are thinly spread over with galena, as smooth and shining as if polished by art, and are then termed looking-glass ore: they are sometimes flat, at others waved; sometimes the waves in the same specimen are both perpendicular and horizontal; often in wedge-shaped nodular masses of various sizes, dispersed in the veins. When their edges occur in the face of the vein, on the miner striking his pick into the vein they separate, in some districts without, in others with a slight report; and in some of the mines in the neighbourhood of Eyam, in Derbyshire, with *loud* reports, particularly in Cracking-hole vein, in Haycliffe title, situated in the shell limestone, beneath the shale stratum, where in the centre of the vein, termed a shack vein, was a small white impalpable (not effervescing) powder, called a mallion, a quarter of an inch thick, which on being scratched, a loud explosion immediately ensued, before which explosion a singing kind of noise was heard. By setting a blast in the vein at a short distance from the mallion, after the blast was fired, in a few minutes an explosion took place, when a large quantity of the vein fell down. In the year 1790, a loud explosion took place from a slide joint of Slickensides going across, but not into the cheeks of the vein containing the mallion, which caused on its being stirred the loudest explosion and the largest quantity of vein materials to come down The vein there was four feet wide, and three hundred yards from a dike vein. The last great explosion was in the year 1805. It has sometimes happened that persons have been maimed, and even killed by this phenomenon; which, however, has not been noticed from Slickensides *where no shale is incumbent*

Are not these explosions occasioned by combining by friction, carbonic acid gas with the hydrogen gas, which probably descends down a vein from the shale, and which hovers in the roofs of many subjacent caverns, and which instantaneously ignites with a tremendous explosion on the approach of the flame of a candle; and instances have occurred in which they have proved fatal to human life.

20. *Date of volcanic agency in Auvergne.*—There is extant in one of the public libraries at Rome a letter from Sidonius Apollinaris, who was bishop of Clermont in Auvergne in the fifth century, (he was born in 430, and

died in 487) to Mamertus, bishop of Vienne in Dauphiny, requesting from him a copy of the form of rogations used by the latter on the irruption of the heathen hordes who entered France by that route, to avert the evils of that event; "for a more dreadful calamity had befallen parts of his diocese, from the breaking out of a creeping fire which was consuming the surface of a considerable district in those parts, particularly in Velay and the Vivarais. Dupin mentions the circumstance in his *Ecclesiastical History of the fifth century.*—*Literary Gazette,* No. 630, p. 108.

ZOOLOGY.

21. *Mammalia.*—Mr Babbage has drawn up a table, to which we direct the attention of travellers and residents in foreign countries, calculated to express in columns all the properties of Mammalia capable of indication by number. Similar tables may be easily formed, so as to include the distinctive characters of the other vertebrated animals; and where specimens cannot be transmitted home whole, a correct statement of the particulars mentioned, will enable the Zoologist to determine, with considerable precision, the zoological characters of an animal from stuffed specimens. The particulars detailed, form the titles of columns in which the dimensions &c. are expressed.

OBSERVATIONS.

Name.

Male.
- Length from tip of tail to end of nose.
- Height from ground to top of shoulder.
- Length of tail.
- Length of head.
- Greatest breadth of head.
- Weight of Animal.
- Weight of skeleton.

Female.
- Length from tip of tail to end of nose.
- Height from ground to top of shoulder.
- Length of tail.
- Length of head.
- Greatest breadth of head.
- Weight of Animal.
- Weight of skeleton.

Number of Mammæ.
Period of Gestation, in days.
Period of blindness after birth.
Period at which they cease sucking.
Period of maturity.
Period of old age.
Number of young at a birth.
Proportion of males to females.
Animal heat. Thermometer of
Number of pulsations per minute.
Number of inspirations per minute.
Number of species known.
Number of toes or claws.
Divisions of hoof.
Facial angle.
Proportion of weight of cerebrum to that of body.
Proportion of weight of cerebrum to cerebellum.
Length of intestinal canal.
Proportion of intestinal canal to length of body.
Proportion of intestinal canal to its circumference.
Nature of food.

Teeth.
- Upper jaw.
  - Grinders.
  - Canine teeth
  - Incisive.
- Lower jaw.
  - Grinders.
  - Canine teeth
  - Incisive.
- Structure of grinders.
- Total number.

Vertebræ.
- Number of Cervical.
- Number of Dorsal.
- Number of Lumbar.
- Number of Sacral.
- Number of Caudal.

Art. XXIX.—LIST OF PATENTS GRANTED IN SCOTLAN SINCE MARCH 26, 1829.

5. March 26. For certain Improvements on the Steam Engine. ' John Udny, Esq. county of Middlesex.

6. March 30. For a certain Medicine or Embrocation to prevent or al viate Sea Sickness. To Philip Derbyshire, Esq. county of Middlese

7. May 20. For an Improvement on Machinery and Apparatus for Ei broidery or Ornamenting Cloths, &c. To Henry Bock, Esq. London.

8. May 20. For an Improvement in the Construction of Made Mas To Richard Green, county of Middlesex.

9. May 20. For an Improvement in the process of making Iron. T Josias Lambert, Esq. London.

Art. XXX.—CELESTIAL PHENOMENA,

From July 1st, to November 1st, 1829. Adapted to the Meridian of Gree wich, Apparent Time, excepting the Eclipses of Jupiter's Satellites, whi are given in Mean Time.

N. B.—The day begins at noon, and the conjunctions of the Moon an Stars are given in Right Ascension.

JULY.

D.	H.	M.	S.	
1	5			☽ ☌ ☿
3	10	37	19	Im. III. Sat. ♃
4	3	15	3	☽ ☌ • ♌ ☽ 33′ N.
4	12	59	19	☽ ☌ π ♌ ☽ 64′ N.
5	3	30		♀ Inf. ☌ ☉
6	11	5	15	☽ ☌ τ ♌ ☽ 34′ S.
8	5			♀ ☌ ♂
8	18	31		☽ First Quarter.
12	10	31	17	Em. I. Sat. ♃
13	5			♀ ☌ ♄
13	14			♀ ☌ δ ♋
16				☿ Stationary.
16	2	42		○ Full Moon.
16	15	41	31	☽ ☌ β ♑ ☽ 16′ S.
16	23			♄ ☌ δ ♋
18	15			♂ ☌ δ ♋
19	0			♂ ☌ ♄
22	9	48	48	☽ ☌ ο ♓ ☽ 32′ S.
22	17			☉ enters ♌
22	18	14		☾ Last Quarter.
25				☿ ☌ ζ ♊
25	12	49	17	☽ ☌ α ♉ ☽ 47′ N.
26				☿ Greatest Elong.
27	7	30		♅ ☍ ☉
28	8	50	5	Em. I. Sat. ♃
28	13			☿ ☌ δ ♊
29				♃ Stationary.
30	5			● New Moon.
30	19			♀ ☌ α ♌
30	23	45		♄ ☌ ☉
31	9	48	49	Em. II. Sat. ♃
31	10	16	43	☽ ☌ ο ♌ ☽ 39′ N.

AUGUST.

D.	H.	M.	S.	
3	6	6	47	☽ ☌ β ♍ ☽ 70′ S.
5	1			♀ ☌ ε ♌
6	8	53	28	☽ ☌ π ♍ ☽ 8′ S.
7	10	13		☽ First Quarter.
8	9	7	28	Em. III. Sat. ♃
8	9	38	58	☽ ☌ θ ♎ ☽ 55′ N.
9	18			☿ ☌ δ ♋
11	8			☿ ☌ ♄
14	10	26		○ Full Moon.
15	13			♀ ☌ σ ♌
18	15	47		♂ ☌ ☉
19	23			♂ ☌ ♀
20	9	4	11	Em. I. Sat. ♃
20	12	15		☿ Sup. ☌ ☉
20	17			♂ α ♌
21	1	35		☾ Last Quarter.
21	11	20	33	☽ ☌ γ ♉ ☽ 62′ N.
21	12	39	31	☽ ☌ 1 δ ♉ ☽ 47′ S.
21	13	10	3	☽ ☌ 2 δ ♉ ☽ 39′ S.
21	18	21	44	☽ ☌ ♉ ☽ 37′ N.
22	5			♀ ☌ β ♍
22	23	33		☉ enters ♍
27	11	57	2	☽ ☌ ξ ♌ ☽ 10′ S.
27	16	38	51	☽ ☌ • ♌ ☽ 38′ N.
28	20	55		● New Moon.
28	21			♀ ☌ ν ♍
29	19	15		♃ □ ☉
30	12	31	52	☽ ☌ β ♍ ☽ 65′ S.
30	14			♀ ☌ σ ♌
31	10			☽ ☌ ♀

SEPTEMBER.

D.	H.	M.	S.	
1	6	48	21	☽ ☌ θ ♍ ☽ 11 S.

D.	H.	M.	S.	
4	7			☿ ☌ β ♍
4	6	23	4	☽ ☌ γ ♎ ☽ 7′ S.
5	7	23	5	Em. I. Sat. ♃
5	10	25	35	☽ ☌ φ Oph. ☽ 20′ S.
6	0	0		☽ First Quarter.
9	8			♀ ☌ ν ♍
9	12	33	11	☽ ☌ β ♑ ☽ 11′ S.
11	11	52	1	☽ ☌ θ ♒ ☽ 16′ S.
12				The Moon will be eclipsed, partly visible at Greenwich.
12	17	25		Eclipse begins.
	17	38	45	☽'s Upper Limb sets.
	18	29	30	Ecliptic Conj.
	18	37		Middle of Eclipse.
	19	49		End of Eclipse.
				6° 5′ Digits Eclipsed on the ☽'s S. Limb.
17	18	35		☽ ☌ γ ♉ ☽ 57′ N.
18	1	25	19	☽ ☌ α ♉ ☽ 32′ N.
19	12	6		☾ Last Quarter.
20	8			♂ ☌ σ ♌
21	16			☿ ☌ α ♍
22	20	17		☉ enters ♎
23	22	43	32	☽ ☌ ο ♌ ☽ 37′ N.
24	8	31	45	☽ ☌ τ ♌ ☽ 20′ S.
24	8			♀ ☌ λ ♍
26	6	44	11	Em. I. Sat. ♃
27				☉ Eclipsed Invisible.
27	14	3		● New Moon.

Times of the Planets passing the Meridian.

JULY.

	Mercury.		Venus.		Mars.		Jupiter.		Saturn.		Georgian.	
D.	h	′	h.	′	h	′	h	′	h	′	h	′
1	0	26	0	50	1	7	9	39	1	46	13	49
7	23	39	0	56	0	59	9	12	1	24	13	29
13	23	5	1	3	0	50	8	46	1	3	12	58
19	22	43	1	9	0	42	8	20	0	42	12	33
25	22	36	1	14	0	34	7	56	0	21	12	9

AUGUST.

1	22	47	1	20	0	24	7	28	23	54	11	40
7	23	8	1	25	0	16	7	5	23	34	11	16
13	23	35	1	30	0	9	6	43	23	15	10	52
19	0	0	1	34	0	1	6	22	22	55	10	29
25	0	19	1	39	23	52	6	1	22	36	10	5

SEPTEMBER.

1	0	40	1	44	23	44	5	38	22	14	9	39
7	0	55	1	49	23	37	5	19	21	55	9	17
13	1	7	1	54	23	29	5	0	21	37	8	55
19	1	17	1	59	23	22	4	42	21	18	8	33
25	1	25	2	5	23	15	4	24	20	59	8	11

Declination of the Planets.

JULY.

	Mercury.		Venus.		Mars.		Jupiter.		Saturn.		Georgian.	
D.	°	′	°	′	°	′	°	′	°	′	°	′
1	18	30N.	23	0N.	22	18N.	20	48 S.	19	48N.	19	33 S.
7	18	7	21	51	21	39	20	44	19	37	19	36
13	18	30	20	18	20	44	20	41	19	27	19	39
19	19	28	18	25	19	49	20	39	19	16	19	42
25	20	36	16	14	18	51	20	38	19	5	19	45

AUGUST.

1	21	19N.	13	22N.	17	34N.	20	39 S.	18	51N.	19	50 S.
7	20	41	10	40	16	25	20	41	18	40	19	54
13	18	30	7	48	15	12	20	44	18	28	19	58
19	14	59	4	49	13	55	20	48	18	16	20	0
25	10	42	1	45N.	12	36	20	53	18	5	20	3

SEPTEMBER.

1	5	19N.	1	51 S.	10	59N.	20	59 S.	17	[illegible]	20	[illegible]
7	0	42N.	4	57	9	33	21	6	17	40	[illegible]	8
13	3	44 S.	7	59	8	6	21	14	17	29	20	[illegible]
19	7	52	10	56	6	36	21	22	17	18	20	4
25	11	38	13	44	5	6	21	30	17	8	20	13

The preceding numbers will enable any person to find the positions of the planets, to lay them down upon a celestial globe, and to determine their times of rising and setting.

Art. XXXI.—*Summary of Meteorological Observations made at Kendal in March, April, and May* 1829. By Mr Samuel Marshall. Communicated by the Author.

State of the Barometer, Thermometer, &c. in Kendal for March 1829.

	Inches.
Barometer.	
Maximum on the 3d, - - -	30.23
Minimum on the 20th - - - -	29.13
Mean height, - - - -	29.72
Thermometer.	
Maximum on the 20th, - - -	52.5°
Minimum on the 16th, - - -	19.5°
Mean height, - - - -	38.34°

Quantity of rain, 0.867 inches.
Number of rainy days, 4.
Prevalent wind, north-east.

This has proved another remarkably dry month. Indeed the contrast of the three months now past in this year, and the first three months of last year, will be striking when it is stated that so far in the present year we have had but 2.848 inches of rain, and 18 rainy days. In January, February, and March last year, we had 13.257 inches of rain, and 50 rainy days. The mean temperature of these three months in the present year is much less than in the last, being in 1828, 40.06°, but in 1829 only 36.20°. In consequence, vegetation generally is several weeks behind what it was at the end of March last year. Though the evenings have been generally clear during the month, the Aurora Borealis has not been noticed. We had violent gusts of wind on the evening of the 20th, a specimen of the equinoctial gales. The weather on the whole has been very pleasant, though we have had a long continuance of dry winds, which usually proceed from the E. and N. E. from which quarters the wind has mostly blown in the day time the greater part of the month.

April.

	Inches.
Barometer.	
Maximum on the 26th, - - -	30.04
Minimum on the 15th, - - -	28.58
Mean height, - - - -	29.34

Thermometer.

Maximum on the 18th, - - -	55.5°
Minimum on the 2d, - - - -	25.5°
Mean height, - - - -	42.77°

Quantity of rain, 3.511 inches.
Number of rainy days, 18.
Prevalent wind, west.

The prevalence of the dry N. E. winds is one of the usual characteristics of this month. During the latter part they have been very frequent both in the day time and the night. The west winds towards the beginning and in the middle of the month were more prevalent than the easterly ones. On the 28th we had a strong gale of wind accompanied with hail, rain, &c. and all the hills in the neighbourhood were capped with snow. The month on the whole has been a cold one, occasioned chiefly by the N. E. winds. These have a tendency from their dryness and coldness to retard the progress of vegetation, which is backward. We have had occasionally sudden squalls of wind, and they have invariably had the effect of lowering the barometer, the mean of which is much less than has been the case for many months. The quantity of rain is still very much below the usual amount, as we have had but 6.359 for the four months of this year, whereas in last year we had 17.269 inches in the same time.

May.

Barometer.	Inches.
Maximum on the 26th, - - -	30.42
Minimum on the 2d and 3d, - -	29.34
Mean height, - - - -	29.89

Thermometer.

Maximum on the 29th, - - -	71°
Minimum on the 26th, - - - -	36°
Mean height, - - - -	53.26°

Quantity of rain, 1.977 inches.
Number of rainy days, 9.
Prevalent wind, west.

This has proved a dry month, except for the first eight days, since which time we have had no rain except a slight shower on the 14th, and a few drops scarcely perceptible on the evenings of the 23d and 31st. The total quantity for this year is no more than 8.336 inches. The season has been a drier one than any other in the seven preceding years. The smallest quantity taken in the first five months of the year during that period was in 1824, which amounted to 16.173 inches, or nearly double that of the present year, during the same space of time. The barometer has been high most of the month, and has fluctuated little. The mean temperature is 53.26°, and this probably would have been much greater had it not been for the dry and cold winds from the east. It has frequently been difficult to decide which might be called the prevalent wind for the day, as it has generally been variable in the day time, especially in the latter part of the month.

Art. XXXII.—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 1½ mile to the south of Edinburgh Castle, about 3 miles from the sea at Leith, and about ¼ 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.

MARCH 1829.

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.
S.	1	41	33	37	32	45	38.5	30.01	30.08	
M.	2	35	32	33.5	25	42	33.5	30.18	30.23	
T.	3	41	39	40	29	46	37.5	30.21	30.15	
W.	4	42	39	40.5	37	46	41.5	30.17	30.13	
T.	5	43	41	42	36	46	41	30.12	30.08	
F.	6	43	41	42	37	47	42	29.98	29.88	
S.	7	43	42	42.5	37	50	43.5	29.87	29.83	
S.	8	43	42	42.5	40	51	45.5	29.80	29.75	
M.	9	46	35	40.5	38	50	44	29.66	29.72	
T.	10	41	37	39	30	44	37	29.75	29.73	
W.	11	41	35	38	33	45	39	29.68	29.68	
T.	12	41	34	37.5	31	49	40	29.55	29.61	
F.	13	35	32	33.5	27	36	31.5	29.62	29.62	
S.	14	35	29	32	25	44	34.5	29.66	29.69	
S.	15	32	31	31.5	21	39	30	29.72	29.52	
M.	16	38	36	37	30	41	35.5	29.35	29.19	
T.	17	38	37	37.5	32	45	38.5	28.97	29.20	
W.	18	43	49	46	34	49	41.5	29.20	29.14	.17
T.	19	50	49	49.5	44	57	50.5	29.10	28.99	.06
F.	20	52	40	46	45	57	51	28.86	28.99	
S.	21	48	37	42.5	35	55	45	28.92	28.88	
S.	22	46	40	43	29	49	39	28.76	28.73	
M.	23	43	32	37.5	37	45	41	28.87	28.89	
T.	24	37	30	33.5	28	42	35	28.84	28.78	
W.	25	43	40	41.5	25	48	36.5	28.80	28.80	
T.	26	43	39	41	37	46	41.5	28.81	28.75	
F.	27	44	37	40.5	37	46	41.5	28.70	28.59	
S.	28	43	38	40.5	36	47	41.5	28.54	28.42	
S.	29	42	37	39.5	33	44	38.5	29.33	29.22	
M.	30	39	35	37	35	42	38.5	29.16	29.16	.09
T.	31	35	30	32.5	32	40	36	29.13	29.16	

APRIL 1829.

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.
W.	1	40	34	37	27	40	33.5	29.19	29.23	.22
T.	2	41	33	37	31	45	38	29.27	29.25	
F.	3	43	34	38.5	29	50	39.5	29.28	29.28	
S.	4	45	35	40	32	50	41	29.29	29.26	
S.	5	45	41	43	31	49	40	29.00	28.93	.16
M.	6	40	38	39	38	48	43	28.91	28.92	
T.	7	41	39	40	35	46	40.5	28.96	29.03	.02
W.	8	41	36	38.5	35	45	40	29.10	29.18	.17
T.	9	43	34	38.5	31	45	38	29.22	29.13	
F.	10	36	33	34.5	33	39	36	29.10	29.24	1.18
S.	11	38	37	37.5	35	44	39.5	29.31	29.24	.45
S.	12	42	44	43	35	54	44.5	28.76	28.65	.18
M.	13	48	41	44.5	40	51	45.5	28.48	28.80	
T.	14	50	43	46.5	37	54	45.5	28.85	28.63	.21
W.	15	48	43	45.5	42	52	47	28.44	28.71	
T.	16	45	36	40.5	38	52	45	28.92	29.13	.13
F.	17	50	46	48	31	56	43.5	29.33	29.46	
S.	18	50	43	46.5	36	56	46	29.46	29.31	.03
S.	19	45	41	43	38	50	44	29.26	29.30	.14
M.	20	48	41	44.5	38	54	46	29.57	29.61	.14
T.	21	48	39	43.5	34	52	43	29.62	29.61	
W.	22	43	40	41.5	38	51	44.5	29.55	29.58	
T.	23	43	40	41.5	37	44	40.5	29.68	29.73	
F.	24	43	40	41.5	37	45	41	29.80	29.86	
S.	25	45	39	42	36	48	42	29.93	29.99	.07
S.	26	48	40	44	37	52	44.5	29.90	29.45	.06
M.	27	48	38	43	36	52	44	29.40	29.48	.11
T.	28	46	35	40.5	34	51	42.5	28.90	29.48	
W.	29	41	35	38	35	46	40.5	29.65	29.73	.03
T.	30	46	47	46.5	30	47	38.5	29.50	29.21	.05

MAY 1829.

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	48	40	44	40	52	46	29.17	29.18	
S.	2	48	40	44	36	54	45	29.12	29.21	.03
S.	3	56	41	48.5	38	59	48.5	29.03	29.20	.11
M.	4	48	41	44.5	37	54	45.5	29.55	29.65	.12
T.	5	48	52	50	39	58	48.5	29.45	29.32	
W.	6	57	47	52	45	51	48	29.40	29.50	.11
T.	7	62	49	55.5	44	58	51	29.56	29.78	.13
F.	8	56	51	53.5	47	57	52	29.74	29.72	
S.	9	57	50	53.5	48	64	56	29.75	29.75	.03
S.	10	60	49	54.5	46	66	56	29.87	29.92	
M.	11	56	43	49.5	44	63	53.5	29.94	29.93	
T.	12	49	45	47	42	57	49.5	29.92	29.80	
W.	13	57	54	55.5	36	67	51.5	29.72	29.63	
T.	14	50	43	46.5	43	58	50.5	29.63	29.70	.15
F.	15	56	47	51.5	36	62	49	29.76	29.78	
S.	16	60	50	55	44	63	53.5	29.87	29.88	
S.	17	58	48	53	46	63	54.5	29.87	29.86	
M.	18	60	51	55.5	40	62	51	29.79	29.72	
T.	19	58	48	53	47	66	56.5	29.79	29.91	
W.	20	58	48	53	44	61	52.5	30.03	30.08	
T.	21	49	39	44	40	59	49.5	30.04	30.00	
F.	22	53	48	50.5	34	59	46.5	29.94	29.93	
S.	23	69	55	62	44	70	57	29.87	29.66	
S.	24	55	46	50.5	47	65	56	29.94	30.25	
M.	25	55	45	50	41	67	54	30.42	30.44	
T.	26	54	43	48.5	37	59	48	30.44	30.33	
W.	27	54	47	50.5	38	60	49	30.22	30.18	
T.	28	58	52	55	44	66	55	30.11	30.07	
F.	29	60	49	54.5	49	70	59.5	30.06	30.06	
S.	30	59	49	54	45	59	52	30.05	29.99	
S.	31	59	53	56	48	64	56	29.95	29.96	

THE

EDINBURGH

JOURNAL OF SCIENCE.

ART. I.—*Historical Eloge of the Marquis De Laplace.** By M. LE BARON FOURIER.

THE name of Laplace has been heard in every part of the world where the sciences are honoured; but his memory could not receive a more worthy homage than the unanimous tribute of the admiration and sorrow of that illustrious body who shared in his labours and in his glory. He consecrated his life to the study of the grandest objects which can occupy the human mind.

The wonders of the heavens,—the lofty questions of natural philosophy,—the ingenious and profound combinations of mathematical analysis,—all the laws of the universe have been presented to his thoughts during more than sixty years, and his efforts have been crowned with immortal discoveries.

From the time of his first studies it was remarked that he possessed a prodigious memory: all the occupations of the mind were easy to him. He acquired rapidly a very extensive knowledge of the ancient languages, and he cultivated different branches of literature.—Every thing interests rising genius. Every thing is capable of revealing it. His earliest success was in theological studies; and he treated with talent

* Pronounced at the public sitting of the Royal Academy of Sciences on the 15th June 1829.

and with extraordinary sagacity the most difficult controversial questions.

We do not know by what fortunate event Laplace passed from the study of scholastics to that of the higher geometry. This last science, which scarcely admits of a divided attention, attracted and fixed his thoughts. Henceforth he abandoned himself without reserve to the impulse of his genius, and he was impressed with the conviction, that a residence in the capital had now become necessary. D'Alembert was then in the zenith of his fame. It was he who informed the court of Turin that its Royal Academy possessed a geometer of the first order—Lagrange, who, without this noble testimony to his merits, might have remained long unknown. D'Alembert had announced to the King of Prussia that there was only one man in Europe who could replace at Berlin the illustrious Euler, who, having been recalled by the Russian government, had consented to return to St Petersburg. I find in the unpublished letters possessed by the Institute of France the details of this glorious negociation, which fixed the residence of Lagrange at Berlin.

It was about the same time that Laplace began that long career which was destined to become so illustrious.

He waited upon D'Alembert, preceded by numerous recommendations, which might have been considered as very powerful. But his attempts were vain, for he was not even introduced. He then addressed to him whose suffrage he solicited a very remarkable letter on the general principles of mechanics, of which M. Laplace has frequently quoted to me different fragments. It was impossible that a geometer like D'Alembert could fail to be struck with the singular profoundness of this composition. On the same day, he invited the author of the letter, and thus addressed him :—" You see, Sir, that I hold recommendations as of very little value ;—you have no occasion for them. You have made yourself better known ;—this is sufficient for me: You are entitled to my support." In a few days he succeeded in getting Laplace nominated Professor of Mathematics in the Military School of Paris. From that moment, devoted wholly to the science which he had chosen, he gave to all his labours a fixed direction, from which he never deviated; for the unchangeable purpose of his mind has always been the principal feature of his genius. He already trenched upon the known limits of mathematical analysis;—he was versed in the most ingenious and powerful parts of this science; and there was none more capable than he of extending its domains. He had solved a leading question in theoretical astronomy. He formed the project of consecrating his efforts to this sublime science;—he was destined to perfect it, and was able to embrace it in all its extent. He thought deeply upon his glorious purpose; and he spent his whole life in accomplishing it, with a perseverance of which the history of the sciences presents perhaps no other example.

The immensity of the subject flattered the just pride of his genius. He undertook to compose the Almagest of his age. This memorial he has left us under the name of the *Mécanique Céleste;* and his immortal work surpasses that of Ptolemy as much as the modern analysis surpasses the *Elements of Euclid.*

Time, which is the only just dispenser of literary glory, and which sinks into oblivion contemporary mediocrity, perpetuates also the remembrance of great works. They alone convey to posterity the character of each succeeding age. The name of Laplace will thus live for ever;—but, I hasten to add, that enlightened and impartial history will never separate his memory from that of the other successors of Newton. It will conjoin the illustrious names of D'Alembert, Clairaut, Euler, Lagrange, and Laplace. I confine myself at present to the mere mention of the great geometers whom the sciences have lost, and whose researches had for their common object the perfection of physical astronomy.

In order to give a just idea of their works, it would be necessary to compare them; but the limits of a discourse like this oblige me to reserve a part of this discussion for the collection of our Memoirs.

Next to Euler, Lagrange contributed most to the foundation of mathematical analysis. In the writings of these two great geometers it has become a distinct science, the only one of the mathematical theories of which we can say that it is completely and rigorously demonstrated. Among all these theories, it alone is sufficient for its own purposes, while it illustrates all the rest; and it is so necessary to them, that without its aid they must have remained very imperfect.

Lagrange was destined to invent and to extend all the sciences of calculation. In whatever condition fortune had placed him, whether prince or peasant, he would have been a great geometer. This he would have become necessarily and without any effort—which cannot be said even of the most celebrated individuals who have excelled in this science.

If Lagrange had been the contemporary of Archimedes and Conon, he would have divided with them the glory of their most memorable discoveries. At Alexandria he would have been the rival of Diophantus.

The distinctive mark of his genius consists in the unity and grandeur of his views. He attached himself wholly to a simple though just and highly elevated thought. His principal work, the *Mécanique Analytique*, might be called Philosophical Mechanics, for it refers all the laws of equilibrium and motion to a single principle; and, what is not less admirable, it submits them to a single method of calculation of which he himself was the inventor. All his mathematical compositions are remarkable by their singular elegance, by symmetry of form, and generality of method, and, if we may so express it, by the perfection of his analytical style.

Lagrange was no less a philosopher than a great geometer. He has proved this in the whole course of his life, by the moderation of his desires, by his immoveable attachment to the general interests of humanity, by the noble simplicity of his manners, and the elevation of his character, and by the justness and profoundness of his scientific labours.

Laplace had received from nature all that force of genius which a great enterprise required. Not only has he united in his Almagest of the eighteenth century all that the mathematical and physical sciences had already invented, and which formed the foundation of astronomy, but he has added to this science capital discoveries of his own which had escaped all his predecessors. He has resolved, either by his own methods or by those of which Euler and Lagrange had pointed out the principles, questions the most important, and certainly the most difficult of all those which had been considered before his time. His perseverance triumphed over every obstacle. When his first efforts were not successful, he renewed them under the most ingenious and diversified forms.

In the motions of the moon, for example, there had been observed an acceleration, the cause of which philosophers were unable to discover. It had been ascribed to the resistance of an ethereal medium in which the celestial bodies moved. If this had been the case, the same cause affecting the orbits of the planets would have tended continually to disturb their primitive harmony. These stars would have been constantly disturbed in their course, and would have finally been precipitated upon the mass of the sun. It would have required the creating power to have been exerted anew in preventing or repairing the immense disorder which the lapse of time would have caused.

This cosmological question is undoubtedly the greatest which human intelligence can propose: It is now resolved. The first researches of Laplace on the immutability of the dimensions of the solar system, and his explanation of the secular equation of the moon, have led to this solution.

He at first inquired if the acceleration of the moon's motion could be explained by supposing that the action of gravity was not instantaneous, but subject to a successive transmission like that of light. By this means he succeeded in discovering its true cause. A new investigation then gave a better direction to his genius. On the 19th March 1787, he communicated to the Academy of Sciences a precise and unexpected solution of this great difficulty. He proved in the clearest manner that the observed acceleration is a necessary effect of universal gravitation.

This great discovery threw a new light on the most important points of the system of the world. The same theory, indeed, proved to him, that, if the action of gravitation on the stars was not instantaneous, we must suppose that it propagates itself more than fifty millions of times faster than light, whose velocity is well known to be 70,000 leagues in a second.

Hence he concluded from his theory of the lunar motions, that the medium in which the stars revolve does not oppose any sensible resistance to the motions of the planets; for this cause would particularly affect the motion of the moon, whereas it produces no perceptible effect.

The discussion of the motions of this planet is pregnant with remarkable consequences. We may conclude from it, for example, that the motion of rotation of the earth about its axis is invariable. The length of the day has not varied the 100th part of a second for 2000 years. It is remarkable that an astronomer need not go out of his observatory to measure the distance of the earth from the sun. It would be sufficient to observe carefully the variations of the lunar motions, and from this he would deduce with certainty the distance required.

A still more striking consequence is that which relates to the figure of the earth; for the form even of the terrestrial globe is impressed on certain inequalities of the lunar orbit. These inequalities would not have taken place if the earth had been a perfect sphere. We may determine the compression at the poles of the globe by the observation of the lunar motions alone, and the results hence deduced agree with the real measures which have been obtained by the great trigonometrical surveys at the equator, in the northern regions, in India, and in different countries.

It is to Laplace that we especially owe this astonishing perfection of modern theories.

I cannot undertake to recount at present the series of his labours, and the discoveries to which they have led. The simple enumeration of them, however rapid it may be, would exceed the limits which I am obliged to prescribe to myself. Beside these researches on the secular equation of the moon, and the no less important and difficult discovery of the cause of the great inequalities of Jupiter and Saturn, we may mention those admirable theorems on the libration of the satellites of Jupiter. To these we may add his analytical inquiries respecting the tides,—a subject which he has pursued to an immense extent.

There is scarcely a point of physical astronomy of any importance that he did not study with the most profound attention; and he submitted to calculation most of the physical conditions which his predecessors had omitted. In the question already so complex of the form and rotatory motion of the earth, he has considered the influence of the waters distributed between the continents, the compression of the interior strata, and the secular diminution of the dimensions of the globe.

Among all these researches we must particularly distinguish those which relate to the stability of great phenomena; for no object is more worthy of the meditation of philosophers. Hence it follows that those causes, either accidental or constant, which disturb the equilibrium of the ocean, are subject to limits which cannot be passed. The specific gravity of the sea being much less than that of the solid globe, it follows that the oscillations of the ocean are always comprehended between very narrow limits; which would not have happened if the fluid spread over the globe had been much heavier. Nature in general keeps in reserve conservative forces which are always present, and act the instant the disturbance commences, and with a force increasing with the necessity of calling in their assistance. This preservative power is found in every part of the universe. The form of the great planetary orbits, and their inclinations, vary in the course of ages, but these changes have their limits. The principal dimensions subsist, and this immense assemblage of celestial bodies oscillates round a mean condition of the system, towards which it is always drawn back. Every thing is arranged for order, perpetuity, and harmony.

In the primitive and liquid state of the terrestrial globe, the heaviest materials are placed near the centre, and this condition determines the stability of seas.

Whatever may be the physical cause of the formation of the planets, it has impressed on all these bodies a projectile motion in one direction round an immense globe; and from this the solar system derives its stability. Order is here kept up by the power of the central mass. It is not, therefore, left, as Newton himself and Euler had conjectured, to an adventitious force to repair or prevent the disturbance which time may have caused. It is the law of gravitation itself which regulates all things, which is sufficient for all things, and which everywhere maintains variety and order. Having once emanated from supreme wisdom, it presides from the beginning of time, and renders impossible every kind of disorder. Newton and Euler were not acquainted with all the perfections of the universe.

Whenever any doubt has been raised respecting the accuracy of the Newtonian law, and whenever any foreign cause has been proposed to explain apparent irregularities, the original law has always been verified after the most profound examination. The more accurate that astronomical observations have become the more conformable have they been to theory. Of all geometers Laplace is the one who has examined most profoundly these great questions.

We cannot affirm that it was his destiny to create a science entirely new, like Galileo and Archimedes; to give to mathematical doctrines principles original and of immense extent like Descartes, Newton, and Leibnitz; or, like Newton, to be the first to transport himself into the heavens, and to extend to all the universe the terrestrial dynamics of Galileo: but Laplace was born to perfect every thing, to exhaust every thing, and to drive back every limit, in order to solve what might have appeared incapable of solution. He would have completed the science of the heavens if that science could have been completed.

The same character appears in his researches on the analysis of probabilities,—a science quite modern and of immense extent, whose object, often misunderstood, has given rise to the most erroneous interpretations, but whose application will one day embrace every department of human knowledge—a fortunate supplement to the imperfection of our nature.

This art originated from a fine and fertile idea of Pascal's: It was cultivated from its origin by Fermat and Huygens. A philosophical geometer, James Bernouilli, was its principal founder. A singularly happy discovery of Stirling, the researches of Euler, and particularly an ingenious and important idea due to Lagrange, have perfected this doctrine: It has been illustrated by the objections even of D'Alembert, and by the philosophical views of Condorcet: Laplace has united and fixed the principles of it. In his hands it has become a new science, submitted to a single analytical method, and of prodigious extent. Fertile in useful applications, it will one day throw a brilliant light over all the branches of natural philosophy. If we may here be permitted to express a personal opinion, we may add, that the solution of one of the principal questions, that which the illustrious author has treated in the 18th chapter of his work, does not appear to us exact; but; taken all in all, this work is one of the most precious monuments of his genius.

After having mentioned such brilliant discoveries, it would be useless to add, that Laplace belonged to all the great academies of Europe.

I might also, and perhaps ought to, mention the high political dignities with which he was invested; but such an enumeration would only have an indirect reference to the object of this discourse. It is the great geometer whose memory we now celebrate. We have separated the immortal author of the *Mécanique Celeste* from all accidental facts which concern neither his glory nor his genius. Of what importance indeed is it to posterity, who will have so many other details to forget, to learn whether or not Laplace was for a short time the minister of a great nation. What is of importance are the eternal truths which he discovered;—the immutable laws of the stability of the world, and not the rank which he occupied for a few years in the conservative senate.—What is of importance, and perhaps still more so even than his discoveries, is the example which he has left to all those who love the sciences, and the recollection of that incomparable perseverance which has sustained, directed, and crowned so many glorious efforts.

I shall omit, therefore, all the accidental circumstances and peculiarities which have no connection with the perfection of his works. But I will mention, that in the first body in the state the memory of Laplace was celebrated by an eloquent and friendly voice, which important services rendered to the historical sciences, to literature, and to the state, have for a long time illustrated.*

I shall particularly mention that literary solemnity which attracts the attention of the capital. The French Academy, uniting its suffrages to the acclamations of the country, consi-

* M. Le Marquis Pastoret.

dered that it would acquire a new glory by crowning * the triumphs of eloquence and of political virtue.

At the same time it chose to reply to the successor of Laplace, an illustrious academician,† with more than one claim, who united in literature, in history, and in the public administration, every species of talent. ‡

Laplace enjoyed an advantage which fortune does not always grant to great men. From his earliest youth he was justly appreciated by his illustrious friends. We have now before us unpublished letters, which exhibit all the zeal of D'Alembert to introduce him into the Military School of France, and to prepare for him, if it had been necessary, a better establishment at Berlin. The president Bochard de Saron caused his first works to be printed. All the testimonies of friendship which have been given to him recal great labours and great discoveries; but nothing could contribute more to the progress of the physical sciences than his relations with the illustrious Lavoisier, whose name, consecrated in the history of science, has become an eternal object of our sorrow and esteem.

These two celebrated men united their efforts. They undertook and finished very extensive researches in order to measure one of the most important elements of the physical theory of heat. About the same time, they also made a long series of experiments on the dilatation of solid substances. The works of Newton sufficiently show us the value which this great geometer attaches to the special study of the physical sciences. Laplace is of all his successors the one who has made the greatest use of his experimental method; he was almost as great a natural philosopher as he was a geometer. His researches on refractions, on capillary attraction, on barometrical measure-

* M. Royer-Collard. † M. Le Comte Daru.

‡ M. Royer-Collard was unanimously elected to succeed Laplace in the French Academy, and on the occasion of his admission delivered a very eloquent oration. To that oration M. Le Compte Daru made an able reply, according to the custom of the Academy. A report of their orations will be found in *Le Globe*, Nov. 15, 1827. If this report is correct, M. Royer-Collard has committed a strange oversight in speaking of the *Systeme du Monde* as the great work of Laplace. The *Mecanique Celeste* is never once mentioned.—Ed.

ments, on the statical properties of electricity, on the velocity of sound, on molecular action, and on the properties of gases, testify that there was nothing in the investigation of nature to which he was a stranger. He was particularly anxious about the perfection of instruments, and he caused to be constructed at his own expence, by a celebrated artist, a very valuable astronomical instrument, which he gave to the Observatory of France.

All kinds of phenomena were perfectly well known to him. He was connected by an old friendship with two celebrated chemists, whose discoveries have extended the boundaries of the arts and of chemical theory. History will unite the names of Berthollet and Chaptal to that of Laplace. It was his happiness to reunite them; and their meetings always had for their object and for their results the increase of those branches of knowledge, which are the most important and the most difficult to acquire.

The gardens of Berthollet at his house at Arcueil were not separated from those of Laplace. Great recollections and great sorrows have rendered this spot illustrious. It was there that Laplace received celebrated foreigners, men of powerful minds, from whom science had either obtained or expected some benefit, but especially those whom a sincere zeal attached to the sanctuary of the sciences. The one had begun their career,—the others were about to finish it. He received them with extreme politeness: He went even so far that he led those who did not know the extent of his genius, to believe that he might himself draw some advantage from their conversation.

In alluding to the mathematical works of Laplace, we have particularly noticed the depth of his researches, and the importance of his discoveries: But his works are distinguished also by another character which all readers have appreciated,—I mean the literary merit of his compositions. That which is entitled the *Systeme du Monde* is remarkable for the elegant simplicity of its style, and the purity of its language. There had previously been no example of this kind of composition; but we should form a very incorrect idea of the work, were we to expect to acquire a knowledge of the phenomena of the heavens in such productions. The suppression of the symbols of the language of calculation cannot contribute to its perspicuity, and render the perusal of it more easy. The work is a perfectly regular exposition of the results of profound study: It is an ingenious epitome of the principal discoveries. The precision of its style, the choice of methods, the greatness of the subject, give a singular interest to this vast picture; but its real utility is to recal to geometers those theorems whose demonstrations were already known to them. It is properly speaking the contents of a mathematical treatise.

The purely historical works of Laplace have a different object. They present to geometers with admirable talent the progress of the human mind in the invention of the sciences. The most abstract theories have indeed an innate beauty of expression. It is this which strikes us in several of the treatises of Descartes, and in some of the pages of Galileo, of Newton, and Lagrange. Novelty of views, elevation of thought, and their connection with the grand objects of nature, fix the attention and fill the mind. It is sufficient that the style be pure, and have a noble simplicity. It is this kind of literature that Laplace has chosen, and it is certain that he has attained in it the first rank. If he writes the history of great astronomical discoveries, he becomes a model of elegance and precision. No leading fact ever escapes him: the expression is never obscure or ambiguous. Whatever he calls great is great in reality. Whatever he omits does not deserve to be cited.

M. Laplace retained to a very advanced age that extraordinary memory which he had exhibited from his earliest years; a precious gift, which, though it is not genius, is that which serves to acquire and preserve it. He had not cultivated the fine arts, but he appreciated them. He was fond of Italian music and of the poetry of Racine, and he often took delight in quoting from memory different passages of this great poet. The works of Raphael adorned his apartments, and they were found beside the portraits of Descartes, Francis Vieta, Newton, Galileo and Euler.

Laplace had always accustomed himself to a very light diet, and he diminished the quantity of it continually, and even to an excessive degree. His very delicate sight required constant care, and he succeeded in preserving it without any alteration. These cares about himself had only one object, that of reserving all his time and all his strength for the labours of his mind. He lived for the sciences, and the sciences have rendered his memory immortal.

He had contracted the habit of excessive application to study, so injurious to health, though so necessary to profound inquiries; but he did not experience from it any inconvenience till during the two last years of his life.

At the commencement of the disease by which he was cut off, there was observed with alarm a moment of delirium. The sciences still occupied his mind. He spoke with an unwonted ardour of the motions of the planets, and afterwards of a physical experiment, which he said was a capital one; and he announced to the persons whom he believed to be present, that he would soon discuss these questions in the Academy. His strength gradually failed. His physician * who deserved all his confidence, both from his superior talents, and the care which friendship alone could have inspired, watched near his bed; and M. Bouvard, his fellow-labourer and his friend, never left him for a single moment.

Surrounded with a beloved family,—under the eyes of a wife whose tenderness had assisted in supporting the necessary ills of life, whose amenity and elegance had shown him the value of domestic happiness, he received from his son, the present Marquis de Laplace, the strongest proofs of the warmest affection. He evinced his deep gratitude for the marks of interest which the King and the Dauphin had repeatedly exhibited.

Those who were present at his last moments reminded him of his titles to glory, and of his most brilliant discoveries. He replied, " What we know is little, and what we are ignorant of is immense." This was at least the meaning of his last words, which were articulated with difficulty. We have often heard him express the same thought, and almost in the same terms. He grew weaker and weaker, but without suffering pain. His last hour had arrived : the powerful genius which had for a long time animated him, separated from its mortal coil, and returned to the heavens.

* M. Magendie.

The name of Laplace honoured one of our provinces already so fertile in great men,—ancient Normandy. He was born on the 23d March 1749, and he died in the 78th year of his age, on the 5th May 1827, at nine o'clock in the morning. Shall I remind you of that gloomy sadness which brooded over this place like a cloud when the fatal intelligence was announced to you. It was on the day and even at the hour of your usual meetings. Each of you preserved a mournful silence; each felt the sad blow with which the sciences were struck. All eyes were fixed on that place which he had so long occupied among you. One thought only filled your minds, every other meditation became impossible. You separated under the influence of an unanimous resolution, and for this single time your usual labours were interrupted.

It is doubtless great—it is glorious—it is worthy of a powerful nation to decree high honours to the memory of its celebrated men. In the country of Newton the ministers of state desired that the mortal remains of this great man should be solemnly deposited among the tombs of its monarchs. France and Europe have offered to the memory of Laplace an expression of their sorrow, less pompous no doubt, but perhaps more touching and more sincere.

He has received an unusual homage;—he has received it from his countrymen in the bosom of a learned body, who could alone appreciate all his genius. The voice of science in tears was heard in every part of the world where philosophy had penetrated. We have now before us an extensive correspondence from every part of Germany, England, Italy, and New Holland—from the English possessions in India, and from the two Americas—and we find in it the same expressions of admiration and sorrow. This universal grief of the sciences, so nobly and so freely expressed, has in it no less truth than the funeral pomp of Westminster Abbey.

Permit me, before closing this discourse, to repeat a reflection which presented itself when I was enumerating in this place the great discoveries of Herschel, but which applies more directly to Laplace.

Your successors will see accomplished those great phenomena whose laws he has discovered. They will observe in the lunar motions the changes which he has predicted, and of which he was alone able to assign the cause. The continued observation of the satellites of Jupiter will perpetuate the memory of the inventor of the theorems which regulate their course. The great inequalities of Jupiter and Saturn pursuing their long periods, and giving to these planets new situations, will recal without ceasing one of the most astonishing discoveries. These are the titles to true glory which nothing can extinguish. The spectacle of the heavens will be changed; but at these distant epochs the glory of the inventor will ever subsist; the traces of his genius bear the stamp of immortality.

I have thus presented to you some features of an illustrious life consecrated to the glory of the sciences. May your recollection supply the defects of accents so feeble. May the voices of the nation—may that of the world at large, be raised to celebrate the benefactors of nations—the only homage worthy of those who, like Laplace, have been able to extend the domains of thought—to attest to man the dignity of his being, by unveiling to his eyes all the majesty of the heavens.

Art. II.—*On Thorite, a New Mineral Species, and on a New Earth, Thorina, which it contains.* By J. J. Berzelius.

The Rev. Mr Esmark of Brevig in Norway having discovered a curious mineral substance in the vicinity of that place, his father, the celebrated mineralogist, Esmark, transmitted it to me for examination, supposing it to be a variety of tantalite. It occurs in the syenite, which composes the island near Brevig. It is massive, black, brittle, and semi-hard. The vitreous lustre of its fracture resembles that of gadolinite. The surface is sometimes covered with a red coating. Its powder is dark brown; its specific gravity 4.8. Before the blowpipe it gives out water and becomes yellow.

This mineral contains a new earth, which possesses so many properties resembling those of what I formerly conceived to be a new earth, to which I gave the same name, that I at first was made to believe that the latter had really contained some of this new Thorina, which, however, I afterwards found not to be the case. This resemblance is the reason why I called the new earth *Thorina.*

The composition of the mineral is as follows:—

Thorina, - -	57.91	Silica, - -	18.98
Lime, - -	2.58	Water, - -	9.50
Oxide of iron, -	3.40	Potash, - -	0.14
Oxide of manganese, -	2.39	Soda, - -	0.09
Magnesia, -	0.36	Alumina, - -	00.6
Oxide of uranium, -	1.58	Insoluble residue, -	1.40
Oxide of lead, -	0.80		
Oxide of tin, -	0.01		99.71

The new earth, thorina, possesses the following properties: It is colourless, infusible, after being strongly ignited it is insoluble in the acids, except the sulphuric acid; nor does it become soluble on being heated with alkaline substances. It is insoluble in caustic alkalies, but is dissolved by their carbonates. The solution heated gives a precipitate of thorina, which is redissolved on the temperature being lowered. The salts of thorina have a pure astringent taste. A concentrated solution of the sulphate of thorina, when boiled, coagulates into a thick pulp, but is redissolved in cold water. This property forms the most prominent character of the new earth. Like the salts of cerium, it is precipitated by sulphate of potash, with which a solution of it is saturated. The precipitate is a double salt and soluble in pure water. Like yttria it is precipitated by the cyanuret of iron and potassium.

Thorina is not reduced by potassium; but the chloride of Thorium is, which may be obtained in the same way as the chloride of aluminium. The reduction is accompanied with a slight detonation. The result is a gray metallic powder, easily soluble in muriatic acid, but very slowly in the sulphuric and nitric acids. Water and alkaline bodies do not act upon the metal. Thorium yields by pressure a bright metallic streak; it burns with a lustre similar to that of phosphorus in oxygen, leaving the earth not melted, and colourless.

Art. III.—*On the reflection and decomposition of light at the separating surfaces of media of the same and of different refractive powers* *. By David Brewster, LL. D. F. R. S. L. & E.

It is a necessary result of the Newtonian theory of light, and one which Newton himself deduced, that when white light is incident on the separating surfaces of different media, it preserves its whiteness after reflection, excepting in those cases where the thickness of one of the media is beneath the 80 millionth part of an inch.

When the discovery of the different dispersive powers of bodies was made, it should have been obvious that reflected light never could be perfectly white under any circumstances, though such a modification was not likely to be detected in the usual routine of optical experiments. The only philosopher indeed who, in as far as I know, has made any experiments on the subject is Mr Herschel; and as his opinions may be considered as representing those of the present period, I shall make no apology for quoting them.

" The phenomena which take place when light is reflected at the common surface of two media are such as from the above theory we might be led to expect, with the addition, however, of some circumstances, which lead us to limit the generality of our assumptions, and tend to establish a relation between the attractive and repulsive forces to which the refraction and reflection of light are supposed to be owing. For it is found that when two media are placed in perfect contact, (such as that of a fluid with a solid, or of two fluids with one another,) the intensity of reflection at their common surface

* The principal experiments contained in this paper were made in 1816, and were signed by the president of the Physical Class of the Royal Society of Edinburgh. A brief notice of them was published in the Quarterly Journal for July—October 1816, and a more extended paper was read at the Royal Society of Edinburgh on the 4th of January 1819. The difficulties of the subject, however, prevented me from pursuing it but at distant intervals; and the more fertile topic of polarization afterwards required all the time I could devote to such inquiries.

is always less the nearer the refractive indices of the media approach to equality; and when they are exactly equal, reflection ceases altogether, and the ray pursues its course in the second medium, unchanged either in direction, velocity or intensity. It is evident from this fact, which is general, that the reflective or refractive forces, in all media of equal refractive densities follow exactly the same laws, and are similarly related to one another; and that in media unequally refractive, the relation between the reflecting and refracting forces is not arbitrary, but that the one is dependent on the other, and increases and diminishes with it. This remarkable circumstance renders the supposition of the identity of form of the function expressing the law of action of the molecules of all bodies on light indifferently, less improbable.

" To show experimentally the phenomena in question, take a glass prism or thin wedge of a very small refracting angle (half a degree for instance: almost any fragment of plate glass, indeed, will do, as it is seldom the two sides are parallel), and placing it conveniently with the eye close to it, view the image of a candle reflected from the exterior of the face next the eye. This will be seen accompanied at a little distance by another image reflected internally from the other face, and the two images will be nearly of equal brightness, if the incidence be not very great. Now apply a little water, or a wet finger, or still better, any black substance wetted, to the posterior face, at the spot where the internal reflection takes place, and the second image will immediately lose great part of its brightness. If olive oil be applied instead of water, the defalcation of light will be much greater; and if the substance applied be pitch, softened by heat so as to make it adhere, the second image will be totally obliterated. On the other hand, if we apply substances of a higher refractive power than glass, the second image again appears. Thus with oil of cassia it is considerably bright. With sulphur it cannot be distinguished from that reflected at the first surface; and if we apply mercury or amalgam (as in a silvered looking-glass), the reflection at the common surface of the glass and metal is much more vivid than that reflected from the glass alone. The destruction of reflection at the common surface of two media of equal refractive powers explains many curious phenomena, &c."*

In the year 1814, when I was investigating the law of polarisation for light reflected at the separating surface of different media†, I had occasion to inclose oil of cassia between two flint glass prisms. The blue colour of the reflected light at first surprised me; but though the fact was new, and the experiment itself interesting, the decomposition of the light was obviously explicable upon known principles. Although the refractive density of oil of cassia exceeds greatly that of flint glass for the mean rays, yet the action of the two bodies on the less refrangible rays is nearly the same; and hence the red rays must be in a great measure transmitted, while there will be reflected a small portion of the orange, a greater portion of the yellow, a still greater proportion of the green, and a very great proportion of the blue: and consequently the colour of the pencil formed by reflection must necessarily be principally blue.

By using different kinds of glass and different oils I obtained various analogous results, in which different rays of the spectrum were extinguished by effecting (as far as possible) an equilibrium between the two opposite actions exerted upon them by the solid and the fluid media. When the blue light is extinguished, the colour of the reflected pencil has a yellow tinge; and it is obvious that the resulting pencil can never have a decided colour, but must always be bluish or yellowish.

As the indices of refraction remain the same for all obliquities of incidence, the tint of the reflected pencil, though it varies in intensity, can never vary in its colour; so that we cannot obtain any succession of tints or coloured rings from this partial decomposition of the incident rays.

These observations establish it as a general fact, that in all cases of reflection from transparent surfaces, the reflected pencil must necessarily have a different tint from the incident pencil, excepting in the extreme case where the two bodies in contact have mathematically the same refractive and dispersive powers.

* *Treatise on Light*, § 547, 548.

† *Phil. Trans.* 1825, p. 137.

I was now anxious to observe the effect of an approximation to this last condition, or to a perfect equilibrium of all the forces which affect the incident rays; as it is often in extreme cases, and at a limit such as this, that nature delights in the developement of new phenomena. This experiment, however, was attended with more difficulty than I expected; but amid the numerous disappointments which it occasioned, I was led to the results which I shall now proceed to describe.

The solids which I employed were two prisms of plate glass, which I shall call A and B. The prism A, whose section was an isosceles right-angled triangle, had its base polished at the plate-glass manufactory where it was made. The prism B was executed for me by Dollond, and very finely polished, having also its section a right-angled isosceles triangle. The refractive indices were

$$\text{In A} \ldots m = 1.508$$
$$\text{In B} \ldots m = 1.510$$

The fluids which I employed were castor oil and balsam of capivi, the latter having a greater and the former a less refractive power than the glass prisms. The refractive indices were

$$\text{In castor oil} \ldots\ldots\ldots m = 1.490$$
$$\text{In balsam of capivi} \ldots m = 1.528$$

The prisms A, B were now fixed together as in Plate III. Fig. 1, and a film C D of castor oil interposed between them. A ray of light R*r* will after refraction at *r* be reflected in the direction *o q m* from the surface C *o* D which separates the prism A and the oil; and another portion of it will be reflected in the direction *p s m* from the surface G *p* H which separates the prism B and the oil. In order that the two rays *q m*, *s n* may be sufficiently separated, the common sections of the faces which contain the right angle are slightly inclined to each other.

When the angle of imcidence R*r* E is very great, the light suffers total reflection at the surface C *o* D. Within the limit of total reflection the light *o q m* is yellow; and by diminishing the angle of incidence gradually, the pencil *o q m* passes through all the tints of nearly three orders of colours, as shown in the following Table :—

	Colours.	Angles of Incidence Rr E.	Angles of Incidence on Surface Co D.*
1st Order.	Yellow, - -	70°	83° 33′
	Orange, - -	63	81 13
	Red, - -	61	80 27
	Pink, - -	59½	79 51
	Limit of pink and blue,	58	79 14
2d Order.	Bluish pink, -	57	78 46
	Full blue, - -	55	77 54
	Greenish blue, -	52	76 30
	Yellowish blue, -	48	74 32
	Yellow, - -	41	70 46
	Reddish Yellow, -	34	66 46
	Redder still, - -	26	61 54
	Red, - -	21	59 4
	Pink red, - -	17	56 11
	Limit of pink and blue,	14	54 14
3d Order.	Blue, - -	+9	50 57
	Bluish green, -	0	45 0
	Yellowish, -	—15	35 46
	Full yellow, -	—22	30 37
	Reddish yellow, -	—31	25 21
	Pink, - -	—52	13 30

The colour of the pencil $p\ s\ n$ produced by the other separating surface G p H is at all incidences a faint yellowish gray, (which is best seen by turning the system of prisms upside down; and receiving the ray Rr upon the prism B, so that the reflected ray $p\ s\ n$ may not pass through the oil;) and its intensity suffers very little change. This fact is a very remarkable one, and arises (as will be presently seen) from some specific property of the glass itself. When the lower prism is of the same glass as A, and produces the colours in the preceding table at different angles of incidence from those of A, the play of colours is particularly fine, and the whole phenomenon is one of the most beautiful in physical optics.

When the incident light is homogeneous, no colours of

* This column is calculated from the formula $A = 45° \pm \frac{\sin. I}{m}$, I being the angles of incidence in the 1st column, A the angles in the 2d, and $m = 1.508$ the refractive index of the glass.

course are seen; but the reflected pencils have their maxima and minima of intensity, like the rings of thin plates or the fringes of inflected light when formed by homogeneous rays.

The following are the periods for red and for blue light:—

		Red Light.	Blue Light.
1st minimum,	-	77° 54′	80° 27
2d minimum,	-	50 57	59 4

If we substitute for the prism A a square prism, the tints are thrown more closely together; and if the luminous object is a long stripe of bright light, we may see most of the colours at one view.

If we now apply heat to the oil so as to diminish its refractive power, the brightness of the colours is greatly diminished, and the first period is completed at a less angle of incidence.

Such are the phenomena which take place when the refractive power of the glass exceeds that of the fluid. We shall now see what happens when the fluid has a greater refractive energy than the solid; a case of peculiar interest, because we are able to reduce the two refractive powers to a perfect equality for any given ray of the spectrum.

The same prisms being employed, let the film C D H G be now balsam of capivi. Before total reflection takes place, the reflected pencil is perfectly white: it then becomes yellow, and passes through the same orders of colours as in castor oil. All the colours, however, are produced at less angles of incidence, the 1st order terminating at an angle of 64° 58′, as appears from the following Table, in which I have given only the leading tints.

	Colours.	Angles of Incidence R *r* E.	Angles of Incidence on Surface C *o* D.
1st Order.	Yellowish, - -	47°	74° 10′
	Yellow, - -	41	70 47
	Pink red, - -	36	67 57
	Pink, - -	33	66 10
	Limit of pink and blue,	31	64 58

	Colours.	Angles of Incidence Rr E.	Angles of Incidence on Surface Co D.
2d Order.	Bluish pink - -	28°	63° 8′
	Full blue, - -	26	61 54
	Bluish green, -	22	59 23
	Bluish yellow, -	18	56 50
	Yellow, - -	10	51 37
	Reddish yellow, -	1	45 40
	Red, - -	— 8	39 42
	Pink red, -	—13	36 25
	Limit of pink and blue,	—16	34 28
3d Order.	Blue, - - -	—22	30 37
	Bluish green, -	—26	28 56
	Green, - -	—30	25 29
	Yellowish green, -	—41	19 13

Having ascertained that at a temperature of about 94° the mean refractive index of the balsam was nearly equal to that of the glass prisms, I proceeded to examine the influence which a varying temperature from 50° to above 94° exercised over the intensity and the colour of the reflected pencil.

The prisms were therefore fixed so as to exhibit the full blue of the second order, and the heat was gradually applied. The colour of the tint was obviously improved by heat, though the intensity of its light was diminished. No particular change marked the instant when the refractive density of the glass and the balsam was equal. Beyond 94° the intensity of the tints increased in consequence of the diminution in the refractive power of the balsam; but when the temperature was considerably augmented, the tints completely disappeared.

Let us now attend to a very remarkable phenomenon exhibited in the relative intensities of the pencils *o q m* and *p s n*. At an angle of incidence of 61° 54′ on the surface C *o* D, and at a temperature of about 50°, the pencil *o q m* is a full blue, while *p s n* is a grayish white of rather less intensity than the blue pencil. By increasing the angle of incidence, the pencil *o q m* increases rapidly in intensity, while the gray pencil diminishes slowly: so that at an incidence of 74° *o q m* is ten or twelve times more luminous than *p s n*; whereas at smaller incidences than 61° 54′, the pencil *p s n* surpasses *o q m* in

As nothing depends on the numerical accuracy of these indices, I did not measure them with any peculiar attention; but by immersing a right angle of each prism in a vessel containing each of the three oils, I carefully determined that, at a temperature of 50°, they acted on the homogeneous yellow light of a monochromatic lamp, in the order in which they are above placed.

I now combined each of the oils in succession with the two prisms, as shown in Fig. 1, and in all the combinations the separating surface of the prism A and the oils produced from a white flame, nearly three orders of colours of the same intensity, and nearly at the same angles of incidence, as in balsam of capivi; while the separating surface of the prism B and the oils reflected only a faint gray image of very little intensity, and generally growing fainter as the angle of incidence increased.

When the homogeneous yellow light of a monochromatic lamp was used, the separating surface of the prism A and all the oils produced the first minimum at nearly the same angle of incidence; and though I applied heat gradually to the least refractive oil, and cold to the most refractive one, so as to produce a perfect compensation of opposite refractions for the yellow rays, yet no perceptible change appeared either in the place of the first minimum or in the intensity of the reflected light. In the case of the mixed oil the compensation was effected without any other change of temperature but what was occasioned by a change of position in the apartment.

In the expectation of discovering some solid or fluid medium which would produce with plate glass a greater number of orders of colours, I made the experiments contained in the following tables.

TABLE, Showing the periods of colours produced at the separating surfaces of plate glass and oils and other fluids.

Image at the Surface of Prism A.	Image at the Surface of Prism B.
Oil of Cassia.—Pale red tints at 65° of incidence; then at less incidences pale blue, and then pale red. Heat strengthens the tints a little.	White and bright.

Image at the Surface of Prism A.	Image at the Surface of Prism B.
Balsam of Peru.—Slight tinges of red; blue as above. Two faint orders of colours brought out by heat.	Yellowish white.
Oil of Anise-seeds.—The tinges of two orders of colours. Heat of 200° brings out two good orders of colours. Limit of pink and blue of the first order at an incidence less than 65°.	Grayish or bluish white.
Balsam of Styrax.—Tinges of two orders of colours. Improved by heat.	Bright white.
Canada Balsam.—Above two orders of colours; pink of second the best. Improved by heat.	Grayish or bluish white.
Oil of Tobacco.—Two faint orders of colours. Heat brings out nearly three.	Grayish white.
Oil of Cloves.—Two faint orders. Heat brings out part of a third. First limit of pink and blue about 65° of incidence.	Yellowish white; but bluish gray with heat.
Oil of Sassafras.—Two orders. First red pale. First blue good.	Grayish white.
Balsam of Capivi.—See page 213.	
Muriate of Antimony.—Two tolerably distinct orders of colours.	Grayish white.
Oil of Cummin.—Two beautiful orders. A fine yellow in the second order. Heat spoils them all.	Faint grayish, becoming more intense and yellow by heat.
Nut Oil.—Two faint orders, the second red and second blue being tolerably good. Heat brings out two fine orders, the first limit of pink and blue ending at about 76° of incidence.	Yellowish white.
Oil of Pimento.—Three good orders of colours. First limit of pink and blue at 65° of incidence.	Pale blue, very faint at great incidences.

Image at the Surface of Prism A.	Image at th Surface of Prisr
Oil of Sweet Fennel-seeds.—Two orders; pink good.	Bluish gray.
Wood Oil.—Three good orders of colours. First pink and blue fine. First limit of pink and blue ends at 65°	Bluish gray, we er at great in dences.
Oil of Amber.—Two excellent orders of colours. First limit of pink and blue at 65°. Improved by heat.	Pale blue, very fa at great incidenc
Oil of Rhodium.—Two and a-half good periods. First limit at 65°. Heat injures them.	Yellowish white.
Treacle.—At temp. 50° three orders, which are not good, especially the pink of first and blue of second order. Heat brings out three splendid orders with periods, as in castor oil. *	Yellowish white.
Balsam of Sulphur.—Three fine orders. First limit of pink and blue at about 67°.	Faint gray, getti fainter and bl at great inciden
Honey.—Two pretty good orders. First limit at about 65°.	Slightly yellowis white.
Oil of Angelica.—Two and a-half orders. First pink and first blue fine; second red good.	Whitish yellow.
Oil of Nutmeg.—Three not very bright orders. First limit at 73°.	Whitish yellow.
Oil of Marjoram.—At a low temperature the orders are scarcely perceptible, the second limit only being visible. Heat brings out the second limit at a less incidence, and creates the first limit at 79°.	Whitish yellow.
Castor Oil.—See page 212.	
Oil of Hyssop.—Colours very faint. Heat brings out three good orders. First limit at 77°.	Whitish yellow.

* The treacle used in this experiment is much inferior in refrac power to the prism A.

Image at the Surface of Prism A.	Image at the Surface of Prism B.
Oil of Fenugreek.—Colours rather better than the preceding. Heat brings out three good orders. First limit at 75°.	Whitish yellow.
Oil of Caraway-seeds.—Two orders, not good.	Whitish yellow.
Oil of Thyme.—Slight tinges of colour. Heat brings out two good orders.	Yellowish white.
Oil of Turpentine.—Two tolerably good orders. First limit at 74°.	Whitish yellow.
Cajeput Oil.—Two tolerably good orders. First red bad, second red good.	Yellowish white.
Linseed Oil.—Two extremely faint orders. Three good orders brought out by heat. First limit at 73°.	Yellowish white.
Train Oil.—Three very good orders. First red and first blue excellent. First limit at 73°. Heat spoils the first order.	Yellowish white.
Oil of Savine.—Almost no colours, both images being yellowish, and that of B brightest. Heat brings out three orders. First limit at 80°, which a greater heat brings to 75°.	Yellowish white.
Oil of Pennyroyal.—Almost no colours. A sort of bluish gray when cold. Heat brings out two good orders when temp. only 90°, but greater heat injures them.	Yellowish white.
Oil of Almonds.—Three tolerable orders. First red bad, second red good.	Yellowish white.
Oil of Mace.—Gives three and a quarter orders when cold. First limit at about 80°.	
When the film of the oil begins to crystallize, it displays red, blue, and greenish tints, at the same incidence, in different places.	Pretty bright.

Image at the Surface of Prism A.	Image at the Surface of Prism B.
Oil of Spearmint.—Very faint colours. Heat brings out three good orders. First limit at about 77°.	Yellowish at great incidences.
Oil of Lemons.—Three fine orders. First limit at 74°. Heat destroys the first order.	Yellowish white.
Oil of Dill Seed.—Two poor orders of colours. First limit at 73°. Heat improves them.	Yellowish white.
Oil of Peppermint.—Two good orders. First limit 73°. Heat destroys the first order.	Yellowish white.
Oil of Rapeseed.—Two very faint orders. First limit at 65° when improved by heat.	Bluish gray.
Naphtha from Persia.—Three very good orders.	White.
Oil of Bergamot.—Three very fine orders. First limit at 73°. Heat spoils first order.	Yellowish white.
Oil of Beech Nut.—Three excellent orders, and well defined. First limit at 73°. Heat spoils first order.	Yellowish white.
Spermaceti Oil.—Two tolerable orders. First red and blue bad, second red and blue good. First limit at 73°.	Yellowish white.
Oil of Olives.—Three good orders. First limit at 73°.	Whitish yellow.
Grass Oil.—Three good orders. First limit at 73°.	Grayish white.
Oil of Rosemary.—Two good orders and more. First limit at 73°.	Whitish yellow.
Oil of Poppy.—Three excellent orders. First limit at 73°. Heat injures the colours.	Yellowish white.
Oil of Lavender.—Three good orders. First red and first blue very fine. First limit at 74°.	Yellowish white.

Image at the Surface of Prism A.	Image at the Surface of Prism B.
Camomile.—Two good periods. t limit about 60°.	Bright yellowish white.
Wormwood.—Three good periods. t limit at 71°, but not well de- l.	Yellowish white.
Juice.—Three faint orders at low eratures, but finely brought out eat. First limit at 73°.	Yellowish white.
tic Acid.—Traces of tints.	Yellowish white.
ric Acid.—Two pretty good or- .	Yellowish white.
s Humour of the Haddock.— es of colours.	Bright.
Rhue.—No colours.	Bright.
Boxwood.—No colours.	Bright.
.—Traces of reddish, bluish, and ish yellow tints.	Bright.
—Traces of tints.	Bright.

experiments * recorded in the preceding pages may be into two classes.

'hose which establish the existence of reflecting forces onfines of media of the same refractive power; and,

Those in which periodical colours are produced at the of particular kinds of glass, and various fluids and ids.

n the first of these classes of facts the following conclu- ay be drawn.

he reflective and refractive forces in media of the same ve power do not follow the same law. This result is established by the experiments with the prism B, which d no orders of colours. Not only was there a strong

se experiments have been extended to a great number of mixed o soft solids, gums and resins, combined with the prisms A and ve also substituted for these prisms others of different kinds of ich give similar results; and I have examined the phenomena at es of different fluids and a great number of minerals of various powers between chromate of lead and fluor spar.

reflected pencil when a perfect equilibrium was effected between the opposite refracting forces, but there was not even an approximation to evanescence, as the forces advanced to their point of compensation. The same result was obtained with a prism newly ground and polished.

2. The force which produces reflection varies according to a different law in different bodies. If the curve which represents the law of the reflective force were exactly the same in the prism B and the fluids combined with it, then the ordinates which represent the intensity of the force at any given point would be exactly equal, and consequently there would be a perfect equilibrium of opposite actions, and no reflection of the passing light. But as a copious reflection takes place even when the opposite forces are balanced, we are entitled to infer that the law of the two forces is different.

The reflective forces in the solid and fluid may be conceived to decrease in various ways.

1. They may extend to different distances from the reflecting surface, and decrease according to the same law. This relation is shown in Plate III. Fig. 2, where MN is the reflecting surface, AB the limit of the sphere of reflecting activity in the solid, and CD that in the fluid,—*a o b* the curve which represents the reflecting force of the solid, and *c n d* that of the fluid. In this case there can be no compensation of opposite reflections, and an unbalanced reflecting force will exist at almost every point of the sphere of reflecting activity. From *a* to *c* the light will be acted upon by the undiminished force of the solid. At *c* the force of the fluid begins to oppose that of the solid, and the unbalanced force at any other line *m o* is equal to *n o*, the difference of the two forces *m n*, *m o*. In this case there will be a sphere of reflecting activity extending from AB to A′B′, and such a combination must reflect light without refracting it.

2. The reflecting forces may extend to different distances, and vary according to a different law. Two cases of this kind are shown in Fig. 3 and 4.

In the case of Fig. 3, the curves expressing the law of the forces have a common ordinate *m n*, where the reflections are compensated; but from *a* to *n* the reflecting force of the solid will predominate over that of the fluid, and from *n* to *d* the force of the fluid will predominate over that of the solid; so that in such a combination there will be two spheres of reflecting activity, one of which begins where the other ends.

In the case of Fig. 4, where the curves have the same maximum ordinate *m b*, we shall have a sphere of reflecting activity commencing at *a*, reaching its maximum at *c*, and its minimum at *b*.

3. The reflecting forces may be conceived to extend to the same distance, and to vary according to different laws. Two cases of this kind may occur; one, as in Fig. 5, where the maximum of unbalanced force is distant from the surface, and another, as in Fig. 6, where the maximum takes place at the reflecting surface.

In the conclusions which we have drawn respecting the independence of the reflecting and refracting forces, it was supposed that the latter follow the same law in solids and fluids. There seems to be no method of determining whether or not this is the case; for experiment indicates only the total effect, or the sum of all the ordinates, and these may be compensated, though they vary according to different laws.

There is one hypothesis, however, on which the preceding experiments may be reconciled with the supposition of the mutual dependence of the reflecting and refracting forces. If we suppose, for example, as in Fig. 3, that the refracting forces of the solid and fluid are regulated by the same curves as their reflecting forces, and that the absolute effect of each is the same; then, though the refractive forces are perfectly balanced, and though the total effect of each reflecting force, taken separately, is the same in the solid as in the fluid, yet light will still be reflected in the manner formerly described. It seems highly probable that the law of the refracting force varies in different bodies; and if we take for granted the mutual dependence of the refracting and reflecting forces, the preceding experiments will establish a variation in the law of the refracting forces of different media.

In the undulatory system, the preceding facts may be explained by supposing that the density or elasticity of the ether varies near the surface of different bodies; a supposition in itself highly probable, and which has been already adopted to explain the loss of part of an undulation in several of the phenomena of interference. In such a case the reflection of the light will commence at a line where the density or elasticity of the ether in the first medium begins to change, and will continue till the ray has penetrated to that part of the second medium where the density or elasticity of the ether is uniform. In this theory, therefore, the preceding facts may be regarded as proving the variable condition of the ether near the surfaces of bodies, and of establishing the beautiful and sagacious deduction of Dr Young, that the part of an undulation lost is a variable fraction depending on the nature of the contiguous media.

II. We come now to consider the second class of phenomena, or the existence of periodical colours at the confines of certain media of the same and of different refractive powers.

That the periods of colour arise, as in all similar phenomena, from the interference of two portions of light cannot be questioned; though it does not appear how these interfering pencils are generated. If we adopt the hypothesis of the reflecting forces shown in Fig. 4, we may conceive the light reflected about C D to be interfered with by the light reflected about C′ D′, so that the same effect nearly might be produced as if C D, C′ D′ were the limits of a thin plate. If this supposition is not admissible, we may hazard the conjecture, countenanced by some facts which will presently be stated, that an invisible film, differing in refractive power from the plate glass, has been formed upon its surface.

There is one phenomenon which has been more than once mentioned, and which requires some farther notice; namely, the decrease in the intensity of the pencil as the incidence becomes more oblique. In re-examining this very perplexing fact, which takes place in the prism B, though it does not produce periodical colours, I have observed at a great incidence a distinct change of colour, from a bluish gray to a blue; so that I have no doubt that in this case the tints are those of a long period approaching slowly to its minimum. This consideration led me to suppose that in the case of balsam of capivi and other fluids, where the first order ends at and below 65°, there might be another minimum between that angle and 90°, which was prevented from showing itself by the intensity of the reflected light. This conjecture was confirmed by a careful repetition of the experiment with tubes of glass, and also by another prism in which the only tint was a pink red at an incidence of about 85°, and a blue shading off into a greenish gray at less angles of incidence. In this case, then, there was only one minimum at about 85°. A slight diminution of temperature shifted this minimum towards 90°, while an increase of temperature brought it to a lesser incidence than 85°.

Although there can be little doubt that periodical tints are more or less developed in every combination of solids and fluids of the same refractive power, yet their production in combinations where there is much uncompensated refraction, is influenced by certain changes on the surface of the solid, the nature and origin of which I have in vain attempted to discover.

Having observed that the colours occasionally became less bright after the media had remained some time in contact, and that different parts of the same surface produced the same tint at inclinations sensibly different, I took a prism which gave with castor oil three fine periods; and having brought it to a white heat, I then ground and repolished its faces. It now ceased to give the same periods as before; but it still decomposed the white light reflected from its confines with balsam of capivi, and reflected a strong pencil of a blue colour, even when the opposite refractions were perfectly compensated. I now ground and repolished one of the faces of the obsidian already mentioned. It also ceased to give the colours with balsam of capivi formerly described; but it now produced, when combined with castor oil, with which it previously gave no colours, a beautiful yellow pencil, the reflected light being white at great incidences, and becoming yellower as the ray approached the perpendicular. In order to ascertain what changes might be owing to the processes of grinding and polishing, I sought out an old face of fracture in a plate of glass, whose wrought surfaces gave fine periodical colours; and I formed a new face of fracture. The old face, which had been exposed for ten years, gave the usual orders of colours;

but the new face gave only one colour, which was a bright blue, but which, from the nature of the surface, I could not trace to high or low incidences.

As these results seemed to indicate that the glass had received from exposure to the air some incrustation, or had absorbed to a small depth some transparent matter in a minute state of division, or had suffered some change in its mechanical condition, I made various fruitless attempts to ascertain the nature of the change. No superficial tarnish could be rendered visible, either by the microscope or by any other means. I boiled the prisms in muriatic acid, and in strong alkaline solutions: I steeped them in alcohol, and applied a strong pressure along their surfaces; but I could not in the slightest degree change their action upon light.

If a superficial film had been formed upon the glass of such a thickness as to give the periodical colours, then its refractive power must be different from that of the glass. I therefore took a prism which gave the periodical colours, and another of the same glass which had been deprived of this property; and I found that they polarised light at exactly the same angle. I then placed them upon the base of a flint glass prism with oil of cassia interposed, and I determined that the angle at which they reflected light totally was the same *. Hence it was manifest that the supposed film did not differ in refractive power from the glass; and even if it did, some one of the oils with which it was in contact in the foregoing experiments must have had the same refractive energy, and must thus have deprived it of its power to develope the periodical tints. In the hope of unravelling this mystery, I took two prisms of glass cut out of the same plate, and which gave fine periodical colours with castor oil. By the aid of screws I pressed the bases of the prisms into optical contact: at great incidences the light was yellow; and by diminishing the inclination of the ray it became gradually orange and deep red when it vanished, no light being visible at smaller angles of incidence. In this experiment the surfaces of the two films, if they do exist, were

* The prism which produced the periodical colours, did not give so distinct a boundary between partial and total reflection as the other.

brought into optical contact, so that we ought to have had orders of colours corresponding to a film of twice the thickness.

But even if such a film could be supposed to exist invisibly on the glass, it could not afford any explanation of the splendidcolours which are exhibited when the solid is a cryst allized mineral, and where its tint is related to its axis of double refraction. That some unrecognized physical principle is the cause of all these phenomena, will appear still more probable when I submit to the Society a paper on the very same periods of colour produced at similar angles of incidence, by the surfaces of metals and transparent solids when acting singly upon light.

The action of the surfaces of crystallized bodies presents many remarkable phenomena, in the investigation of which I have been long occupied. The results to which I have been led will form the subject of two communications. The first will treat of the action of the surfaces of bodies as an universal mineralogical character, with the description of a lithoscope for discriminating minerals. The second will contain an inquiry into the influence of the doubly refracting forces upon the ordinary forces which reflect and polarise light at the surfaces of bodies. My early experiments on this subject are recorded in the *Phil. Trans.* for 1819, but I have resumed the inquiry, and have obtained results of considerable interest *.

ALLERLY, *February* 2, 1829.

ART. IV.—*Notice of some of the Birds of Madeira.* By C. HEINEKEN, M. D. Communicated by the Author.

SIR,

IF you can find room for the following notice of two or three of our birds, I shall feel obliged by its insertion; and should they prove to be well known, I can only offer the few particulars of their habits, &c. which are added, as a compen-

* From the *Phil. Trans.* 1829, Part 1, p. 187

sation for their want of novelty; and circumscribed means of information as an excuse for my ignorance.

I am, Sir, your obedient servant,

C. Heineken, M. D.

Funchal, *Madeira*, *25th April* 1829.

Columba ———?

Brownish ash; head, neck, breast, vent and rump, ash; neck imbricated, and together with shoulders and breast iridescent; belly vinous; wing and tail feathers brown black, the latter with a broad blue ash bar one-third from the tips, which are black; the outer web of the 2d, 3d, and 4th primary of the former, edged with white; bill red, tipped with black; nails black; legs red, feathered anteriorly a little below the knee; iris pale straw colour; length 19 inches; tarsus 1½ inch. (Adult male in the spring.)

Ash; head, neck, belly, and rump blue ash; *neck only* iridescent and imbricated: breast and shoulders vinous; length 18 inches; tarsus 1½ inch. (Adult female in the spring.)

General Characters.

Those of Section B, (*Columbæ antarctopodiæ*) of Wagler. Tarsus and middle toe (measuring the nail) equal; the former feathered about one-third anteriorly; tail of twelve feathers, 6½ inches long (in the female) and slightly rounded; gape (female) 14 lines; imbricated feathers dense, rounded, and of a pearly colour; more conspicuous in the female than in the male; weight about 18 ounces.

I am almost afraid to hazard either a specific name or a *nobis* to this pigeon; the genus is so extensive, and I have so little practical knowledge of it. I cannot, however, reconcile it with any species given by Wagler in his *Systema Avium;* and should it prove to be hitherto undescribed would propose its provincial name "Trocaz" as a specific designation. It is found in the most wooded and unfrequented parts of the island, and is so shy and difficult to get at, that I can learn but little of its habits. The Palumbus (which is much more rare here) is occasionally seen at the same drinking spots with it; but there is not the slightest reason to suspect that they ever cross together, or even associate intimately. An intermediate plumage is never seen, and they are both constant in their marks to a feather. The berries of the *Persea fœtens* are found in its stomach; and during the berry season the birds are fattest and best flavoured. They build in high trees in the thickest and most inaccessible places; and as a nest is rarely taken, I can give no account of either the eggs or young. They are killed generally when drinking.

Procellaria Anjinho.

Bill shorter than the head, and compressed towards the tip; nostrils united in a single tube at the surface of the bill, but the septum distinctly seen a little within the orifice; tail slightly forked, extremity of wings not surpassing it; plumage *entirely* brown black or soot colour; bill black; legs smoky; length 11 inches; tarsus 1 inch. (Adults taken in spring and summer.)

This bird belongs to the *Petrel hirondelle* section of Temminck's *Manuel*. It is larger and thicker than the *P. pelagica*, and has *no white* in any part of the plumage. I cannot find it described in any of the few works to which I have access. It is well known here as the "Anjinho" (literally "little angel," but figuratively perhaps "imp," for there is certainly more of darkness than light both in its hue and habits,) and is found on the uninhabited and unfrequented islands of this place and Porto Santo, where it breeds, laying one dirty looking egg. It appears first in February and March; begins to lay early in June; the young are hatched in July, and after September few or none are seen until the following spring. It is never seen in the bay or near the inhabited or much frequented parts of the island, or in flocks, as our other petrel (*Procellaria puffinus*) is, but keeps out at sea, or in the neighbourhood of its haunts, and is in a great measure nocturnal in its habits. At the Denetas, (uninhabited islands about 8 leagues S. E. of Madeira,) it breeds in considerable numbers along with the *P. puffinus*, and its young are taken and salted indiscriminately with those of that bird. The fowlers know the nest from the intolerable stench of the hole in which it is made. Although so well known, and apparently so much within reach, still, as its haunts are some way out at sea, and when visited periodically, yet always by those who salt the young on the spot, and are too indolent and indifferent to be at the trouble of bringing away either living or uninjured specimens, I failed in obtaining a bird before May 1828, when I met with several at Porto Santo; and in the following month sent one to a friend in London, who promised to ascertain whether the species was (as I suspected) new; but I have not since heard a word of either friend or bird, they being *both* perhaps *only summer* visitants. About a month since I accidentally learnt that Sir William Jardine had a petrel from Madeira, which he was inclined to think new; and although my informer knew nothing of either its size or colour, yet as, excepting one specimen of the *Procellaria Anglorum* which was taken last summer, I have never either seen or heard of any other than this (our Anjinho), and the *P. Puffinus* (our Cagarra), I suspect they must be the same. I have therefore left the specific name open, to be filled up by that which Sir W. Jardine may have given, supposing them to be *identical and new;* but should they prove distinct, and the one which I have described *new also,* the provincial name " Anjinho," (pronounced as though spelt " An-ji-gno" in *Italian,*) will, I think, make as good a specific one as any other. *

Cypselus murarius? (Tem.)

In the winter of 1827 one was brought to me with *the whole of the plumage black,* but unfortunately it was thrown away by a servant before I had ascertained more than *this fact,* and that it was *truly a Cypselus.* During the following summer I received three, black with a *white chin,* measuring 7 inches 5 lines, and four much browner than any I had ever seen in England, (nearly dun in colour,) with *white chins* also; evidently *very old* birds, and measuring 7 inches 9 lines. Throughout the last winter (although I applied to every one in the habit of using a gun, ill health preventing me from doing so,) I did not procure a single specimen of any description. So that I

* Sir W. Jardine is of opinion that this species is new. He possesses the *P. Leachii* from Madeira, which Dr H. does not mention, and also the *Cypselus murarius,* and the black-chinned individual, which he thinks is a distinct species, and probably undescribed.—Ed.

am unable to ascertain whether the *black chinned* individual was a *C. murarius* in the *winter plumage* (about which very little is I believe known), or a different and perhaps a new species. This bird is *stationary here*, and simply, I imagine, because it is equally well supplied with the food throughout the year. It builds in rocks, and abounds most on the Serras. I have therefore had but few opportunities of inquiring into its habits; and my principal object in naming it now, is to invite information respecting its *winter* plumage, and to ask whether the St Domingo swallow of Brisson and Bajon mentioned in Buffon, be a Cypselus or Hirundo; for if the former, some mutual assistance may perhaps be afforded by it and ours, in elucidating one another.

Art. V.—*Observations on certain Resinous and Balsamic substances found in Guiana.* By Dr Hancock.* Communicated by the Author.

Carana.—*The Ackaiari of the Caribs, Macosis, and other tribes.*

This gum resin exudes spontaneously from the ackaiari tree

* In justice to Dr Hancock's labours, we beg to quote the following account of them from Lord Stanhope's address to the Medico-Botanical Society. " Many interesting and important papers have, during the last year, been read at your meetings, and that which, without any disparagement to the others, claims the preference, and is entitled to the gold medal which your council has awarded, is the communication of Dr Hancock on the *Angustura bark tree*, which, as you are well aware, is imported in considerable quantities and employed with great advantage. This had erroneously been named *Bonplandia trifoliata;* but was first ascertained by Dr Hancock to belong to a neighbouring genus, and is now termed by him *Galipea officinalis*. This great and valuable discovery, which affords an additional proof of the extreme utility of botany to the materia medica, deserves your grateful acknowledgment; and the importance of his communication is very much enhanced by his having employed the *Angustura bark* with great success in cases of contagious disorders. It affords me particular satisfaction, that we have the pleasure of seeing Dr Hancock on this occasion, and that I have the opportunity of thanking him in the name of the Society, of expressing our respect for his talents, our admiration of his exertions, and our earnest hopes that we shall often benefit by his assistance, and often receive his instructive and excellent communications, and of offering to him our best wishes for his prosperity, for the

which grows plentifully upon the sides of the mountains and high grounds of the interior, and especially on the Macosy mountains and those of Parima where I travelled in 1810 and 1811.

The tree has a tall straight trunk, covered with a rough bark, and attains to a very considerable size, often measuring eight or ten feet in circumference.

The leaves are oblong, smooth, and pointed.

The wood resembles mahogany in colour and texture, but is less ponderous. It is an aromatic wood similar to cedar, and is so called by the wood-cutters high up the rivers.

I am unable, however, to give a systematic description of the tree, never having seen the flower. The seeds which I picked up as they fell from the tree and sowed after my return, did not vegetate. They were blackish, of oval form, near the size of peas, and inclosed in oval capsules.

I believe the tree to be a species of *Cedrela*, *Anniba* of Aublet, or a species of *Amyris*.

The gum resin, as I take it to be, besides exuding spontaneously, is also procured by making an incision in the trunk, and is then given out very copiously.

This gum, when recently procured from the tree, or well preserved from the air, exhales an extremely fragrant odour. The Macosis collect and wrap it in palm leaves in oblong rolls.

M. Humboldt, in his *Personal Narrative*, vol. v. p. 258, observes, that "the Carana is a resin strongly odoriferous and white as snow," &c.—This, however, is not Carana, but Hyowa, most common here as well as on the Rio Negro, whence it is brought to Angostura for sale.

continuance of his health in these distant regions to which he intends to return."

(The President then addressed Dr Hancock, and presented to him the Gold Medal.)

Another paper of great curiosity and merit was also written by Dr Hancock on the *Vandrella diffusa*, a decoction of which acts as an emetic, and is employed to cure both continued and intermittent fevers; and that plant was also used by him, and with favourable results, in chronic disorders of the liver. In these cases it would be very desirable to find an effectual substitute for those mercurial preparations which may be dangerous in their application.—Ed.

M. Humboldt speaks of this product as consisting of divers kinds. At the page just cited he has a note of inquiry, "Are not the substances known by this name (*Carana*) at the Orinoko partly gums? I was assured at Esmeralda, that savage nations living to the east of the high mountains of Duida eat the Carana. This name is given to very different plants."

The cause of disappointment and error here chiefly arose from M. Humboldt's being surrounded by people who regarded the Carana only as an article of traffic, who considered his inquiries as having no other aim; and, in answering these inquiries, were ambitious only of giving information of every thing that could possibly answer for Carana, for stopping canoes, or to send to Angostura for sale; for these they heap together from every tree that affords a gummy resinous exudation. M. Humboldt has hence confounded the Carana, the Mani, Hyowa, Courucay, and several resinous substances which are as yet undefined.

Of the true Carana there is but one kind, viz. the *Ackaiari* of the Caribes and Macosis.

This gum resin possesses much bitterness which seems to reside in an extractive principle, but is not well determined.

The Indians make use of it principally in two ways; either as medicine or as a perfume.

For the latter purpose they mix it with the paint and oil which they use for anointing their bodies; being in a recent state either soluble or miscible in oils.

As a medicine, they use it for catarrhal defluxions, coughs, and affections of the lungs, inhaling the fumes arising from its combustion; and it is spoken of by the upper Indians as a sovereign remedy in such complaints.

The coloured people of the Essequebo say, that, melted with oil, it makes an excellent plaster, both for recent cuts and old ulcers; and it is certainly true that the pure Carana melted with a little tallow, forms an ointment of a most healing nature; but as it contains, besides resin and essential oil, a portion of gummy and extractive matter, it will not at all dissolve with the grease. It should therefore be strained whilst hot, and the residue rejected.—These remarks are equally applicable to the *Hyowa* gum, which also forms a choice ointment.

The *Ackaiari* is one of the most valuable of the timber trees of Guiana. The wood is perhaps adapted to a greater variety of uses than any other in that country. It is indeed rather distant from the settlements; yet I should conceive it could be easily floated down the Essequebo, being nearly as light as deal or pine timber, although as durable perhaps as cypress.

Hyowa.

This is obtained from the *Amyris Ambrosiaca* of Willdenow, *Icica 7-phyllæ* of Aublet, and grows abundantly over all Guiana.—It is often mixed with the Courucay, the gum of another species of the same genus.*

The Hyowa is held in high esteem by the inland natives, as one of their most sovereign antihectic remedies.

The semifluid juice in particular, when recently taken from the tree, is extremely fragrant and odoriferous. It may be preserved in this condition, if it be drawn into a bottle from the tree and well corked.

In this state I have heard cited very numerous instances of its effecting cures, in cases of cough and emaciation, among the creoles of these colonies, being taken in new milk and sugar. Thus fresh from the tree, Mr Baker says it cured him of emaciation and consumptive cough (of such severity and continuance that very little hopes were entertained of his recovery,) by taking it every morning and evening, and washing it down with new milk. He was not exact, but thinks he took it usually about a small teaspoonful to the dose.

Its fumes are sometimes inhaled in coughs, by placing it on a heated stone. It is thus carried more into immediate contact with the diseased parts. I believe, however, that its beneficial effects arise chiefly from a more general resolvent alterative action on the system, through the medium of the stomach and absorbent system, and that it is best taken as first stated.

If it is dried, it becomes necessary to employ it in a spiritous tincture; or, if still drier, in powder; but it has much less effect than the recent juice, as, by drying, it parts with

* This substance is very similar to the gum Elemi of the shops. We employed it as a pitch for stopping our corials and canoes in navigating the Essequebo. Various other species are eligible for the same purpose.

its essential oil, and is reduced almost to the state of a simple resin.

I may take this opportunity of observing, that the vapours of the balsamics, as hyowa, capivi, and laurel oil, may have soothing effects, and a healing tendency, after or along with those of the mineral ones, as of mercury, arsenic, &c. especially if a gentle degree of heat, as that of boiling water, be used to elicit them, producing no decomposition of the essential oils, which must ensue with heated stoves or irons, and which must also occur in the process of boiling tar, as it requires a much greater degree of heat.

It is reasonable to suppose that these volatile oils, in a state of vapour, conjoined with that of hot water, must excite some sensible action on ulcerated surfaces. The volatile oil itself will thus be actually applied to the diseased part.

It will probably be less offensive or irritating to the tender organs, than the fuliginous matter and carburetted hydrogen, evolved from tar, and the like resinous substances, in a state of combustion,—as in this case the volatile oils will be separated, and rise along with the watery vapour; and it is certain, as asserted by Mr Brande in his *Elements of Pharmacy*, that the active principle of balsam capivi resides in its volatile oil.

The three substances just cited, viz. hyowa, capivi, and laurel oil are well proved to possess balsamic or healing properties in an eminent degree; they are capable of direct application to the lungs in a state of vapour *undecomposed*; and, therefore, we are encouraged to expect some benefit from them as topical applications, used in conjunction with general constitutional alteratives.*

In its recent state it is an excellent balsamic in ulcerous disorders of the bladder and urinary organs, and especially if made use of in very liberal doses.

Arakusiri.

This is another species still more fragrant but less abundant. It is known amongst the Arowaks by the name of Ara-ku-siri.

* For a mercurial fumigation, calomel seems, from trials, the best preparation; but, for external sores, I should, from trials made with different forms of the mineral, be inclined to give the preference to corrosive sublimate.

The tree producing it is the *Icica aracouchine* of Aublet, *A. heterophyllæ*, as improperly named by Willdenow, which would convey an idea that the leaves were of different forms. The folioles, however, are alike constantly pinnate, with an odd one, varying only in this, that there are only one or two pair of wings, and most pinnate plants vary much more in number.—It ought to be named *A. odoratissima*. Aublet calls it a middle-sized tree, and assigns it twelve or fifteen feet in height.

This is perhaps the most odoriferous balsam known, not even excepting the true balsam of Gilead, *Amyris Gileadensis* or *opobalsamum*.—It has the consistency and appearance of honey when recently drawn from the tree.

I have a sample of the gum which I brought from the Macosis country about fifteen years ago, and it still retains much of its native odour.

It is adapted to the same useful purposes as the hyowa.

Its estimable qualities recommend it both as an internal and external remedy. It is indeed an excellent vulnerary; and, inwardly, an admirable detergent and corroborant, in gleets, leucorrhea, seminal weakness, mucous discharges from the bladder, &c. It also promotes digestion, and stregthens the stomach and nervous system.

It may be taken in doses of from ten to forty drops if fluid, or as many grains if inspissated, beginning in the smaller dose and increasing in gradation. It imparts a scent to the urine similar to that which ensues from taking the laurel oil; and their action on the system are in most cases probably nearly identical, especially if the Arakusiri be recently drawn and kept in a fluid state, secure from the air, as otherwise the more volatile and useful part exhales, leaving a gum resin to predominate, which, in contact with nervous expansions on delicate membranes, as those of the eyes and urethra, would, like most other balsams, prove too irritating.

Its antiseptic nature, plasticity, and grateful flavour, render it a useful masticatory when inspissated, preservative of the teeth and gums, sweetening or correcting fetid breath, and at the same time strengthening the stomach. It does not, like mastic, turn hard and brittle in the mouth, and, although ıewed an hour daily, it will retain its aromatic bitter for eeks.*

Mani.

Whilst on this subject of the gummy products of Guiana, t would not be right to pass unnoticed that of Mani. This s the name it is known by in Guiana, as well as at Cayenne, ınd on the Rio Negro. On the Orinoko, this black resin is :alled Paraman, and by the Arowakes Caraman.

The tree, as suggested by Humboldt, is the *Moronobea coc- :inea* of Aublet. It is abundant on the back of the Deme- rara coast. The wood, which is white and soft, is used chiefly as heading for sugar hogsheads.

It is collected, they say, chiefly by the Piarsa Indians of the Raudalas of Atures, who prepare it by boiling. It is of most extensive use throughout Guiana, as fastening for the arrows and every purpose to which shoemakers wax might be applicable, as well as for candles. It forms an important article of traffic amongst the natives of Guiana, to whom it is almost as necessary as gas light to the inhabitants of London.

I have seen at Angostura quantities of the three last mentioned substances from the Rio Negro, abounding with dirt and impurities, thrown together, and sold under the name of brea and carana indifferently.—It is this which has led M. Humboldt into perplexity; and the inhabitants of Angostura, ignorant of those products, were unable to satisfy his inquiries.

* To this account of *Arakusiri,* I may subjoin an old notice of it by Philippe Fermin, in his *Descrip. de Surin. p.* 83.—" La frugalité de ces peuples les met à l'abri de presque toutes les incommodites que nous connaisons, si l'on en excepte la caducité qui les oblige à rester dans leur *bumac,* et s'il leur en survient, ce qui est fort rare, ils sont leur propres médecins et chirurgiens, et si ont pour tous remedes que quelques huiles, qu'ils prennent interieurement, et un excellent baume qu'on appelle *racaciri.* Ce baume sort d'un arbre des environs de la riviere des Amazons; on le fait découler, dans un calebasse, par des incisions qu'on a faites dans l'arbre. Cèst un souverain remede pour toutes plaies récentes, de même que pour les vieux ulceres, en l'appliquant en forme d'emplâtre, le plus chaudement qu'il est possible."—" Il est encore fort salutaire pour la poitrine, et infaillible pour arrêter les fleurs blanches et les vieilles gonorrhées."

Two intelligent Indians of the Mandavac tribe, from the Rio Negro, informed me, that carana is the Mandavac name for the gum before spoken of, and that the tree is there called Waia-waia; that the Hyowa tree and its gum is there called Mana; and that Mani is their name both for the tree and gum which we know by the same name.

Simiri.

This is the resin of the *Hymenea Courbaril*, the produce of the colony of Demerara upon the high lands.

This substance might probably, by drawing it in a bottle, be maintained in a fluid state, so as to be ready at any time for application as a varnish.

I find that it is insoluble in oils both fixed and volatile, in alcohol, and not at all acted on by the alkalies, not even when they are boiled upon it.—Is it not, then, improperly denominated a resin? The reverse of this, or solubility in these substances, constitutes the chief distinguishing character of the resins. It seems to possess more of the characters of the amber than of any thing else. Its fracture is conchoidal like that of amber. It would be interesting to know whether any thing analogous to the succinic acid or oil could be obtained from it. It appears to possess the hardness and lustre of amber; and it might answer equally well for the manufacture of ornaments. Both these substances burn with the same aromatic odour and they leave a similar coal. Amber indeed is said, by chemical writers, to be soluble in the alkalies. This, I apprehend, is not strictly correct, as simiri is not so, although assisted by heat, as before observed, whilst both readily dissolve common resin, which Mr Brande says is a perfect example of resin. It must have been its resinous appearance more than its chemical properties which has given to copal or the simiri a place amongst the resins.

Ducali.

This milky substance is produced very abundantly on making an incision in the tree called by the Arawaks Ducali.

The tree grows very large, and is plentiful in the vicinity of the coast in sandy soils.

The tree is not described; but it bears a large apple containing several oblong seeds, and it appears to belong to the family *Sapotaceæ*.

The ducali is a substance, differing from all others, perhaps, with which naturalists and chemists are acquainted. It is milk white and thick as new cream. Its taste is slightly bitter and sourish. It is diffusible and miscible in water cold and hot, and remains unchanged thereby.

On mixture with spirit (proof 18), it instantly forms a solid elastic mass, strong like cahuchi, but growing brittle on drying, or even though remaining in the liquid. This cake is white, and half the bulk of the milk used. Beside the cake there is a white curdy loose precipitate which falls from the liquor.

The milk is not changed or at all acted on by the mineral or vegetable acids that I have tried, viz. the nitric, sulphuric, oxymuriatic, or acetic, though both strong and diluted, were tried. No change takes place with carbonate of potass, lime-water, or oxymuriate of mercury.

The only two substances yet found to affect it are the acetate of lead and the nitrate of silver. The latter throws down a reddish precipitate; the former, a copious white, half curdy precipitate. The supernatant liquor filtered is not affected by alcohol; but, inversely, the filtered spirituous tincture lets fall a blue feculent precipitate on adding the acetate of lead. It is evident that the acetate of lead unites with both the cake and the loose precipitate, and even with that part held in solution by the alcohol or by the watery part of the spirit, which now sinks.*

The cake or coagulum appears to me a singular substance. It is not soluble in any proportion in water or alcohol, nor in the strong mineral or vegetable alkalies. It has all the appearance of a resin, soft and adhesive while moist; when dried,

* The articles expected to unite or precipitate each other should not be too much diluted. If much diluted with water, they require a proportionally longer time, perhaps may scarcely act at all. I find the best method is to put them together strong, *i. e.* both well concentrated, and afterwards dilute the mixture. In this way, the experiment will always succeed, if they are substances which mutually act on each other. They must be strong, in order effectually to act on each other, and diluted to show their action, especially if viscid.

pulverizable, softening with heat (not inflammable however), and no part of the powder is soluble in spirit or water, cold or hot, nor in the strong vegetable and mineral acids or alkalies, &c. thus differing in chemical affinity from the resins, from cahuchi gum, gum resins, &c.

The milky fluid of ducali is, as already said, instantly coagulated to a hard mass by addition of alcohol, although heat has no such affect upon it; thus in one instance appearing to be referable to albumen, but not in the other; and it is singular, that though from its insolubility in this menstruum, one would expect to find the alcoholic coagulum altogether differing from a resin, yet like resins it is liquified by heat, burning like them, and soluble also in oils. It therefore seems to be allied more in its nature to wax than to any other of the vegetable proximate principles.—It would probably serve for candles after being washed with spirit.

The milk is much employed by the Indians as a dressing for yaws and other foul sores.

Caoutchuc or Cahuchi tree;—in Arowak, Haatie;—in Acawai, Kináh;—in Caribe, Póme.

It grows abundantly on the Sipperuni; and other branches of the Essequebo, and along the Tapacoma.

This tree is the *Siphonia elastica* of botanists.—The flowers are small and so very scarce and caducous, that it is difficult to procure a dried specimen with them attached. The fruit has three seeds covered with a pulpy capsule, which gives it precisely the form of an apple.—On showing one of them to a Carib and asking its name, he answered me almost in Latin "Pomæ," with a short sound of the penultima. It is the tree *Siphonia*, however, which they call by this name, and not the fruit.

The Macosis make balls of it as toys for their children to play with; and, so elastic are they that they will rebound several times between the ceiling and floor of a room, when thrown with some force.

Large quantities of the caoutchuc might be found at the Essequebo; but I have not learnt at what season it flows most abundantly.

We find, moreover, a number of different lactiferous trees in Guiana, some of which, as the Haia-haia and Ducali pour it out in great abundance at certain seasons. They also contain a portion of elastic gum; but too much modified with other substances to be used as such, unless means could be found of separating it.

Balsamo Real, or Royal Balsam.

This is produced by a species of *Amyris*. In scent it is very like our hyowa, but more balsamic or glutinous; the hyowa soon becoming dry and brittle. I believe this substance to be in no respect inferior to the true balsam of Onaica, or the produce of the *Amyris gileadensis*.

This I know to be a valuable article as a vulnerary, and a pectoral remedy; and it is considered, I believe, with justice, to be one of the best balsamics in cases of inward ulceration. It forms also an excellent detergent and healing ointment for old ulcers, prepared after the manner of the Ung. Elemi, with the addition of a little calamine stone.

The dose is about a drachm, once or twice a-day, beat up with yolk of egg, or new milk and sugar.

Vesicamo.

I may here allude in a cursory manner to a new resinous substance with which I am very little acquainted. It is an exudation of a deep sea-green colour, strongly adhesive, and about the spissitude of crude or Venice turpentine.

It is procured, by incision, from an unknown tree growing up the rivers Barima and Amakuru. The Indians say the tree is very much like the Bisi or Bishi, of which they chiefly form their canoes and corials, a wood also resembling cedar.

A small quantity of this resin was procured by Mr James Fraser, on a journey to the Orinoko. Having brought a sample with me to London, I have recently committed it for examination to a gentleman of the highest chemical talents, Professor Brande.

It is not one of the aromatic resins, or has only a peculiar odour, much fainter than most of those whose resin is modified by an essential oil.

Kofa.

Amongst the multitude of *Guttiferæ* found in Guiana, we must not omit to notice the great Kofa vine, which, although a climber, grows to the size of a man's body. It is a species of *Clusia*.

This great parasitic bears a large and fragrant flower, in the disc of which is found a species of vegetable wax of a yellow colour, soft and adhesive.

On striking through its bark with an axe, it gives out its milky fluid in a stream which on drying acquires a brown and resinous aspect.

Of another genus, namely *Vismia*, there are several species which likewise yield most abundantly a bright blood-coloured adhesive resin, said to be a strong cathartic, equal to gamboge.

These trees grow to a considerable size.—They are the blod-hout of the Dutch creoles, the Woraly of the Arowaks. They must not be confounded with the Worari or arrow-poison,—a mistake I have noticed in the book (*Wanderings*) of my friend Mr Waterton.

The writer of this paper has samples of most of the resinous substances which are mentioned in it.

Art. VI.—*Observations on Turtles, &c.* By Dr Hancock. Communicated by the Author.

The Tortuga.

The Tortuga or large fresh water turtle travels far at times. It deposits its eggs in the sand with surprising address. The land turtles, it is said, are most stupid in this particular, dropping their eggs, one by one, as they hobble over the ground, neither covering nor taking any care of them whatever, nor paying any regard to their offspring. The tortuga, on the contrary, covers its eggs so accurately as to leave no signs perceptible of its nest; and, however strange it may seem, she so arranges it as to make her track appear unbroken over the sands, and, after laying her eggs, she proceeds on again in the same direction to complete the deception.

I should certainly be inclined to doubt this fact, if I had not witnessed it myself in a number of instances in the Essequebo, and the same is attested by the Indians, and every one acquainted with the subject.

The Matta-matta.

This is a very uncommon species of turtle. The shell is very uneven, marked longitudinally with six prominences, three on each side. The margin of the shell has many angular indentments. Its legs are covered with thick strong scales, and its feet palmated, with five nails on each foot. Its tail is three inches long.

The most remarkable parts of this animal are the head and neck. The head is angular, depressed or flattened, and resembles that of an alligator. The head and neck are disproportionately large, and abound with irregular cutaneous appendages or prolongations of the skin, rough and wrinkled, forming a truly distorted and hideous figure.

The matta-matta forms an anomaly of the turtles as the *Pipa* does of the frog kind. In fact, there is a remarkable resemblance in the head : both the matta-matta and *pipa* have the flat angular front, and are extended at the ears.

The eyes are small and situate near the nostrils, which, as in other species of turtle, are close to the apex of the upper mandible. The tongue is short, broad, and cuneiform.

The length of the shell is about 19½ inches ; the breadth 14. The breadth of the head is about seven inches, and the girth of the neck 14 inches.

The matta-matta that we took near the head of the Repoononie or western branch of the Essequebo, was laying quietly on the surface of the river, and allowed the Caribs to approach and lift him into the canoe without making any resistance.

Whether they are naturally so very sluggish, or sleep in this manner, and, like the owls, see badly by day, seems to me a matter of doubt. The Indians said it was not good eating ; but they used it in default of other food. These turtles are not numerous in Guiana. This was the only one we observed during a journey of near eight months duration.

Caspan—the largest of the fresh water turtles of Guiana.

Both the flesh and eggs of the Guana are by many people esteemed a great delicacy. I have tasted them, but cannot say I think very highly of either. The flesh and eggs of the turtles, both of the land and rivers, I consider vastly superior, whilst they excel in point of flavour. The greater portion of gelatine must likewise render them much more nutritive.

The Caspan (so called by the Dutch,) are exceedingly numerous up the Essequebo; and their eggs are exceedingly luscious and nutritive. They contain a great proportion of oil, which resides in the yolk, and is easily separated by maceration in water.

The Indians procure this oil in great quantities merely by throwing the eggs into a corial (a species of boat), mashing and throwing water on them, which causes the oil to rise on the top, whence it is skimmed, and, when settled for a day or two, is quite clear and pellucid. It is a very wholesome and useful kind of oil for the kitchen, and is in common use in the Orinoko as a culinary article.

The eggs of birds contain little oil comparatively. Those of the domestic fowl afford about an ounce of oil to the dozen; but they seem to require a particular operation to elicit the oil. They are first boiled hard, and the yolks are then taken out and roasted, a small quantity of oil being pressed out whilst hot. Thus they may be said to require the dry operation: no oil could be obtained by the moist one. This oil has been much celebrated for certain purposes in medicine. It is not probable, however, that it possesses any superiority over other bland oils in general.

When we reflect that the yolk of egg is employed as one of the most efficient substances for rendering oils miscible with water, a question naturally arises, How the oil can be separated and collected from the yolks by the above process, for no oil could be gathered by that method from the eggs of the domestic fowl, or of any other birds perhaps?

Probably we are to account for it merely from the superabundance or supersaturating quantity of the oil contained in the turtle eggs, and must conclude, that doubtless a very considerable portion of the oil is rendered miscible, suspended in the water, and consequently thrown away with it. Still a large return of oil is procured.

This also is a singular fact,—that while the white of the eggs of birds consists almost entirely of albumen, (and so speedily hardened by the heat of boiling water,) the white part of the turtle's egg seems to be rather a gelatinous substance than albuminous, and appears to contain less of albumen than the yolk; for the latter soon becomes hard on boiling, whilst the white remains liquid.

ART. VII.—*Thoughts on the Deluge* *. Communicated by a Correspondent.

THE principal and most important object of the Holy Scriptures being to instruct us in the material doctrines and duties of our religion—to show us the origin of all things from an Almighty power—to teach us concerning the creation of man, his fall, and redemption, we are not to expect in them a perfect system of philosophy or correct ideas in science.—"It is plainly no part of them to guide men's opinions, or to inform their minds on any subject except that of religion and morals, and consequently, the writers of them were probably left as uninstructed on other points as other men. For example, they no doubt laboured through their lives under the same mistaken popular ideas that prevailed in their age and country regarding the order of the universe, the globe we inhabit, and every subsequent acquisition and improvement in science.—In every other particular besides religion, mankind was left to the natural progress of human intellect and human experience." (Slightly altered from *Four Letters on Religion*, by a Layman. Bath, 1801.)

To instruct us in philosophy is not their object, but a far more important one, to instruct us in all things necessary for our salvation.

The investigation of the truths of science is left to employ

* We have inserted this paper at the request of a much esteemed Correspondent, although we do not adopt the views which it contains.—ED.

the mind of man here, on whom talents have been bestowed for that purpose; and they, if pursued with a proper feeling and desire of spiritual improvement, all tend to lead him to a more elevated idea of the almighty power and beneficence of his Maker, the great First Cause,—" to look from nature up to nature's God."

We generally find in the Scriptures, that on subjects connected with natural philosophy, the expressions are adapted to the popular ideas, and infant state of man's knowledge at the time they were written. Had it not been so, a revelation would have been necessary to make their language intelligible to man, which, however, was only given to explain to him those more important truths regarding his immortal soul.

In the Bible the terms *earth* and *world*, very often comprehend only the then known parts of the globe,—as indeed has been the case in more modern times, so that, after the discovery of America, *it* was called the New *World*, to distinguish it from that previously known,—which terms of the " New " and " Old World " are still in frequent use.

In Scripture these words are often used, even in a still more limited sense, to express that part under the dominion of the Jews or of the Romans.—In the same limited acceptation I understand it to be taken in the account given us of the *deluge*, and that this catastrophe was not universal, but confined to that district of the globe which was then inhabited by man, on whose account it was so visited.

Noah was directed to take with him into the ark certain animals, which would be of use to him when he left it,—to serve him for food, and to replenish that part of the earth which he would otherwise have found destitute of provision for him, all other animals there having with man been destroyed.

According to the history of the deluge given by Moses, the rising and retreat of the water appears to have been very gradual, and therefore I do not conceive that *it* can have had much effect in altering or destroying the surface, which would thus have been rendered unfit for the habitation of man, or even for that of animals, as all vegetation would be destroyed by such convulsions.—That it was very gentle, even so as not to have uprooted the trees, may perhaps be inferred from the dove having plucked an olive leaf only when the waters had abated, which must have been from a tree which was still standing.

The organic remains found in the old alluvium and generally attributed to the deluge, themselves, I think, tend rather to prove that they are of a different epoch, as they mostly belong to *species* of animals which are now extinct.—The alluvium in which they occur I would refer to a more ancient period, even to one before the creation of man, whose bones, I may remark, have never been discovered in it.

One of the most difficult facts to account for in any other way than by limiting the deluge to those parts of the earth inhabited by man, is the occurrence of certain species, and even genera and families of plants and animals, peculiar and confined to countries far separated from each other, to which we cannot imagine them to have travelled from the ark, without having stocked the tracts they would pass through; or introduce them in any other way than by supposing them to have been left from their first creation—unless we have recourse to a miracle, a new creation,—a solution which I think should in no case be adopted, but when there appears no secondary way of accounting for a fact, and even then with great caution, as our ignorance may be owing only to the limited knowledge of mortals concerning "the wondrous works of Him who is perfect in knowledge." (Job, xxxvii. 16.)

February 1828. W. C. T.

ART. VIII.—*On the Mean Temperature of* TWENTY-SEVEN *different places in the State of New York for* 1828.

IN the sixteenth number of this *Journal* we have given a brief abstract of the "Returns of Meteorological Observations made to the Regents of the University by sundry Academies in the State of New York" for the year 1826. Owing to some accident we have not received a copy of the Report for 1827; but as that for 1828 has just reached us, we shall proceed to lay before our meteorological readers a summary of its highly valuable contents. The abstract of the returns contained in the report were prepared by Mr T. Romeyn Beck and Mr Joseph Henry.

In 1826 the number of complete returns were *ten*, and the number of incomplete ones *twelve*, making *twenty-two* in all; but in 1828 there are *twenty-four* complete returns, and *nine* incomplete, making *thirty-three* in all.

In our analysis of their First Report we took the liberty of pointing out some defects, which, in the true spirit of science, the preparers of the present report have done their utmost to supply. These defects related to the positions and altitudes of the places of observation, and to the hours at which the observations were made. The first of these defects is partly supplied by the following table.

List of academies reporting.	N. Lat.	W. Lon.	Observers.
Albany,	42°39′	73°47′	T. Romeyn Beck, M. D. Principal.
Auburn,	* 42 55	76 55	Rev. John C. Rudd, D. D. Principal.
Cambridge, Washington,	* 43 02	73 42	Rev. N. S. Prime, Prin. and M. Stevenson, M. D. a Trust.
Canandaigua,	* 42 53	77 56	Henry Howe, Prin. and J. G. Thurber.
Cherry-Valley,	* 42 48	75 06	Dr William Campbell.
Clinton,	41 00	72 19	Hon. Jonathan Dayton.
Delaware,	* 42 17	75 16	Stephen C. Johnson, Principal.
Dutchess,	41 41	74 45	Eliphaz Fay, Principal.
Erasmus-Hall,	40 37	73 58	Jonathan B. Kidder, Instructor.
Fairfield,	43 06	74 52	J. J. H. Kinnicut & David Chassel Jun.
Greenville,	* 42 25	74 21	E. B. Wheeler, Principal.
Hamilton,	42 48	75 32	Zenas Morse, Prin. [G. B. Miller] Prof.
Hartwick,	42 05	74 55	Rev. E. B. Hazelius, D. D. Prin. & Rev.
Hudson,	42 15	73 45	J. W. Fairfield, Principal.
Ithaca,	42 26	76 30	S. Phinney, Principal.
Johnstown,	43 00	74 08	Rev. A. Ammerman, Trustee, & A. Bennet and X. Haywood, Teach.
Lansingburgh,	42 48	73 46	Alexander MacCall, Principal.
Lowville,	* 43 47	75 51	Stephen W. Taylor, Principal.
Middlebury,	42 49	78 10	Seth Cushing Jun. Principal.
Montgomery,	41 32	74 00	Peter A. Millspaugh, M. D.
Onondaga,	43 02	76 31	Samuel B. Woolworth, Principal.
St Lawrence,	44 40	75 00	J. B. Hale and Ira Pettibone, Teachers.
Union-Hall,	40 41	73 56	Pierpont Potter, Teacher.
Utica,	43 06	75 12	D. Prentice, Principal.
Washington,	* 43 08	73 41	William Williams, Principal.
Newburgh,	41 30	74 05	Wm. S. Burt & Nathan Stark, Prins.
Pompey,			E. S. Barrows, Prin. H. Howe, and A. Huntington.

* The latitudes and longitudes marked * were found by approximation.

On the subject of the time at which the observations are made, the report states, "that they are taken as early as possible in the morning (say 6 A. M.);—at 3^h P. M., and an hour after sunset. The mean is calculated by adding together the morning's observation, twice the afternoon's and evening's, and the next morning's, and dividing the amount by six."

The learned individuals who have prepared the report do not state upon what grounds the preceding hours have been adopted, and we can scarcely conceive a reason for adopting two fixed hours, viz. about 6 A. M., and at 3 P. M., and the variable time of an hour after sunset. Professor Dewey found that 7^h A. M., 2^h P. M., and 9^h P. M., gave in North America the same result nearly as that of 24 hourly observations; and in the *Edin. Phil. Journal*, vol. vi. p. 352, we have shown, from the observations made by Professor Dewey, that *two observations at* 10 *A. M. and* 10 *P. M.* gave a result still nearer the mean daily temperature. Since that time the hourly meteorological journal has been kept at Leith; and on the authority of its results we would recommend to the Regents of the University to fix for the hours of observation any two similar hours, such as 8^h A. M. and 8^h P. M.; or 9^h A. M. and 9^h P. M.; or 10^h A. M. and 10^h P. M.; or, what is still better, adopt the two instants of the mean daily temperature, viz. $9^h\ 13'$ A. M., and $8^h\ 27'$ P. M.

With regard to the rule for calculating the mean adopted in the report, we think there must be some oversight; for the equation which expresses it seems to us to be identical with that which gives the simple mean of the three observations. Thus let a, b, c, d, be the four observations employed, and M the mean daily temperature, the rule for the mean given in the report is

$$\frac{a + 2\,b + 2\,c + d}{6} = \mathrm{M}$$

Now, since a and d are observations made at the same hour in the morning of two consecutive days, they must be *nearly the same*, or, what is more correct, in the calculation of averages, their difference must be inappreciable. Hence we may make $a = d$ and $a + d = 2\,a$, and the equation becomes

$$\frac{2\,a + 2\,b + 2\,c}{6} = \mathrm{M} \text{ or } \frac{a + b + c}{3} = \mathrm{M}$$

so that all the labour of doubling *b* and *c* and adding *d* may be saved, without perceptibly affecting the daily averages.

We may therefore regard the daily averages in the report as equivalent to the mean of the three ordinates of the daily curve at 6h A. M., 3h P. M., and an hour past sunset; so that if we suppose an hour after sunset to be a little colder than the mean temperature of the day, the periods adopted should give very nearly the mean daily temperature; for as the maximum temperature takes place about 3h P. M., and the minimum before 6h A. M., the mean of these two will be a little higher than the mean temperature of the day, and this mean being again combined with the observation after sunset, which we suppose a little lower than the mean temperature, will give a result not very far from the mean temperature required. Had the third observation been at a fixed hour, we could have calculated exactly by means of the Leith results the difference between the averages in the report and the mean temperature of the twenty-four hours.

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

In the year 1826 the mean temperature of *ten* of the above places was - - - - 49°. 4
In 1828 the mean temperature of twenty-three places is 49 .99
The mean temperature of a point in the State of New York, corresponding to the mean position of all these places, is, according to Dr Brewster's general Formula, - - - - - - 49 . 8
So that the formula must err in defect, as the mean altitude of the different places must be considerable.

Albany, -	30.24	35.24	39.61	45.85	61.46	74.04	71.11	72.92	62.17	48.50	39.77	35.42	51.36	96	2	94
Auburn, -	30.25	34.34	38.16	42.69	59.47	70.57	69.37	67.64	60.84	49.73	41.67	34.85	49.96	94	— 4	98
Cambridge, -	29.36	33.36	36.75	41.71	59.16	70.10	68.96	68.03	57.49	45.48	40.29	33.80	49.00	92	—13	105
Canandaigua,	27.17	29.62	38.53	43.53	56.70	70.03	78.61	76.46	59.58			34.31	50.71*	94	0	94
Cherry-Valley,	26.93	31.94	34.74	39.20	54.13	69.55	67.34	68.92	59.06	46.73	36 79	32.66	47.08	96	—18	114
Clinton, -	35.72	38.72	39.01	42.60	54.00	65.02	69.45	69.47	63.92	52.97	46.11	38.53	51.29	92	11	81
Delaware, -	31.08	34.08	36.78	38.18	54.87	67.80	65.13	67.58	54.18	42.68	38.18	32.12	46.89	93	—17	110
Dutchess, -		42.44	44.55	50.60	63.29	75.16	75.25	80.63	67.07	54.57	47.51	42.23	58.57†	91	— 0	91
Erasmus-Hall,	36.75	41.28	42.13	45.57	58.88	70.12	71.88	73.45	64.47	53.05	46.30	40.31	53.68	92	9	83
Fairfield, -	24.09	32.80	33.12	39 03	53.11	72.96	69.75	69.68	58.77	44.50	35.69	30.69	47.01	93	—14	107
Greenville, -	30.38	34.99	37.87	44.88	59.59	71.65	70.89	75.83		48.01	41.30	34.08		92	— 2	94
Hamilton, -	26.38	32.63	36.05	40.76	56.22	66.74	65.00	68.29	60.87	46.97	37.65	32.00	47.46	95	—20	115
Hartwick, -	28.27	32.16	35.60	39.88	56.92	68.40	65.03	67.37	58.37	46.72	38.18	26.42	46.94	92	—14	106
Hudson, -	30 20	35.34	39.81	45.33	61.56	72.87	73.46	76.57	64.79	53.31	43.61	37.27	52 84	99	2	97
Ithaca, -	33.68	35.57	43.04	44.28	60.48	69.24	69.62	70.64	61.15	49.36	42.10	37.73	51.35	96	8	88
Johnstown, -	27.58	31.60	34.40	40.98	57.17	70.33	68.20	71.93	55.97	46.69	37.91	31.96	47.89	96	3	93
Lansingburgh,	29.02	34.32	38.03	44.58	59.86	71.77	70.13	72.87	62.93	49.57	41.17	34.78	51.71	96	—12	108
Lowville, -	25.76	31.09	33.46	40.61	57.88	69.27	65.58	68.74	59.28	46.26	36.83	28.51	46.95	99	—24	123
Middlebury,	31.52	35.67	39.75	41.16	56.98	68.48	66.93	69.54	59.37	49.39	44.87	36.25	49.62	94	4	90
Montgomery,	28.42	38.34	40.86	45.73	59.38	72.72	70.71	73.39	64.97	50.49	43.41	36.79	52.18	100	— 5	105
Onondaga, -	33.50	38.39	42.24	44.64	60.47	72.40	68.33	73.52	63.04	46.40	35.12	32.79	50.90	99	—15	114
St Lawrence,	22.75	28.87	30.80	37.47	57.15	68.28	66.21	68.63	58.45	46.31	38.86	28.43	46.01	95	—13	108
Union-Hall, -	34.85	39.52	40.10	43.83	57.55	69.04	70.21	72.24	63.16	52.35	44.02	37.82	52.05	90	8	82
Utica, -	28.72	33.39	36.79	42.93	60.48	73.37	70.66	72.89	62.34	49.32	39.59	34.30	50.40	97	—16	113
Washington, -	30.72	33.62	36.17	40.95	58.05	69.76	68.34	69.52	59.26	45.31	39.39	32.48	48.63	90	—14	104
Newburgh, -	32.10	37.52	39.81	45.08	59.12	71.27	71.36	73.00	63.11	51.26	42.57	37.73	52.00	99	4	95
Pompey, -	26.32	30.90	34.35	37.44	58.39	71.01	68.88	71.18	58.41	45.27	36.21	29.67	47.33			

* Deduced from the observations of the ten months. † For the eleven months.

The following table shows the quantity of rain and snow which fell in the state of New York in 1828.

TABLE II.

	Inches.		Inches.
Albany, -	37.66	Ithaca, -	24.45
Auburn, -	34.91	Johnstown, -	40.39
Cambridge, -	43.68	Lansingburgh, -	37.91
Cherry-Valley, -	34.39	Lowville, -	35.48
Clinton, -	30.91	Middlebury, -	38.42
Delaware, -	28.85	Montgomery, -	40.36
Erasmus-Hall, -	45.14	Onondaga, -	35.79
Fairfield, -	45.51	St Lawrence, -	35.67
Franklin, -	25.86	Union-Hall, -	48.91
Greenville, -	30.84	Utica, -	36.57
Hamilton, -	34.18	Newburgh, -	43.30
Hartwick, -	32.67	Pompey, -	33.47
Hudson, -	43.25		

In 1826 the mean quantity of rain which fell at the nine places where observations were made was - 36.34

In 1828 the mean quantity of rain which fell at the twenty-five places in the table was - - 36.74

Mean, 36.54

So that we may regard the mean quantity of rain which falls in the State of New York as very accurately established. The result corresponds pretty well with the theoretical quantity calculated in the article Hygrometry in the *Edinburgh Encyclopædia* for a latitude corresponding to the mean latitude of the State of New York.

The following table contains some interesting miscellaneous observations.

TABLE III.

		Places.
Lilacs in blossom,	March 28,	Delaware.
Mezereon do.	March 29,	Albany.
Currants do.	May 2,	Albany.
Shad bush do.	May 3,	Albany.

		Places.
berries do. -	May 3,	Albany.
do. -	May 5,	
s do. -	May 15,	
ɔts do. -	March 24,	Union Hall.
y do. -	March 31,	
t trees do. -	June 12,	
ıut trees do. -	July 2,	Cambridge.
lil do. -	March 28,	Erasmus Hall.
rees do. -	May 10,	Clinton.
ood do. -	May 4,	
do. -	May 10,	Franklin.
·lions do. -	May 17,	
berries do. -	May 9,	Clinton.
ican Poplar do.	May 10,	Fairfield.
Root do. -	April 19,	
ı Corn in silk,	July 19,	Clinton.
birds seen -	March 10,	Delaware.
swallows seen -	April 24,	
ıs seen -	March 10,	Dutchess.
berries ripe, -	June 9,	Cambridge.
nts ripe, -	July 8,	Franklin.
ies ripe, -	July 5,	Clinton.
ıarvest began -	June 25,	Dutchess.
ripe apples, -	July 16,	
ıt harvest began,	July 10,	
ı ripe, -	Aug. 27,	Kingston.
ıes ripe, -	Aug. 29,	
swallows disappeared end of August,		Cambridge.

fall of snow, In October in some places, and in November in others.

killing frost, In the middle of October almost every where.

MISCELLANEOUS OBSERVATIONS.

rora Borealis, noticed January 18, at Dutchess Acade-
ʔranklin, Hartwick, Johnstown, Middlebury, Lowville,
daga, Utica.

ı. 19, at Albany.

ı. 20, at Auburn, Clinton.

b. 3, at Johnstown, Utica.

Feb. 19, at Utica.

April 11 and 12, at Hartwick.

July 5, at Albany, Dutchess, Lowville, St Lawrence, Utica.

August 14, at Clinton.

August 16, at Cambridge, Lowville, Utica. On the evening of the 16th, the aurora borealis exhibited a beautiful bow of light. In its position and appearance, it was so similar to the one which happened in September of the previous year, that a particular description is thought unnecessary. Its arch, however, was less complete, and the time of its continuance shorter.—(Utica.)

Sept. 8, at St Lawrence. The coruscations extended nearly to the zenith, illuminating about one-half of the horizon, and very brilliant.

Sept. 12, at Utica.

Sept. 26, at Albany, Auburn, Erasmus-Hall, Lowville, Johnstown, Clinton, Schenectady, Lansingburgh.

Sept. 27, at Cambridge.

Sept. 29, very brilliant coruscations at Albany, Cambridge, Middlebury, Johnstown, Schenectady, St Lawrence, Utica, Lowville. Vast segment of a dusky area and luminous arch —lively coruscations; stars distinctly seen in the area. It continued several hours.—(Lowville.)

Sept. 30, at Dutchess.

Oct. 3, at Cayuga.

Oct. 8, at Albany, Dutchess. At 12 P. M. a brilliant arch, at right angles to the magnetic meridian, about 5° wide, and the crown about 10° above the horizon.—(Albany.)

Oct. 11, at Hartwick.

Nov. 8, at Utica.

Dec. 1, at Clinton, Schenectady.

Mock Suns, Haloes, &c.—Jan. 23, two mock suns appeared north and south of the sun at its setting.—(Johnstown.)

April 24, A. M. noticed a mock sun.—(Union-Hall.)

Jan. 31. Circle round the moon, colour of the rainbow.—(Erasmus-Hall.)

Feb. 8. Circle round the moon.—(Utica.)

Jan 2 and 30. Circle round the moon.—(Union-Hall.)

April 17, 18, 19. Solar haloes before noon, and visible for several hours.—(Lowville.)

Nov. 18. Lunar halo of extraordinary magnitude.—(Lowville.)

April 28. Rainbow formed by moonlight.—(Johnstown.)

May 4. Rainbow after sundown. The afternoon had been cloudy and showery. The showers were slight, but continued at intervals till after sundown. The wind having changed to the southwest, had elevated the clouds in the southwest and west, several degrees above the horizon. The sun went down clear and brilliant, while the rain continued falling in the east and over head. After the sun had been several minutes below the horizon, our attention was directed to a large and well defined rainbow in the east—its arch a little more elevated than it usually appears, and its ends terminating by fainter colours at about 30 degrees above the horizon.—(Utica.)

Meteor.—Sept. 6. Extraordinary meteor observed at half-past four P. M. by three persons at different stations in this village and its neighbourhood. When first observed, it was on the meridian, altitude 45°; the apparent diameter of the globular part, or body of the meteor, was at least two feet; apparent length of the blaze (conical) which it drew after it, was between thirty and forty feet, and the body of the meteor and its blaze were bright as the flame in an oven. A faint white light, resembling a long and narrow white cloud, remained for some time in the region through which the meteor passed.—(Lowville.)

Variation of the Compass.—Sept. 20, observed the variation of the compass at $10\frac{1}{2}$ A. M. 6° 16′ W.—(Albany.)

Sept. 22, observed do. at 7 A. M. 6° 12′ W.—(Albany.)

Rain, Snow, &c.—June 18. About 10 A. M. a sudden and violent thunder shower, accompanied with wind and hail, arose from the west. After it had subsided, it was observed that the surface of pools and vessels of water was slightly covered with a dark yellow substance, which the credulity of some supposed to be sulphur. On a careful examination of the ominous matter, it was concluded to be the pollen of plants, perhaps of wheat, which had been disengaged by the wind. The appearance attracted more attention, from the circumstance that a similar deposit had been observed on the 17th of June in the previous year, after a shower which happened at about the same time of day.—(Utica.)

It has been often noticed by the observer, that a southeast wind is followed in this section of country *invariably* by a rain storm, within twenty-four or forty-eight hours.—(Canandaigua.)

July 17. This morning, between six and nine o'clock, four and a quarter inches of rain fell. The water poured from the hills in torrents, and the country for several miles around has sustained considerable damage. Fences, barns, one mill and a strong stone bridge were swept away. A man was drowned during the shower in a brook, which the day previous he might have leaped across.—(Fairfield.)

July 23. The rain last night has raised the streams and done much damage. The Onondaga creek, it is said, has not been so high in twenty years, at this season of the year.—(Onondaga.)

August 14. This day, two and a half inches of rain fell between 3 and 11 P. M. It occasioned the most sudden and violent overflowings of the springs and streams ever witnessed by the oldest inhabitants.—(Onondaga.)

Oct. 16. It is to be noted as remarkable, that we have had two inches of snow before the appearance of frost.—(Onondaga.)

Oct. 13. *Dark day*. Wind S. W. The atmosphere was filled with smoke, which, with intervening clouds, intercepted the sun's light to such a degree as to require the use of candles several times during the day. The water which fell in the afternoon and evening was so much affected by the smoke as to be bitter to the taste.—(St Lawrence.) Windy, with remarkable dense smoke, which at intervals caused a darkness that almost rendered candles necessary at noonday.—(Middlebury.) It was necessary to light candles at 3 P. M. to read and write. Some thunder and lightning.—(Auburn.) Uncommon darkness at 4 P. M.—(Franklin.)

River Hudson. The ice in the Hudson broke up Jan. 1.—(Hudson.) Jan. 3.—(Lansingburgh.) Jan. 4.—(Albany.)

River closed Jan. 20.—(Hudson.) Jan. 22.—(Poughkeepsie.)

River open Feb. 8.—(Hudson.) Feb. 9.—(Poughkeepsie.) Feb. 8.—(Lansingburgh.)

River closed Dec. 22.—(Lansingburgh.) Dec. 31.—(Hudson.) Dec. 23.—(Albany.)

Canal.—Feb. 23, canal open and boats running.—(Utica.)

March 29. Canal navigation commenced for the season.—(Utica.)

Dec. 18. Canal closed by ice.—(Utica.)

Hamilton Academy is situated in a valley twenty-eight miles from Utica, and about 700 feet above the Erie canal at that place. A branch of the Chenango river enters the village from the northwest, thence runs southwest, and the winds, following the course of the river, are, for the most part, in those directions.—(Hamilton.)

We hope it will be in our power to favour our meteorological readers annually with an abstract of the above reports, which are equally creditable to the zeal of the Principals and Professors of the College, and to the public spirit of the Regents of the University. There is no country in the world where the sciences of observation are making such rapid progress as in North America; and before another century is completed, those sciences which depend on abstract reasoning, and which are fast declining in our own country, will in all probability find a sanctuary in the New World. If, during eight centuries, England has produced only one Newton, how unreasonable is it to expect that America should have given birth to another in the first century of her political existence.

ART. IX.—*Physical Notices of the Bay of Naples.* By JAMES D. FORBES, Esq. Communicated by the Author.

No. V.—*On the Temple of Jupiter Serapis at Pozzuoli, and the phenomena which it exhibits.*

Piscium et summa genus hæsit ulmo,
Nota quæ sedes fuerat columbis ;
Et superjecto pavidæ natarunt
Æquore damæ.
HOR. *Carm.* I. 2.

AT the south-western base of the hill of the Solfatara, which was the last object of our inquiry, and almost within the precincts of the small town of Pozzuoli, stand the remains of the Temple of Jupiter Serāpis,—remains which yield to few in the variety of interest they are calculated to excite, and which form an additional and striking example of the surprising, and, if I may be excused the term, the eccentric mode in which nature has sometimes pursued her course in the interesting region we have undertaken to illustrate.

Most specimens of the architecture of the ancients are objects of interest merely in an antiquarian point of view. The Forum of Rome, the Athenian Acropolis, or the Temples at Pæstum, have little else than their antiquity and their picturesque beauty to recommend them to the intelligent observer.* What then shall we say to a fragment of other times, which, besides its mythological, antiquarian, and archæological interest, affords a subject of inquiry and speculation to the geologist, the cosmographer, the mineralogist, the lover of the picturesque, the zoologist, and the hydrographer. Considering the many claims, therefore, of this building to our attention, I shall be excused, however briefly or imperfectly I may recount them, for devoting a whole paper to the object of so much curious inquiry and original speculation.

Of the early history of this beautiful monument we know

* We may except the Coliseum, which to the botanist affords a high treat. Amidst its stupendous ruins no less than 261 species of plants have been observed, of which 148 are British.—See Williams's *Travels in Italy and Greece*, vol. i. Appendix.

surprisingly little. Neither the laborious Cluverius nor the more modern and elegant Cramer * has noticed any traces of such a temple being preserved by the classic writers; yet its ruins prove the conspicuous character it must have held as a work of art; and to them alone we must look for a key to its entire history, which is almost unknown till the very late period of its discovery, which took place in 1750, when some projecting columns, formerly concealed by bushes, attracted sufficient curiosity to induce an excavation, which produced the discovery of what to this hour would have been one of the grandest remains of Roman antiquity, had not the rapacity of the then reigning king of Naples made the splendid pillars of African breccia a seizure, to adorn his magnificent palace of Caserta, where they still form the supports of the vestibule of the royal chapel. Inscriptions, those luminaries of antiquity, though so often neglected and oftener perverted, have not failed to throw some light on these remains; but the recorded notices of them are extremely dispersed, and of most of them I have not been able to procure copies at length. We accordingly find among different authors considerably various accounts of the date of the temple. It has been stated by an Italian writer,† and thence, I presume, copied into one of our best guide books,‡ that an inscription was found, indicating the date of the edifice to be the sixth century of Rome, or the third before Christ; and it is added, that its richness and elegant taste prove the high advancement of the Romans in the fine arts at that early period. But, unfortunately for this opinion, it is well known that hardly any foreign marbles were introduced even into Rome till after the commencement of our æra.§ The whole style of the architecture has been referred by some authors to the second, ‖ and by others even to the third century ¶ after Christ; and, what is most satisfactory, inscriptions were actually found in the Atrium of the temple, in

* *Ancient Italy*, vol. ii. An excellent work.

† Ferrari *Guida di Napoli.*

‡ Starke.

§ See this *Journal*, vol. ix. p. 30, &c.

‖ *Voyage Pittoresque dans la Royaume de Naples*, folio, vol. ii.

¶ Göthe *Morphologie.*

which Septimus Severus and Marcus Aurelius record their labours in adorning it with precious marbles.* The inscription of the third century B. C. must therefore have referred merely to the original temple, of which the ground-plan, probably borrowed from the Greeks, was perhaps preserved, but subsequently entirely renewed from the pavement to the roof, the former being composed of a variety of ornamental stones in a pattern, the latter of slabs of Pentelic marble, and the columns in the interior of Cipollino and Africano marbles, and of granite.

But another guide to its date involves a question equally interesting, To what deity this temple was dedicated? The very extraordinary want of publicity under which the ancient inscriptions relating to these remains seems to have lain, has sometimes raised a doubt on the subject, or even admitted of complete scepticism. Lalande † supposes that it was more probably a temple of the Nymphs than one of Serapis; Spallanzani and Breislak speak of its designation as one of conjecture; De Jorio ‡ gives as the reason for admitting its designation, that medicinal baths were evidently employed in it, which certainly were strongly characteristic of the *Serapea*, or temples in honour of this god, who presided over the medical art in the estimation of the Egyptians, from whom his worship was derived. The authors of the superb French work, the "*Voyage Pittoresque dans la Royaume de Naples*," have founded the authority of the appellation of these ruins upon an altar found there with the obscure inscription DVSARIS SACRVM, to which, in rather a circuitous method, an interpretation in favour of the worship of Serapis has been applied from two Celtic words; though how the Celts or their lan-

* Breislak, *Campanie*, ii. 167.

† *Voyage en Italie*, vii. 341.

‡ I regret that I have had no access to the pamphlet of this author published expressly on the Temple of Serapis, and which may possibly contain some of the inscriptions above alluded to, and of the want of publicity of which I have complained. Yet, however agreeable and well-informed a man the Canonico de Jorio may be, and I know him to be so, his works, as far as I have seen them, contain little either of originality or of research.

guage should have reached the south of Italy, or more distant coasts of Egypt in the golden days of Rome, seems the greatest problem. A more probable derivation has been given by Nixon, * on the authority of Vossius, who derives the Greek word δουσαρης, which seems actually to have been applied to Serapis by the Arabs and Phœnicians † from two Hebrew roots, signifying "Lætitia Terræ,"—a characteristic appellation of Bacchus, who is well known to have been one of the divinities represented by this mysterious Egyptian hierarch. ‡ Perhaps the most incontestible proof of the nature of the temple arises from one of the statues found in it, the very existence of which is unaccountably passed over by almost every writer on the subject, and by some is merely alluded to, apparently in the same ignorance of its nature and authority in which the reader is left, yet its testimony is indisputable, being a perfect resemblance of this rare divinity with his attributes. He is seated, having a long beard, with the *Modius* on his head; at his right hand a Cerberus, and with a spear in his left,—characteristics perfectly coinciding with those of Serapis preserved in the Vatican. § The image from Pozzuoli, which, from the silence or confusion of authors, one might have fancied to be altogether traditionary, was dug up in 1750, and is actually preserved in the museum at Naples.

But besides all this evidence, by far the most interesting fact remains, yet perhaps is less generally known than any other which has served to enlighten the history of the temple,—an inscription which is not only decisive of the nature of the divinity, but also will rectify the doubts already enumerated as to the date of the building. Upon this authority, and no other, the rumour (for it is little better) of the origin of the temple in the sixth century of Rome, above noticed, is founded, though it is improbable that either of the writers mentioned as referring to it ever saw the original. The inscription relates to a period somewhat posterior to that just mentioned, and does not treat of the foundation of the temple, but of some re-

* *Philosophical Transactions* at large, vol. l. p. 166, &c.

† Romanelli, *Viaggio*, &c. ii. 132.

‡ *Pitisci Lexicon Antiquitatum*, Voce *Serapis*.

§ *Galerie Mythologique par Millin*, vol. 1. Pl. lxxxvii.

pairs and additional walls to be built towards the side of the sea. This lapidary document I had first the good fortune to find copied in the margin of a fine map of the Bay of Naples, published by Morghen in 1772. I afterwards met with it in an extremely curious little work of Capaccio, an author to whom I had occasion to refer as an authority in the last Number of these Notices,* but whose "*Vera Antichità di Pozzuolo*" I had not then met with, which bears the date of 1652. He does not mention the discovery of this antiquity, which had probably been long preserved in the town of Pozzuoli. It was thence removed to Naples, and afterwards to the villa of S. Arpino, where, according to a later testimony,† it appears long to have remained, and perhaps does so still. It has been transferred, confessedly, from the copy of Capaccio into the work of Romanelli; ‡ and in these three works only, two of them old and scarce, does the knowledge of this interesting inscription lie, which I hesitate not to call one of the most remarkable, and considering its date and great length, one of the most fortunately preserved inscriptions I have met with. The space it would occupy forbids my transferring it into a paper rather of a scientific than an antiquarian character, which I should willingly have done, since it has not found its way into any of the voluminous "*Thesauri*," and "*Corpora Absolutissima*" of inscriptions, of which I have examined a great number. The following fragment of the commencement will illustrate our immediate object of inquiry :—

A. COLONIA. DEDVCTA. AN. XC.
N. FVFIDIO. N.F. M. PVLLIO. DVOVIR.
P. RVTILIO. CN. MANLIO. COS. OPE
RVM. LEX. II. LEX. PARIETI. FACIVN
DO. IN. AREA. QVAE. EST. ANTE. AEDEM.
SERAPI. TRANS. VIAM.
QVI. REDERIT. PRAEDES. DATO. PRAE
DIAQ. SVBSIGNATO. DVVMVIRVM.
ARBITRATV.

* * * *

* In the last No. of this *Journal*, p. 127.

† Galanti, *Descrizione di Napoli e Contorni.*

‡ *Viaggii*, &c. ii. 133.

Here we have an admirable guide to the date of the inscription; for the consuls therein mentioned, P. Rutilius and Cn. Manlius (or Mallius, or Manilius, as have sometimes been put in place of it), held their office, as appears from the *Fasti Consulares*,* in the year of Rome 648. But farther, we have the direction *A Colonia deducta an. XC.* It appears from the testimony of Livy,† that this colonization of Puteoli took place in A. V. C. 559,‡ so that the consular year above indicated corresponds to the 90th subsequent year. This train of proof, therefore, satisfactorily indicates the early period at which the worship of Serapis was introduced into Italy,—a point much mistaken by authors, since it has been asserted that Antoninus Pius was the first who, in A. D. 146, introduced it; for not merely had Vespasian, § and his sons, Titus and Domitian, || introduced Serapis on their coins, but Dio informs us ¶ that in Rome, A. V. C. 699, the senate, to check the introduction of foreign deities, ordered the destruction of all private temples of Serapis; and the inscription before us shows that half a century before, this divinity was not new to the south of Italy. It is, however, certain that Antoninus Pius and M. Aurelius were great promoters of the worship of Serapis, as appears by the various inscriptions in his honour, erected by these emperors,** which have been preserved; and in the ruins of the Puteolan temple inscriptions of the latter emperor, and of Septimus Severus, have, as we have observed already, been found, indicating that to them the edifice owes its incontestible marks of splendour, and therefore the inscription of A. V. C. 648, bears no evidence to the advancement of the arts in Rome at that early period, as was falsely imagined. These inscriptions

* Consult Hook's *Roman History*, 4to, vol. iv.

† L. xxxiv. 24.

‡ See the note on the last quoted passage in Drachenborch's *Livy*. 4to. v. iv. p. 853.—Morcelli makes it 560, but by some the consulship of Rutilius and Manlius is placed in 649, which equalizes the difference. See *Morcelli de Stilo Inscript. Lat.* (Edit. Patav. 4 vols. 4to, 1819.) i. 56.

§ *Venuti, Coll. Antiq. Rom.* folio.

|| Middleton, *Germana Antiq. Mon.* quoted in *Phil. Trans.* 1757.

¶ Dio, l. xl. See Freinshemius *Supp. in Liv.* lib. cvi. c. 23.

** Gruter, page lxxxv. where there are three of Marcus Aurelius and two of Antoninus Pius.

were found in the excavation of the temple, at the base of the pillars of the Atrium. Strange to say I have not found copies of them in any of the numerous works I have consulted. We have already observed the attachment of Marcus Aurelius to the worship of Serapis; and Elius Spartianus particularly mentions the name of Septimus Severus. The temple, therefore, as it finally stood, cannot have been completed before the close of the second century,—a date agreeing very well with the opinion of those who have reasoned upon the style of its architecture.*

I shall now say a few words upon what is known or conjectured of the history of the temple during the middle ages, and of its re-discovery; then shortly describe the form and parts of the temple; and conclude in a more detailed manner with an account of the natural phenomena exhibited by the ruins, and the inductions they furnish, to which the previous historical discussions will be found of importance.

Many dates might be proposed for the period of the ruin of the temple. The horrors of war and of natural convulsions have frequently been wrecked on the town of Pozzuoli. In A. D. 456 it was ruined by Alaric; in 545 by Genseric, and subsequently by Totila; by Romualdo II. Duke of Benevento in 715; the eruption of the Solfatara occurred in 1198; † an earthquake in 1488; ‡ and in 1538 the Monte Nuovo, a considerable hill within a short distance, was thrown up by a volcanic explosion in little more than a day and a night. Probably to some of the earlier barbarian irruptions must be ascribed the ruin of the temple; but we have not sufficient data for imputing its interment by the volcanic matter out of which it was dug, to any other event than the eruption of 1198 or else that of 1538. A moment's consideration must induce us to prefer

* I have not had an opportunity of stating, that there can be no doubt of the application of the inscription of the seventh century of Rome to the particular temple of Serapis under consideration, as the *Decuriones* of Puteoli are in the after part of the inscription specially referred to as the superiors of the work.

† See last No. p. 127.

‡ As stated by Capaccio, (*Antichità di Pozzuolo*, p. 121, 1652.) In a later author I find it called 1458. In another 1448.

the former, if indeed no earlier catastrophe, of which we are not informed, was the agent; for, in the first place, the crater of the Solfatara is only one-third of the distance of that of Monte Nuovo from Pozzuoli; and besides it is hardly to be presumed, that had this temple existed in all the magnitude and splendour in which it was dug out, we should have received no tradition from a period less than three centuries since, when the Monte Nuovo was formed, when the arts, and the study of antiquities especially, were rapidly rising to vigour from the deep oblivion of the middle ages. This seems impossible. We may therefore more naturally refer the event to the obscure and barbarous period of the twelfth century.

A question, has, however, arisen as to whether no portion of the edifice remaining unburied excited the curiosity of antiquaries previous to its disinterment in the middle of the last century; and I am decidedly of opinion that the upper portion of the three great pillars, which now form the most striking features of the ruins, were never entirely covered. That they were not sooner excavated can be considered a matter of no surprise in a country so full of those "fragments," not natural, but symptomatic of civilization and of refinement "of an earlier world," that satiety blunts the keenness of research. This opinion has, however, been strongly opposed by the Italian geologist Pini, in one of his papers, * who, though he admits that Capaccio and Mazzella mention three pillars which they supposed to belong to the temple of Neptune, but might be those of the temple of Serapis, which lies at the foot of an eminence on which the former building stood, adopts the opinion, that these were some other columns, since they are spoken of by Ferrante Loffredo in 1570, an attentive and diligent observer, as having fallen from the temple of Neptune. But if Loffredo speaks of the same columns, it is easy to see from the relative situation of the buildings just mentioned, how he might have fancied them to have come from the higher site. But my authority is more conclusive than any description, being a map in another work by Capaccio,† in which the ob-

* *Memorie di Matematica e di Fisica della Società Italiana delle Scienze*, 4to, tom ix. p. 211.

† Pini quotes the "*Antiquitates et Historia Campaniæ.*" This is the work on Pozzuoli.

jects are represented according to the old style in perspective, and executed in a very distinct and interesting manner. We there see, exhibited in a manner decisive both from appearance and situation, the three pillars, which no one can imagine to be any other than those of Jupiter Serapis now standing. This evidence I consider quite satisfactory, and I shall not spend time by pursuing it farther, since, though a part of the columns should have always been exposed, it is only what, on any consideration we should expect, and does not at all affect any geological theory. Two points, marked FONS on the map, obviously correspond with the well-known thermal springs, one of which must have been exactly in its present site.

The disinterment of the temple seems to have proceeded rather from a growing taste for antiquities than from an accidental discovery. It has been asserted that in 1750, "a peasant fortunately espied the top of one of the columns a few inches above ground," upon which an excavation was undertaken; but Messrs Cochin and Bellicard,* who wrote soon after that period, expressly inform us, that in 1749 the three pillars were visible, being only half buried in the ground, which is at all events the most probable supposition. In 1750 the disinterment was commenced by Charles III. King of Naples, and (unfortunately for art and antiquity,) two years after, the palace of Caserta was commenced, to which were transferred almost all the portable riches of these splendid remains.

The plan of the temple given in Plate IV. will make its structure clear, with a short description. The atrium or court of the temple was enclosed by a series of chambers regularly disposed in a quadrangle, measuring 134 feet by 115 to the exterior. Half of these apartments entered from the portico which, though now ruined, seems to have existed all round the interior court H H H H, while those which opened externally were intended for persons who resorted to the temple for the benefit of their health and used those medicinal waters which were the constant accompaniments of the *Serapea* of the Greeks.† The two apartments at the corners D D, seem to have been more particularly adapted for the use of bathers,

* *Antiquités d'Herculaneum*, 8vo.

† De Jorio, *Pozzuoli*, &c. p. 32.

and to which the water was conveyed and distributed in marble ducts. The particular arrangements of these chambers have excited considerable controversy.* The great entrance to the temple was at G, and the *cella* B, was separated from the *naos* or true temple A, which had a circular form, by the *pronaos*, which was decorated with four surpassing columns of Cipollino marble, the colour of which is a greenish gray veined with white. They were 5 feet in diameter, and 46 feet high, being of the Corinthian order. Three are still standing, and are the most remarkable objects of the temple; the fourth lies in fragments at their bases. They are not fluted as Lalande declares.† Their appearance is represented in the sketch in Plate IV. The interior circular temple was raised above the Atrium and ascended by flights of five marble steps, corresponding to each of the sides of the portico, as shown in the ground plan. The diameter of the temple was 70 feet,‡ and supported by sixteen columns of beautiful African breccia. All of these that were entire have unhappily been transported to Caserta: some fragments still remain on the elevated platform. The pavement of the temple was adorned with various marbles, and the roof which covered the portico already mentioned, was formed of pieces of Pentelic marble fitted like tiles. Breislak § informs us that it is a Dolomite limestone.

Between the pronaos and the temple, and between the latter and the entrance, were two rings of bronze|| P, P, for securing the victims, which are still preserved, the great altar being placed in the centre of the circular temple; and cylindrical vases were placed between the pillars, which are supposed, according to the latest authority, to have been intended to contain the entrails of the victims for examination,¶ though it was long imagined that they were the tops of wells, the water

* See Nixon in *Phil. Trans.* 1757; Romanelli, *Viaggii;* De Jorio, *Guida di Pozzuoli*; Göthe in *Edin. Phil. Journ.* vol. xi. *Voyage Pittoresque*, &c.

† *Voyage en Italie*, vii. 341.

‡ According to Ferrari, *Guida di Napoli* (who calls it 80 palms.) Others have stated it at 65 and 54 feet.

§ *Campanie*, ii. 166.

|| Göthe is mistaken in calling them iron.

¶ De Jorio, *Guida di Pozzuoli*.

of which was used for purification. The portico in the court was supported on the three sides different from the *pronaos*, by a colonnade of twenty-four granite pillars *, eight on each side; and on the fourth, besides the great columns of Cipollino, one author informs us that there were four small ones of that rare stone the *giallo antico* or antique yellow marble †. Supported by this splendid range was a frieze of the *pronaos*, executed with arabesques, leaves, lions and griffins. Within these, and close to the entrance of the *cella*, were formerly two other great Cipollino columns, and their corresponding semi-columns attached to the wall, the bases of which are seen in the ground plan. The architecture of the *pronaos*, I think, may probably be referred to the period of Antoninus Pius, as it will recal to the classical traveller a very similar specimen of art in the temple of that emperor in the Roman forum, the pillars of which are of the same marble, and commonly reputed to be the largest known; but those of Jupiter Serapis I suspect exceed them. The frieze almost precisely answers the above description. In the *cella* was found the image of the divinity already noticed.

Some idea of the extreme richness of the temple may be formed from the consideration, that beside each column of the whole temple, (except the four small ones of giallo antico), was placed a marble pedestal with a statue, many of which were found, and must have amounted to 42 in all, the number of pillars being 46. In the splendid French work the "*Voyage Pittoresque dans le Royaume de Naples*," there is a beautifully executed though somewhat fanciful view of the ruins of the temple partly restored, and embellished with its original ornaments.

We learn from the historian Philostratus, who lived under the Emperor Severus, that the use of water, and especially of mineral water, was one of the necessary rites in the Serapea of the Greeks. Accordingly, there is a fountain in the very atrium of the temple at Q, which still overflows. But the most important spring is a thermal and medicinal one just behind

* Breislak says that the granite appears to be that of Elba.

† *Romanelli*, ii. 137. For an account of the different ornamental stones here mentioned, I may refer to my paper in this *Journal*, July 1828.

he temple, and which probably gave rise to the choice of the site, and was conveyed by ducts into bathing-rooms already mentioned. The temperature of the water in the reservoir I found on the 24th of March 1827 to be 98°.5 Fahr. The mineral matter is principally muriate of soda, with a little carbonate of soda and sulphate of lime. It is still recommended for medicinal purposes, taken internally, and has even been imitated in Naples.

This short description will give an idea of the general position of the temple and its structure. We must now notice the curious phenomena connected with it, which have given rise to so much controversy.

At the height of ten feet above the base of the three standing pillars, and in a position exactly corresponding in all, is a zone of six feet in height where the marble has been injured by the action of the well known shell-fish which live in cavities pierced by themselves in the rocks they inhabit. To discuss these minutely, or to theorize upon the method by which they form their dwellings, would be quite beyond our present purpose; but I am happy to refer to the excellent paper of Mr Stark in the *Edinburgh Transactions*, vol. x. for many curious details on the subject. I shall not fatigue the patience of the reader by giving all the conflicting opinions of travellers and naturalists on the animals which once inhabited the pillars of the temple of Serapis, but content myself with quoting two of the ablest observers of Italy, Spallanzani * and Pini †. They agree that the shells here found are the *Mytilus lithophagus* of Linnæus, a bivalve shell, but not dissimilar in habits to that of the *Pholas*, a true multivalve, with which it has often been confounded‡. By the powerful action of the valves of their smooth shell, they have at some former time made holes to the depth of four inches in the hard limestone of which these pillars are composed, avoiding, however, the nodules of quartz and felspar which sometimes occur. Several fragments of the

* Travels, i. 84.

† *Memorie della Societa Italiana, &c.*

‡ It may save some confusion to mention, that the whole genera of stone-piercing animals are known in Italy by a variety of synonimous terms, "Mangia-pietre," "Forapietre, "Datteri del Mare" (from their resemblance to the date fruit in form), "Foladi," "Litofagi."

of which was used for purification. The *n* their sides, have also
supported on the three sides different ranite pillars are untouch-
lonnade of twenty-four granite pil which in the course of these
on the fourth, besides the gre *n* his work, asserts, apparent-
thor informs us that there des only work at the surface of
stone the *giallo antico* or *pletely* overthrown by Spallanzani,
ed by this splendid ra *found* them on the Italian coast at
ted with arabesques *m* fished from all depths, down to that
and close to the e *rk*, however, where the tides are consi-
great Cipollino *ts* uncovered at low water, as in those at Jop-
attached to t *ia*, of which Mr Stark has given an account,
plan. Th *I have* myself examined. At present, the *Mytilus*
bly be re *is not* found in perfection in the Bay of Baja,
to the *found* some small specimens of a similar species
temr *piers of* the ancient mole commonly called that of
wh This, however, need not surprise us, or render the
t *occurrence* of these shells in the pillars of the temple more unac-
countable, for animals of this description are migratory in their
habits, of which we have an example given by Pennant,†
who states that these genera abandoned Livonia and Curlan-
dia in 1313, and subsequently the shores of the Baltic, but
reappeared abundantly in 1713. A partial desertion of the
coast need not therefore surprise us; and a foreign traveller
who has attended to the conchology of this part of Italy, gives
Naples as a *habitat* of the "*Mytilus lithophagus*," and especi-
ally the coast of Taranto, where they are used as an article of
food.‡ The *Pholas dactylus* of Linnæus occurs in the same

* Ferber's *Travels*, p. 172.

† *Arctic Zoology.* Since writing the above, the following ingenious remark, confirmatory of the theory supported in the sequel of this paper, has been communicated to me by a gentleman well versed in zoology. "The migrations of boring testaceous mollusca which burrow in submerged wood as well as calcareous rocks, are easily accounted for by means of drift-wood, &c. But if the sea, by some convulsion of nature, had very suddenly receded from the location of the *Mytili* and *Pholades* in the Bay of Baja, the reason of their having afterwards ceased to multiply in that place is apparent from the instant destruction of the colony. If, on the contrary, the sea had gradually receded, the presumption is, supposing the submerged rocks of a nature to afford them a retreat, and all things else the same, that they should still be found alive at that locality."

‡ Ulysses' *Travels in Naples*, 1789. Translated by Aufrere. Appendix, p. 498.

localities. The holes of these lithophagous mollusca have a very peculiar form. They are pear-shaped, the external opening being minute and gradually increasing downwards, the animal being found in the bottom. This proceeds from the increasing size of the inhabitant as it grows older, which requires it to employ the means furnished by nature for forming its abode to correspond with the increasing magnitude of its shell. The mytilus is therefore enclosed in a perpetual and solitary prison, since no two animals can ever reside in the same hole.

The perforations at the Temple of Serapis are of considerable depth and size, and therefore manifest a long-continued abode of the *Mytili*, and consequently a long-continued immersion in sea water. How this should have taken place it is most perplexing to explain. The marks of the perforations begin at ten feet above the level of the pavement and continue for six feet, exactly corresponding on all three columns, as shown in the perspective view, Plate IV. With regard to their present height above the sea, it is a singular fact, that the platform of the temple is about one foot *below** high water-mark, (for there are small tides in the Bay of Naples,) so that the sea water actually rises and falls at present in the building, being only 100 feet from it. It cannot possibly be imagined that the temple was built under such circumstances. There are, therefore, proved to be two *relative* changes of the level of the sea, which it is the business of the naturalist to explain. By losing sight of the latter change altogether, or by purposely giving it up as inexplicable, some writers have given a novel and ingenious speculation, but rather, we think, overshot the mark.

Two opinions at first were prevalent, and certainly they are the most obvious and natural: That the sea had risen and fallen successively as these marks indicate, supported by Ferber and originally by Breislak; or, as was most commonly held, that the land was alternately lowered and elevated by earthquakes, and hence the *relative* level of the sea changed,—an idea entertained by the greater number of the older writers,

* See Pini, Breislak, and Romanelli. That it is certainly below the level of the sea my own observations confirm; but some authors have made strange mistakes on this subject.

and particularly supported by Mr Playfair, as confirmatory of the splendid Huttonian theory, which he undertook so ably to illustrate.* To the former of these opinions has been objected, the impossibility of any partial rise of the level of the waters of the globe, which are in any way connected with the great ocean, and that we have no reason to admit such a general rise over the surface of the globe, which could alone explain a few such insulated facts. Against the second explanation it has been urged, that the temple would not now have been standing if it had been thus shaken about by earthquakes, or at least the pillars must have been put off the perpendicular: And farther, that in all probability the spring of medicinal water must have been dried up by such a natural convulsion.

The third and fourth explanations, which, from their extravagance, we class together, have received more formal refutations than they deserve. Spallanzani, apparently in a fit of despair and nonchalance, suggests, that the columns were perhaps accidentally buried in the sea, and then dug up and employed in the temple. But will any man in his senses believe, that three pillars could by accident have been worn in places corresponding precisely in each, when they were finally set up in their new situation; or that, if they had once formed part of a temple now covered, and had been thus fished out, they would have been employed in an edifice of surpassing grandeur, without even a covering of stucco or the very shells being extracted from the holes? Besides, fragments of pillars were found in excavating the temple, which still lie on the elevated platform, and are not only perforated in the whole length of their exterior, but on the cross fracture, at right angles to the axis of the pillar, another animal has fixed itself, the *Serpula*, both the *triquetra* and *contortuplicata* of Linnæus; the most satisfactory of all proofs that the immersion of the temple succeeded its final ruin. The other hypothesis, which is proposed by Raspe, the translator of Ferber's *Letters*, shows the most palpable ignorance of the state of the facts; he imagines that the stone may have been perforated before it was cut into columns, an idea which I

* *Illustrations of the Huttonian Theory*, § 397, p. 450.

need hardly say, if I have at all succeeded in explaining the phenomena of the spot, could not for a moment be entertained by any person of common sense, who has even looked at the pillars.

The fifth and only other theory which, as far as I know, has been suggested, is very ingenious, and more far-fetched than any of those above-mentioned, to which it is also subsequent in date. It has been supported by Göthe, Pini, De Jorio,* and Daubeny, and seems to have been originally proposed by the first of these authors. They suppose, that when the temple was covered by volcanic tufa a hollow was left, as might naturally enough be supposed, in the court of the temple; that afterwards by some means sea water was introduced, and formed a salt lake surrounding the three standing pillars, in which the *Mytili* bred and pierced the stones. The lake being then dried up, the shells were left in the holes, and the building restored to its former condition as a ruin.

The hypothesis of which the above are the leading facts, strikes me, I confess, as one of the most assumptive that could be formed, and, with due respect for the names which have supported it, I cannot help looking upon the temporary popularity it has received, as the result of the singular obscurity of the subject, and the ingenuity manifested in finding a new explanation which might evade many difficulties of the old ones, and, as it were, surprise the reader into belief, hardly giving him time to consider the peculiar and weighty objections which take the place of former ones. Indeed, the supporters of this lacustrine hypothesis have seldom condescended upon the particulars of the operations they so boldly assert to have taken place; and it is in the paper of Pini, formerly quoted, that we find the subject pursued to its details, which we suspect rather invalidate the argument than support it.

It is of little consequence at what period the temple was

* I have already mentioned, that I am not in possession of De Jorio's book expressly on the subject; but I infer his opinions from the citations in Dr Daubeny's work, and a short expression of his opinion of a lake in the "*Guida de Pozzuoli*," though in the same work he brings forward testimonies which we might have thought would have suggested a more simple theory.

buried, as far as regards the theoretical question; but it is surprising that Pini should appear rather to lean to the eruption of the Monte Nuovo, which took place in 1538; since which time one would expect to have received some account of such strange phenomena as the theory assigns to this period. Not contented by such a simple explanation as that a huge wave might have been thrown into the bed prepared by the eruption, a wave merely occasioned by a contemporaneous commotion of the sea, it has been thought necessary to waste much argument upon the proof of a more miraculous source, namely, through the very Monte Nuovo itself, which certainly did throw up water at the time, but must have been most surprising in quantity, if such torrents were conveyed to the distance of an hour and a-half's walk from the mountain. Let this pass, however, and see how the lake is to be stocked with the fish. The theorist is not even here satisfied with the unwarrantable assertion, that at the moment of a wave being projected into the bed prepared for it, a quantity of the germs of the *Mytili* were floating in readiness to be wafted into their new dwelling; but he must make the unfortunate animalculæ pass through the bowels of the explosive volcano and reach the temple along with the requisite supply of erupted water! He even enters into a variety of details to prove how the temperature of the water fresh from the seat of volcanic fire should be cool enough not to cook the embryo mytili*. It is then presumed that the lake being once formed, the springs of fresh water from the mountain of the Solfatara adjoining supplied the waste by evaporation, whilst the animals commenced their labours. It is rather amusing, however, to notice in this very

* Lest the reader should think I am exaggerating when in fact I dwell only lightly on the absurdities of the hypothesis, I refer him to the *Memorie della Societa Italiana*, ix. 221.—All the reasoning about the germs, too, is a tissue of unwarrantable assumptions, the subject being still quite obscure. These animals have in each individual the reproductive faculty; but how the young are sustained, whether they float about in the sea or not, and how they commence their holes, are problems in zoology of which we have no attempt at solution. The lacustrine theorists, in order to complete their assumptions, have only to suppose with Rondeletius that the sea water lodging in rocks is actually transformed into *Pholades*.—See Mr Stark's paper in the *Edinburgh Transactions*, x. 487.

paper, that Pini attributes the migration or destruction of the *Lithophagi* in the Bay of Baja to the volcanic salts thrown in vast abundance from the Monte Nuovo. We would ask if they found themselves better off in the wretched volcanic pool which he has prepared for them? Nor has Göthe much mended the matter by assuming a directly opposite principle to support this laboured theory. He supposes that the saltness (which according to every known law must have rapidly diminished) was replenished by a constant infusion of those very volcanic soluble compounds which Pini considered the agent of the extirpation of the shell-fish in a large and open bay, consisting, we suppose, of such salubrious ingredients as sulphate of lime and magnesia, or of sal ammoniac. We might, from the great difficulty of the problem, tolerate some bold statements or rather opinions on the habitudes of the *Lithophagi*, if the time of the supposed existence of the lake required to be only very short. But when we consider the great size of the perforations, which are four inches deep, and that these appear by the best accounts to be formed in this very hard and even quartzose marble by the mere action of a smooth shell of the common degree of induration, which, like the fall of a drop on a stone, must act "non vi, sed sæpe cadendo."—And farther, when we know that it could only be by the complete range of the animal's natural term of growth that these holes were by slow degrees completed, we shall not wonder at the assertion of Spallanzani,* the best informed observer in these branches of natural history that ever wrote on the phenomena before us, who declares that he can prove by "incontestable facts," that to form cells of such a depth, the animals must have inhabited them for *nearly half a century*. Why, after lasting half a century, the lake should not have lasted to this hour, the theorists must make some additional assumption to explain.

I have no wish to hold any one supporter of the lacustrine hypothesis to these minuter details of the methods which may probably have been employed by nature to produce the imaginary effects, as the formation of the volcanic bed, the filling with water, the stocking with fish, and the opportune disap-

* *Travels*, i. 88.

pearance of the lake as soon as nature had completed the operation for which she put so marvellous and eccentric a train of wheels in motion. Let all these be accounted for by the individual fancy of the theorist; but I put it to the candid inquirer, whether we ought not to view with suspicion a theory consisting of a series of hypotheses involving a chain of independent, unrecorded, and imaginary events, and to which even analogy forms no guide, emanating in a spirit like that which Göthe displayed in his more popular works, romantic rather than profound, resembling more the workings of an imaginative disposition than the patient inductions of the natural inquirer,—like the Miltonic sphere, composed of

> "Cycle and Epicycle scribbled o'er,
> To save appearances."

If it now be asked to what theory I attach myself, I answer that I am disposed to the second of those above enumerated, which attributes the apparent changes of the water line to an alternate depression and elevation of the land, without, however, altogether setting aside the first, which infers an actual change of level in the Mediterranean; for in some cases philosophers have not yet been enabled to separate the existence of such a real from an apparent motion. In order, however, to revive with effect an explanation which of late years has been somewhat thrown into shade by the more showy hypothesis of a lake, I shall first briefly state the advantages of the argument by such a supposition, and then endeavour to answer the objections which have been urged against it, the most weighty of which we have already candidly stated. By combining some collateral testimonies to which sufficient attention has not been paid, I hope to make out a clearer case for this hypothesis than has yet been effected.

In the *first* place, then, the present position of the temple below high water-mark indicates some cause in action which the lacustrine theory does not explain. This important fact has been wholly misrepresented by Göthe, who, in the sections given by him, under the pretence of showing the relative levels of the sea, the temple, and the supposed lake, he actually places the second *thirty-two feet above* the first instead of one foot *below* it—a most extraordinary oversight. Daubeny again, after De Jorio, imagines that the temple may have been always below the level of the sea, and subject to its incursions, since, as we have already said, it is only 100 feet from it,—a supposition which common sense can never warrant, and which, till it became necessary to alter either the facts or arguments to shape them to the new hypothesis, was always considered physical proof of a second change of relative levels. Pini, the most elaborate supporter of that theory, is compelled to admit its inadequacy to account for this fact, and affords, I think, the strongest possible testimony to the opinion which I endeavour to support. He admits that *the land must have suffered a depression*, and even points to the occasion on which it may have happened, the earthquake of 1488 or that of 1538, on both which occasions, according to Mazzella,* an old writer, many houses in Pozzuoli were overthrown: (*subbissati.*) Pini, therefore, by this admission, obviously does away with the necessity of his much laboured explanation of the opposite phenomenon, the apparent descent of the level of the sea; for is it not the most natural course in the world to account for two events similar in their nature and object by a single cause, rather than drag in an assumptive hypothesis to explain one since it cannot explain both? Farther, the idea that the present situation of the sea-line indicates no rise in its level from some period or other, is rendered quite untenable by facts in the very same bay. We there find the bases of pillars which appear to have belonged to a temple of Neptune, and another of the Nymphs, at all times under water, as I have myself witnessed. We draw no argument from the projection of the ruins of villas into the water, since we know that to have been at one time a fashionable rage. † But it happens that *two Roman roads* now exist under water, one reaching from Pozzuoli towards the Lucrine Lake, which may still be seen, ‡

* *Situs et Antiquitates Puteolorum.*

† ———— Struis domos;
Marisque Baiis obstrepentis urges
Summovere littora,
Parum locuples continente ripa.—Hor. *Carm.* ii, 18.

‡ De Jorio, *Guida, &c.*

and the other near the Castle of Baja.* These speak unanswerably. The mole, too, which exists at the port of Pozzuoli, and is commonly called that of Caligula, has the water up to a considerable height of the arches, whereas, as Breislak justly observes,† it is next to certain that the piers must formerly have reached the surface before the arches sprung. Nor are these effects so local as some would have us to believe, for on the opposite side of the bay of Naples, on the Sorrentine coast, which, as well as Pozzuoli, is very subject to earthquakes, a road with some fragments of Roman building is covered to some depth by the sea.‡ It is also certain that in the island of Capri, which is situated some way at sea in the opening of the bay of Naples, one of the palaces of Tiberius is now partly covered with water.§ So much, then, for the argument of a real depression of the land in this neighbourhood.

In the *second* place, we may show that the temporary elevation of the land or depression of the sea level, which took place for a period of not less than half a century, between the time of Septimius Severus and the present, was not of that purely local nature which the supposition of a salt lake would make it. As the most indisputable proof, not only are there marine depositions mixed with the volcanic strata at the foot of the Monte Nuovo, but there is a fragment of building at the same place, where shells are found in small cavities in the stone, at the height of six feet above the sea, and being larger than the entrances of the holes in which they lodge, it is obvious that, just as in the case of the *Mytili* at the temple of Serapis, the animals must have increased in size within the cells during the continuance of the sea at that level. This observation was made by Pini,|| the very man who supports the lacustrine theory, which it is so completely calculated to overthrow.

If we choose to generalize our views a little, we shall find

* Represented in the curious old map by Capaccio.

† *Campanie*, ii. 162. note.

‡ Starke.

§ See the Map in Hadrava's "*Lettere sopra l'isola di Capri.*"—Dresda, 4to, and Breislak, i. 48.

|| On the authority of Breislak, who accompanied him.

plenty of concurring facts to prove, at some period, a continued rise of the sea-line, though for how long, and to what extent, we have not observations enough to verify. Marks of lithophagous animals abound in many places, and render this a matter of certainty. They are found at Palermo on the north coast of Sicily, in Calabria, and on the Monte Circello, between Naples and Rome.* I do not say that these necessarily refer to the same period as when the temple at Pozzuoli was covered, but they point to a strong confirmation of any attempt to generalize such facts.

Thirdly, we have all the agents required for the accomplishment of our theory within the bounds of recorded information or the most direct analogy, without any pure assumptions whatever. The tremendous natural convulsions to which the vicinity of the temple has to our knowledge been subjected, are amply sufficient to explain more frequent, more considerable, and more surprising changes in the natural features of the country than those required by the theory. It is absurd to say that we want direct evidence of the lowering of the land on which the town of Pozzuoli and the temple stands. We know from contemporary writers, that the former has at least three times, in 1198, 1488, and 1538, been ruined by earthquakes, inundated by the sea, or half buried by volcanic ashes; and the latter bears irrefragable testimony to a similar fate, stamped on its features in nature's own most unequivocal characters. But this is not all. We have recorded statements and actual observation to prove, that such changes in the level of the sea have taken place; and we infer distinctly from the testimony of an old Italian writer, Loffredo, in 1580, that fifty years previously the sea washed the base of the hills which rise from a small alluvial flat extending along the shore between Puzzuoli and the Monte Nuovo. That writer, on attempting to fix the position of Cicero's villa to those ruins since called the Stadium, proves, by mentioning them as the only ones between Puteoli and the Lucrine Lake, that the many walls and bases of pillars which we have already mentioned, and which exist where the sea now washes that alluvial plain, were then covered with sand which the water had deposited; and he tells us expressly, that fifty

* Scrope on *Volcanos*, p. 216, and Brocchi.

years before, one might have fished from the supposed site of the villa, (one of the classical criteria of its position,) since at *that period* that flat land called La Starza *was actually covered by the sea.* This invaluable testimony, which, as far as I know, has never been applied to the theory of the temple of Serapis, (for De Jorio, though he was acquainted with it, seems to have strangely neglected its bearing,) gives us an epoch for our calculations, which I think will be definitive. Fifty years previous to 1580 brings us to 1530, or just eight years anterior to the tremendous explosion of the Monte Nuovo. Not a doubt can remain that the upheaving of the ground by this awful catastrophe caused the apparent sinking of the level of the sea. Pursuing the chronology backwards, we come to the next important phenomenon in 1488, when the great earthquake which desolated Pozzuoli points to the most natural possible epoch for the lowering of the temple to such a depth, that the *Mytili* worked at 16 feet above the present level of the Mediterranean, and for *a period of fifty years exactly.* But as we have seen that the temple had been previously buried by volcanic matter, (which, forming a bed within the temple, prevented the attack of the *Mytili* on the lower part of the columns,) there just remains the paroxysm which we have already pointed out on other grounds, as the most probable—namely, the eruption of the Solfatara in 1198.

Fourthly, I may very briefly add, that the opinion of Mr Playfair, that the shores of the Mediterranean are slowly sinking, is rather confirmed by an attentive consideration of the phenomena already detailed, though I confess I think his induction proceeds on too few and imperfect facts, especially in a country which is so liable to extraneous and contrary paroxysmal elevations. We have seen that the lowering of the sea-line certainly took place suddenly in the beginning of the sixteenth century, and, according with the striking phenomenon of a volcanic protrusion, so well as to leave no doubt of the identity of the cause. This, however, is the only direct coincidence of which we are informed; and, instead of the height of the water in the fifteenth century being owing to any sudden action, it may have been the result of a continued depression of the land. I confess, however, this does not appear so probable to me, as that it was the effect of some sudden and local change at that period, both from the extent of the change, (sixteen feet,*) and that from the depth of water it must have been placed in at the end of the twelfth century, the inclosure of the temple in volcanic tufa must have been referred to some earlier event than the eruption of the Solfatara, and with no such are we acquainted so well fitted for this purpose. Certain it is, however, that since the sixteenth century the water has again continued to rise in the Bay of Pozzuoli, and has gradually laid open those ruins covered with soil (probably thrown from the Monte Nuovo) in the days of Loffredo, and is at present making slow but constant encroachments on the former shore. This action is precisely the reverse of what happens in the Baltic, the water of which is lowered four inches in a century, and probably depends on some analogous cause; but whether it arises from the motion of the land, or from some of those internal agencies which undoubtedly may affect the level of confined seas, I shall not now attempt to inquire.†

It remains for me to endeavour to answer the objections which have been urged against the theory of the elevation and depression of the land, with that brevity which the length to which this paper has already insensibly extended requires.

1. The great argument raised in opposition to this opinion is, that, had these apparent changes been owing to earthquakes, the pavement would not now have been horizontal, nor the pillars and the walls standing on their foundations. We admit that it is remarkable; but, to make it a strong objection, shows an oversight both in the consideration of the phenomena of such a species of earthquake, (being no more than a volcanic protrusion,) and of the condition of the building at

* Dr Daubeny, by following Göthe's most perverted and inconsistent statements too closely, has given thirty feet as the extent of this change, though almost on the same page he admits (which that author did not) the depression of the present level of the temple below the sea, and accurately states the height to which the marks of perforation on the pillars extend to be only sixteen feet.

† It is well known that the Mediterranean is considerably higher than the Red Sea. It is likewise a curious fact, that there are many feet of difference of level between the Atlantic and Pacific Oceans.

the time. Pini, who has enlarged most on the objection, dwells absurdly on the internal force requisite to raise so vast a temple, and on the improbability that the whole area of it should be elevated in a state of parallelism; but he wholly overlooks the prodigious scale on which earthquakes usually act, not elevating planes of a few yards, but of whole square leagues, and the damage usually done being, from some accompanying vibrations in the soil, which shake buildings to fragments, and often with a rotatory motion; but in a simple act of volcanic elevation, where the motive power had an exit at so short a distance as the Monte Nuovo, (in one case,) and where the building in question was not a house with unencumbered walls, which might be shaken in pieces like those of Pozzuoli, but a ruin closely imbedded in a volcanic tufa; *they could not possibly* have been overturned till that soil was artificially removed, and then could only have fallen from a want of verticality, which, if we consider the theory of the case, must necessarily be so small, wherever the mass of land elevated is large, and the rise only of a few feet, as to have no sensible effect in the devastation committed by earthquakes. Besides dismissing all arguments of probabilities, it is only necessary to recollect, that the very author who has produced this as the sole objection to our theory has been compelled, as we have already seen, in another part of the same paper, to employ the self-same agent to account for the depression of the temple below high-water mark.

2. A more refined objection has been urged from the existence of the spring of medicinal water, probably in the very spot where it was found by the builders of the temple, which might probably have been dried up or led into a new channel by the effects of an earthquake.* This is mere hypothesis; and in that ignorance in which we must *ever* remain of the particular effects which powerful subterranean agents *may* produce, we dare only argue by analogy; and here again it is in our favour. We have seen that the change of level extended over the shores of this bay; yet still the hot spring in the baths of Nero, which are considerably nearer the Monte Nuovo than is the Temple of Serapis, continues to flow in the

* Daubeny *on Volcanos*, p. 161, &c.

very same spot as it did 2000 years ago, marked decisively by the termination of the passage cut by the ancients through the tufaceous rock, and being immensely hotter than the spring at Pozzuoli, we may presume its greater proximity to the volcanic centre, and its greater liability to alterations arising from such a source.

3. Pommereuil, the translator into French of Breislak's *Topografia Fisica di Campania*, says, in a note upon this subject, "L'idee du baissement et de l'exhaussement successif du terrain avec la precision de 5 mètres rassemble à une plaisanterie. C'est couper la nœud gordien parcequ'on ne peut le denouer." Now this is a positive mistatement. There is no such correspondence between the fall and rise of the ground as is here alleged. One of our great objects has been to show that it did not rise so much as it sunk; but what the difference might be, it is impossible to divine. For anything we know, the temple might have originally been twenty feet above the sea, and is now one below it.

4. Another objection of the same author is equally frivolous. "Le môle de Pouzzoles," says he, "est un temoin irrefragable que la mer n'a point baissé depuis son erection." It is no doubt a proof that the sea is not now lower than when the mole was built; but that is the very evidence we have already drawn from it, and infers nothing respecting its intermediate condition. No one ever said that the marks of the *Lithophagi* on the temple represented the level of the sea at the time of the Greeks and Romans; on the contrary, its present situation proves that the sea was once relatively lower than at present, and therefore gives the very same evidence as the mole of Pozzuoli. This objection has, like many similar ones, been hazarded from the obscurity of the subject, without examination, or even common attention. It is therefore perfectly irrelevant.

These objections I think it will be seen required for their refutation little more than a calm and careful examination of their nature, which they seem never to have received, and have been, therefore, most inaccurately held decisive against a theory, the very simplicity of which rendered it less liable to be assailed than the more refined and speculative one which succeeded it. I shall consider the time and pains which I have bestowed on the investigation of this very curious point well spent, if I shall be thought to have succeeded in reviving an opinion held long by some of the best observers, and affording some geological illustrations of importance connected with the apparent level of the sea,—illustrations extending we have seen to the early period of the second century before our era, and connected with some of the most remarkable natural convulsions of the middle ages. To refute the hypotheses of others on an obscure subject has always been an easy and a thankless task; but to unite this with the support of an opinion calculated to throw light upon the physical history of distant countries or of remote ages, and to give it the advantage of that ordeal which the researches of learned men and the test of experience can alone produce, by answering the objections to which these have given rise, is to serve the cause of truth, and add a new fact towards the promotion of our acquaintance with the material world. "Opinionum commenta delet dies, naturæ judicia confirmat."

Art. X.—*On the Shock experienced by Animals when they cease to form part of an Electric Circuit.* By Dr Et. Marianini, Professor of Natural Philosophy at Venice.

The very interesting memoir of which we propose to give a brief abstract, was addressed in the form of a letter to the academy of Roveredo in November 1827. An extract from it was published in the *Tyrolese Messenger* on the 15th January 1828, and from this it has been translated and inserted in the *Ann. de Chim.* vol. xl. p. 225—256. The general object which the author had in view, and the general results of his experiments, will form the subject of this notice.

"In repeating the first experiments by which Volta demonstrated that the frog is only passive in the contractions which it experiences when it forms part of the arc of communication established between two heterogeneous metals placed in contact, it is often observed that the same contractions are repeated s the instant when the frog ceases to be thus placed in the current. Volta and Fowler seem to have been the first to remark this phenomenon, which was afterwards observed by Valli and several members of the Academy of Sciences at Paris, and by Pfaff, the last of whom regarded it as a great objection to Galvani's theory of animal electricity. Volta himself has given an explanation of it; but it would seem that this great philosoper had only given it a transient consideration. He speaks of it but incidentally in section 49 of his celebrated memoir on the Identity of the Electric and Galvanic Fluids, and in the following manner :—'Such a contraction takes place only at the first irruption of the electric current, and sometimes also at the moment when, by the rupture of the circuit, this current is suddenly stopped, or rather driven back, as we may suppose, by the obstacle which it suddenly encounters.'

This explanation has been adopted by other natural philosophers, as appears from section 80 of the excellent memoir published in 1814, by M. Configliachi on the Identity of the Galvanic and Electric Fluids.

Being unable to comprehend how, in breaking the circuit, a reflux of electricity could be produced, and finding the phenomena to which it relates very remarkable, I studied the subject experimentally, and began by an attentive examination of the explanation of it given by Volta."

After detailing at great length a series of experiments on frogs, by which he overturns the explanation given by Volta, and establishes some new and important results, he concludes with the following general remark and summary of his results.

"Is there an animal electricity, as Galvani always maintained? Or, what perhaps amounts to the same thing, is the electric fluid identical with the nervous fluid, as formerly conjectured by other philosophers? The preceding experiments put us into a situation for determining this point.

But, however this may be, the analysis which I have given of the shock experienced by animals at the instant when they cease to form part of the arc of communication between the poles of a galvanic apparatus, afford me, I think, with certainty the following conclusions.

1. The principles upon which the theory of Voltaic apparatuses hitherto rest, do not authorise us to admit in these apparatuses a reflux of electricity at the instant when the circuit is interrupted.

2. When this reflux has taken place, the shock experienced by the animal at the instant when it ceases to form part of the circuit, cannot be attributed to it.

3. The two kinds of contractions produced in the muscles by electricity, viz. the *Idiopathic contractions* and the *Sympathetic contractions*, deserve to be distinguished from one another, in so far as the *first* take place whatever be the direction according to which the current penetrates the muscles, and the *second* only when the current runs along the nerves in the direction of their ramification, that is, when the part where the current enters the nerve is nearer the origin of the nerve than the part at which the current comes out from it.

4. The agitation which the animals experience when they come suddenly to form part of an electric circuit, arises from this, that the electricity, when it moves in the nerves in a direction contrary to that of their ramification, produces a shock at the instant when it ceases to penetrate it, and not when the circulation is established.

5. When the electric fluid penetrates the nerves in a direction contrary to that of their ramification, instead of occasioning a contraction, it produces a sensation.

6. The animal experiences a sensation at the instant when we interrupt the electric current which runs along the nerve in the direction of its ramification.

Art. XI.—*Notice of the performance of Steam-Engines in Cornwall for April, May, and June* 1829. By W. J. Henwood, F. G. S., Member of the Royal Geological Society of Cornwall. Communicated by the Author.

Reciprocating Engines drawing Water.

Mines.	Diameter of cylinder in inch.	Length of stroke in cylinder in feet.	Length of stroke in the pump in feet.	Load in lbs. per sq. in. of area of piston.	No. of strokes per minute.	Millions of lbs. weight lifted 1 foot high by the consumption of 1 bush. of coal.
Stray Park, -	64	7,75	5,25	7,6	4,2	24,7
Huel Vor, -	63*	7,25	5,75	17,5	5,7	28,
	53	9,	7,5	19,5	5,4	43,5
	48	7,	5,	8,	5,6	33,3
	80	10,	7,5	13,9	6,1	60,4
	45	6,75	5,5	13,7	6,1	50,9
Poladras Downs,	70	10,	7,5	9,2	6,2	57,2
Huel Reeth,	36	7,5	7,5	15,3	3,4	26,6
Balnoon, -	30	8,	7,	7,5	3,4	22,1
Huel Towan, -	80	10,	8,	10,4	6,6	79,
	80	10,	8,	5,8	4,	63,7
United Hills, -	58	8,25	6,5	6,9	4,4	38,4
Great St George,	70	10,	7,5	9,4	4,9	36,3
Perran Mines,	38	6,75	6,	8,2	8,7	21,3
Crinis, -	56	6,75	6,75	9,5	5,9	40,5
Huel Unity, -	52	6,666	5,75	5,	7,2	18,9
	60	7,25	5,75	11,7	6,3	37,1
Poldice, -	90	10,	7,	10,2	6,	47,9
	60	9,5	6,25	12,5	6,6	35,8
Huel Damsel, -	42†	7,5	5,75	20,	6,3	31,1
	50	9,	7,	8,2	3,3	37,6
Ting Tang, -	63	7,75	6,75	14,	6,	44,6
	66	9,	7,5	10,4	2,2	40,7
Cardrew Downs,	66	8,75	7,	10,4	6,7	49,8
Huel Montague,	50	9,	7,	10,6	5,9	40,9
Dolcoath, -	76	9,	7,5	11,8	5,	39,4
Great Work, -	60	9,	7,	10,2	7,6	45,7
Huel Penrose,	36	8,5	6,5	11,7	6,5	31,

Mines.	Diameter of cylinder in inch.	Length of stroke in cylinder in feet.	Length of stroke in the pump in feet.	Load in lbs. per sq. in. of area of piston.	No. of strokes per minute.	Millions of lbs. weight lifted 1 foot high by the consumption of 1 bush. of coal.
Huel Caroline,	30	7,	6,	24,2	12,9	30,4
St. Ives Consols,	36	7,	7,	16,1	6,2	32,7
Lelant Consols,	15	7,5	4,5	17,2	2,8	12,7
Binner Downs,	70	10,	7,5	11,1	8,	63,9
	63	9,	7,5	7,8	9,9	36,6
	42	9,	7,5	13,2	7,2	46,1
Consolidated Mines,	90	10,	7,5	8,8	6,3	58,1
	70	10,	7,5	9,6	7,	63,3
	58	7,75	6,5	18,7	6,5	43,8
	90	10,	7,5	8,2	6,3	62,1
	90	10,	7,5	10,3	3,2	37,1
	64.5	10,	7,5	11,1	4,9	61,
United Mines,	90	9,	8,	7,9	5,1	44,
	30	9,	7,5	12,9	8,	40,2
Huel Beauchamp,	36	7,75	6,	13,	4,2	29,4
Huel Rose, -	60	9,	7,	13,1	6,3	58,9
Pembroke, -	80	9,75	7,25	11,7	4,3	50,6
	50	9,	6,5	9,8	6,5	45,3
East Crinnis, -	60	5,5	5,5	8,5	4,8	25,
	70	10,	7,	9,1	6,	39,3
Huel Hope, -	60	9,	8,	11,	6,3	68,6
Huel Tolgus,	70	10,	7,5	8,2	4,7	50,5
Tresavean, -	60	9,	7,	6,2	3,9	22,
Huel Falmouth,	58	8,75	6,5	3,4	7,9	27,1

Average duty 41,8 millions of lbs. weight lifted one foot high by the consumption of one bushel of coal.

Watt's rotatory double engines employed to move machinery for bruising tin ores.

Huel Vor,	24.	6.	6.	12.	16.7	19.6
	27.	5.	5.	12.	17.7	21.5
	16.5	5.	5.	8.5	26.6	13.9

Average duty of rotatory engines, 18.3 millions.

* Watt's double.

† Trevithick's high pressure combined with Watt's single.

Art. XII.—*Abstract of a Memoir upon the Bones procured from Butcher's Meat.** By M. D'Arcet, Member of the Academy of Sciences.

We propose in this memoir to call the attention of the administration, and to enlighten the public opinion upon the use of the gelatine of bones, considered as an alimentary substance. The long and difficult investigations which we have undertaken with this view since 1812 have enabled us to trace to its foundation this economical question; and we are inclined to believe, that in a few years bones, so rich a source of nutritious matter, will take the rank they so well deserve among the animal substances employed for the nourishment of man.

We submit this work to the judgment of those enlightened persons who devote themselves to the relief of the indigent classes. We are desirous that they should approve of the results of our labours; and we hope that they will give us their support, in order that we may attain the useful objects which we have in view.

Bones ought to be divided into two classes. Those which are compact, flat, or cylindrical, contain very little fat. They are sold at high prices to the turners, button-makers, and fan-makers; and they ought to be laid aside and kept for these purposes.

The other bones which remain after these have been selected, and among which are found the spongy heads of the large bones and the ends of the flat bones, are those which ought to be employed for the alimentary substance. Numerous analyses have shown us that these bones when dry contain per quintal about

Earthy substance, -	60
Gelatine, - -	30
Fat, - - -	10

The heads of the large bones contain as much as 50 per cent. of fat. It is remarked that the bones of mutton and the bones of roast meats give very often rancid fat, or fat smelling

* Translated from the *Ann. de Chimie*, tom. xl. p. 422-430.

of tallow, and that they ought to be laid apart, and managed separately.

One hundred kilogrammes of bones contains thirty kil. of gelatine; and since ten grammes of gelatine are sufficient to *animalise* a half litre of water at least of the best household soup, one kil. of bones is sufficient to prepare thirty basins of soup of one demi-litre; but one kil. of meat will only furnish four basins of soup; from whence it follows, that by equal weight, the bones supply to water seven times and a-half more animal matter than meat.

One hundred kil. of butcher's meat contain about 20 kil. of bones. This quantity of meat making 400 basins of soup, and the twenty kil. of bones making 600, we see, that, by extracting all the bones procured from a given quantity of meat, three basins of soup can be made with the bones, while the meat and the bones united actually give but two.

With the bones contained in the meat consumed in the single department of the Seine, we could prepare more than *eight hundred thousand* basins of soup per day.

The gelatine of the bones can be extracted by submitting them whole to the action of steam; but the operation would be tedious, even though there were no risk of changing the nature of part of the gelatine in making use of the steam strongly compressed, and it is better to bruise them; but some precautions are necessary.

The bones must not be broken by redoubled blows, because they contract thereby a very disagreeable empyreumatic smell. They must be first soaked as much as possible, and then broken by a single blow in passing them between two grooved cylinders, or under a mell sufficiently heavy. On a small scale we may make use of a dish and a mass of wood, both covered with an iron plate cut into diamond points. In either case care must be taken to soak the pieces of bone in water which are to be broken again. They ought to be used immediately after, or else kept plunged in fresh water, or, what is better, in water almost saturated with salt. The bones, subjected a short time after to steam strongly condensed, and to a dry heat of 130 to 140° C. break very easily; but this process is subject to the inconvenience of detaching parts of the fat; and it should only be employed upon those which do not contain any fat, or upon old and dirty bones.

The preservation of bones as an alimentary substance is of the highest importance, as they easily turn to putrefaction.

A portion of the gelatine changes into ammonia, and this, by combining with the gelatine not decomposed, takes away its property of turning into jelly by cold, and makes it soluble in cold water.

Various methods of preservation have been tried. In all cases we should begin by depriving the bones of the fat, or else they acquire in time a rancid smell, which renders them useless.

The broken bones boiled in a copper with water furnish a great deal of fat, but they still retain enough to turn rancid. It has been proposed to remove it by saponifying, with soda, the remaining fat; but the success has not been complete: the gelatine is often altered; and retains a disagreeable smell of soap. The salting, which can be applied even to the fresh bones, is preferable; if on a great scale it is not too expensive; and if the products are as agreeable as in the fresh state.

The method which appears to me to succeed best is that of Plowden for the preservation of meat, which consists in immersing the meat into a strong solution of the juice of meat or of gelatine, and then drying them in the open air. I took a solution of gelatine containing about twenty centiemes of dry gelatine; I heated it to 80° or 90°, and I dipped in it several times the clear bones, broken into little bits, and stripped of their fat. The bones when taken out of the solution of gelatine are put to dry upon strings in the open air, and then treated once or twice in the same manner, to increase the thickness of the layer of gelatine. It is followed by a perfect desiccation in a stove at 20° or 25°.

The extraction of the gelatine of bones is yet but imperfectly known, and demands particular attention. It appears that the first attempt was made by Papin, who proposed in 1681, for this end to employ condensed steam; but the gelatine obtained by this process was almost always altered, had a disagreeable empyreumatic smell, and was no longer a jelly.

Braconnot, is a mixture of seventy parts of marine salt, and thirty of chloride of potassium. Evaporated when it comes out of the cylinder, after being seasoned with herbs or with the juice of meat, we obtain either cakes of gelatine or cakes of soup. Their uses are numerous, and it is easy to perceive all the advantages of this most salubrious alimentary substance. The fat contained in the bones turns into soap very quickly when it is exposed to the action of compressed steam. It is advantageous to take off the fat with boiling water, or even at a lower temperature, because the fat is much better if exposed to a low heat. The bones give out their fat very quickly in steam a little condensed; but the quantity of fat changed into soap, and which remains in insoluble combination with the lime, rises to four or five centiemes weight of bones, and such a loss ought to be avoided.

An apparatus composed of four cylinders is established at the Hospital de la Charité. Each cylinder is 1^m in height, and 0^m, 333 in diameter, and contains about 40^k of bones, furnishing about a thousand basins of soup per day.

The importance of the work of M. D'Arcet has induced us to give an abstract of the principal part of it. For farther details, we must refer our readers to the original memoir, printed in the *Annals of Industry* for February 1829.

Art. XIII.—*Abstract of M. Kupffer's Memoir on the Specific Gravity of Metallic Alloys and their Melting Points.*

In Kastner's *Archiv.* tom. vii. p. 331, M. Kupffer has shown that alloys have always a specific gravity less than the calculated specific gravity, that is, that they dilate in mixing, and that the value of this dilatation reaches its *minimum* when they are united in a proportion which approaches nearly to that of two atoms of tin to one atom of lead. More recently he has found that the amalgams of tin experience a considerable contraction, which always diminishes, beginning with the amalgam composed of two atoms of tin and one atom of mercury, and which ends by becoming very small in an amalgam of one atom of tin and two atoms of mercury. By increasing the number of atoms of mercury the contraction increases again, so that in the amalgam of one atom of tin to three of mercury the contraction is nearly as great as in the amalgam of one atom of tin and one atom of mercury. Hence it was probable that between the proportion of one atom of tin to two atoms of mercury, and one atom of tin to three atoms of mercury, there would be found an amalgam in which the contraction is nothing. He found, indeed, that *tin* and *lead* may be combined in such a proportion that the alloy has neither contraction nor dilatation of bulk, and that when this happens the proportions of their elements are to their specific gravities in a very simple ratio.

I. *Alloys of Tin and Lead.*

The following are some of the results :—

	Atoms.		Atoms.	Specific Gravity. Observed.	Specific Gravity. Calc.	Difference.
		Lead,		11.3303		
		Tin,		7.2911		
Tin,	1	Lead,	1	9.4263	9.4366	0.0103
—	1	——	2	10.0782	10.0936	0.0154
—	1	——	3	10.3868	10.4122	0.0254
—	1	——	4	10.5551	10.6002	0.0431
—	2	——	1	8.7454	8.7518	0.0064
—	3	——	1	8.3914	8.3983	0.0069
—	4	——	1	8.1730	8.1826	0.0096
—	5	——	1	8.0279	8.0372	0.0093
—	6	——	1	7.2210	7.9326	0.0116

Hence it appears that there will be an alloy between tin 2 and lead 1, and tin 3 and lead 1, where the dilatation should be nothing. M. K. then took an alloy of *one* volume of lead to *two* volumes of tin, and found the specific gravity to be 8.6371, while the calculated specific gravity is 8.6375.

II. *Amalgams of Tin and Mercury.*

The following experiments were made on the amalgams of tin and mercury.

	Atoms.		Atoms.	Specific Grav. Obs.	Specific Grav. Calc.	Ratio.
Tin,	3	Mercury,	1	8.8218	8.7635	1.006632
—	2	———	1	9.3185	9.2658	1.005685
—	1	———	1	10.3447	10.2946	1.004865
—	1	———	2	11.3816	11.3480	1.002960

The following experiments were made upon amalgams, in which the quantities of the metals were proportional to their specific gravities.

	Vol.		Vol.	Specific Grav. Obs.	Calc.	Ratio.
Tin,	1	Mercury,	1	10.4729	10.4240	1.00469
—	1	———	2	11.4646	11.4683	
—	1	———	3	12.0257	11.9905	1.00294

These observations prove that the tin and mercury experience in general a considerable contraction in amalgamating; but this contraction is nothing when one volume of tin is alloyed with two volumes of mercury.—The specific gravity of the mercury at 17° cent. is 13.5569, and that of the tin 7.2911. The specific gravities were calculated by the formula $\frac{(W + w)\, S s}{W s + w S}$ where W, *w*, denote the weight, and S, *s*, the specific gravities of the alloyed metals.

III. *Amalgams of Lead and Mercury.*

The following experiments were made on the amalgams of lead and mercury.

				Specific Grav. Obs.	Calc.	Ratio.
Lead,	1	Mercury,	4	13.1581	13.1116	1.00355
———	1	———	3	13.0397	13.0003	1.00303
———	1	———	2	12.8648	12.8147	1.00392

Hence the amalgam composed of one volume of lead and three of mercury experiences the least contraction.

Another curious result may be deduced from these observations, viz. that the dilatation of all these amalgams by heat is smaller than what is obtained by calculation, on the supposition that each metal preserves the dilatation which belongs to it. Hence it follows that the approach of the molecules increases the resistance which their mutual attraction opposes to the effect of heat.

IV. *On the melting points of the preceding Alloys.*

In remelting the alloys of tin and lead, which had been employed in the preceding experiments, M. Kupffer had occasion to observe their melting points, which were as follows:—

		Melting Point, Centigrade.
Lead,		334°
Tin,		230
Tin, 5 atoms,	Lead 1 atom,	194
—— 4	—— 1	189
—— 3	—— 1	186
—— 2	—— 1	196
—— 1	—— 1	241
—— 1	—— 3	289
—— 2 volumes,	—— 1 volume,	194

These temperatures were determined by a small thermometric bulb terminated by a very short capillary tube filled with mercury at a determinate temperature, (that of melting ice for example.) This bulb was plunged into the melting alloy, and when the mercury which run over it was removed, it was carefully weighed. The results of these weighings were calculated after the experiments of M. Dulong, and consequently give the temperatures immediately in degrees of the air thermometer.

The preceding article is a very general abstract of the original, which is published in the *Ann. de Chim.* tom. xl. p. 285—302.

Art. XIV.—*Contributions to Physical Geography.*

1. *Account of an extraordinary Avalanche in the White Mountains of New Hampshire, which took place on the 28th August* 1826. By Professor Silliman, Rev. C. Wilcox, and Mr T. Baldwin.*

The whole day's ride, in an open waggon, has been in the winding defile of mountains, which probably have not their equal in North America, until we reach the Rocky Mountains. The portion of the Notch which is the grandest, is about five or six miles in length; it is composed of a double barrier of mountains, rising very abruptly from both sides of the wild

* Abridged from Professor Silliman's *Journal of Science,* vol. xv. No. 2, p. 216—233. Jan. 1829.

roaring river Saco, which frequently washes the feet of both barriers; and sometimes there is not room for a single carriage to pass between the stream and the mountains; but the road is cut into the mountain itself. Imagine this double barrier, rising on each side, to the height of nearly half a mile in perpendicular altitude, often exceeding this height, and capped here and there, by proud castellated turrets, standing high above the continued ridges; these are not straight, but are formed into numerous zigzag turns, which frequently cut off the view, and seem to imprison you in a vast gloomy gulf. But the most remarkable fact remains to be stated.

The sides of the mountains are deeply furrowed and scarred, by the tremendous effects of the memorable deluge of August 28th, 1826. I will recal to your recollection the awful catastrophe, which, on the night succeeding that day, destroyed, in a moment, the worthy Willey family, nine in number, and left not one to tell their painful story. For two seasons before, the mountains had been very dry, and on the morning of August 28th, it commenced raining very hard, with strong tempestuous wind; the storm lasted through that day and the succeeding night, and when it ceased, the road was found obstructed by innumerable avalanches of mountain ruins, which rendered it impossible to pass, except on foot. The first traveller who came to the Willey house found it empty of its inhabitants, and in the course of a few days the mangled bodies of seven out of nine were found about fifty or sixty rods from the house, buried beneath the drift-wood and mountain ruins, on the bank of the Saco, or rather in the midst of what was for the time a vast raging torrent, uniting one mountain barrier to the other. The effects of the torrents, which on that occasion descended from the mountains, now form a most conspicuous and interesting feature in the scenery.

The avalanches were very numerous; they were not, however, ruptures of the main foundation rock of the mountain, but *slides*, from very steep declivities; beginning, in many instances, at the very mountain top, and carrying down, in one promiscuous and frightful ruin, forests, and shrubs, and the earth which sustained them; stones and rocks innumerable, and many of great size, such as would fill each a common apartment; the slide took every thing with it, down to the solid mountain rock, and being produced by torrents of water, which appear to have *burst* like water spouts upon the mountains, after they had been thoroughly soaked with heavy rains, thus loosening all the materials that were not solid, and the trees pushed and wrung by fierce winds, acted as so many levers, and prepared every thing for the awful catastrophe. No tradition existed of any slide in former times, and such as are now observed to have formerly happened, had been completely veiled by forest growth and shrubs. At length, on the 28th of June, two months before the *fatal* avalanche, there was *one* not far from the Willey house, which so far alarmed the family, that they erected an encampment a little distance from their dwelling, intending it as a place of refuge. On the fatal night, it was impenetrably dark and frightfully tempestuous; the lonely family had retired to rest, in their humble dwelling, six miles from the nearest human creature. The avalanches descended in every part of the gulf for a distance of two miles; and a very heavy one began on the mountain top, immediately above the house, and descended in a direct line towards it; the sweeping torrent, a river from the clouds, and a river full of trees, earth, stones, and rocks, rushed to the house and marvellously divided within six feet of it, and just behind it, and passed on either side, sweeping away the stable and horses, and completely encircling the dwelling, but leaving it untouched. At this time, probably towards midnight, (as the state of the beds and apparel, &c. showed that they had retired to rest,) the family probably issued from their house, and were swept away by the torrent; five beautiful children, from twelve to two years of age, being of the number.

Search was, for two or three days, made in vain, for the bodies, when they were at length found, in consequence of the swarms of flies which, it being hot weather, were hovering over the places. The bodies were evidently floated along by the torrent, and covered by the drift-wood. A pole, with a board nailed across it, like a guide post, now indicates the spot where the bodies where found, and we saw remnants of their apparel still sticking among the splinters of the shattered trees. Had the family remained in the house they would have been entirely safe. Even the little green in front and east of the house was undisturbed, and a flock of sheep, (a part of the possession of the family,) remained on this small spot of ground, and were found there the next morning in safety—although the torrent dividing just above the house, and forming a curve on both sides, had swept completely around them, and again united below, and covered the meadows and orchards with ruins, which remain there to this hour. This catastrophe presents a very striking example of sudden diluvial action, and enables one to form some feeble conception of the universal effects of the vindictive deluge which once swept every mountain, and ravaged every plain and defile.—In the present instance, there was not one avalanche only, but many. The most extensive single one was on the other side of the barrier which forms the northern boundary of the notch. It was described to us by Mr Abbot of Conway, as having slid, in the whole, three miles—with an average breadth of a quarter of a mile; it overwhelmed a bridge, and filled a river course, turning the stream, and now presents an unparalleled mass of ruins. There are places on the declivities of the mountains in the notch, where acres of the steep sides were swept bare of their forests, and of every moveable thing, and the naked rock is now exposed to view.

In the greater number of instances, however, the avalanches commenced almost at the mountain top, or high upon its slope. We pursued some of them to a considerable distance up the mountain, and two gentlemen of our party with much toil followed one of them quite to the summit. The excavation commencing, generally, as soon as there was any thing moveable—in a trench of a few yards in depth, and of a few rods in width, descends down the mountains—widening and deepening—till it becomes a frightful chasm, like a vast irregular hollow cone, with its apex near the mountain top, and its base at its foot, and there spreading out into a wide and deep mass of ruins of transported earth, gravel stones, rocks, and forest trees."

At the time when this extraordinary accident happened, the Rev. C. Wilcox was on an excursion to the White Mountains, and saw the extraordinary effects of the avalanche and ruin the very day after the event. His account of it is highly interesting, and we copy it verbatim, though it contains a slight repetition respecting the fate of the Willey family.

"I left Hanover on Saturday last in company with two gentlemen of my acquaintance from the city of New York, and rode as far as Haverhill, where we all spent the Sabbath. The road over which we passed was like a bed of ashes two or three inches deep; and the country around us exhibited the usual effects of a long drought. The abundant rains that fell three weeks ago over the southern half of New England did not reach the upper part of the valley of Connecticut River. On Monday morning it began to rain at Haverhill, and continued along our route for most of the day, but so moderately, and at such intervals, that, with the help of great coats and umbrellas, we proceeded on our journey in an open waggon as far as Bethlehem, fifteen miles west of the White Mountains. As we approached the vicinity of the mountains, the rain increased till it became a storm, and compelled us to stop about the middle of the afternoon.

The storm continued most of the night; but the next morning was clear and serene. The view from the hill of Bethlehem was extensive and delightful. In the eastern horizon, Mount Washington, with the neighbouring peaks on the north and on the south, formed a grand outline far up in the blue sky. Two or three small fleecy clouds rested on its side, a little below its summit, while from behind this highest point of land in the United States east of the Mississippi, the sun rolled up rejoicing in his strength and glory. We started off towards the object of our journey, with spirits greatly exhilarated by the beauty and grandeur of our prospect. As we hastened forward with our eyes fixed on the tops of the mountains before us, little did we think of the scene of destruction around their base, on which the sun was now for the first time beginning to shine. In about half an hour we entered Breton Woods, an unincorporated tract of land covered with primitive forest, extending on our road five miles to Rosebrook's Inn, and thence six miles to Crawford's, the establishment begun by Rosebrook's father, as described in the travels of Dr Dwight. On entering this wilderness we were struck with its universal stillness. From every leaf in its immense masses of foliage the rain hung in large glittering drops; and the silver note of a single unseen and unknown bird was the only sound that we could hear. After we had proceeded a mile or two, the roaring of the Amonoosuck began to break in upon the stillness, and soon grew so loud as to excite our surprise. In consequence of coming to the river almost at right angles, and by a very narrow road, through trees and bushes very thick, we had no view of the water, till with a quick trot we had advanced upon the bridge too far to recede, when the sight that opened at once to the right and to the left drew from all of us similar exclamations of astonishment and terror; and we hurried over the trembling fabric as fast as possible. After finding ourselves safe on the other side we walked down to the brink; and, though familiar with mountain scenery, we all confessed that we had never seen a mountain torrent before. The water was as thick with earth as it could be, without being changed into mud. A man living near in a log hut showed us how high it was at daybreak. Though it had fallen six feet, he assured us that it was still ten feet above its ordinary level. To this add its ordinary depth of three or four feet, and here at daybreak was a body of water twenty feet deep, and sixty feet wide, moving with the rapidity of a gale of wind, between steep banks covered with hemlocks and pines, and over a bed of large rocks, breaking its surface into billows like those of the ocean. After gazing a few moments on this sublime sight we proceeded on our way, for the most part at some distance from the river, till we came to the farm of Rosebrook, lying on its banks. We found his fields covered with water, and sand, and flood-wood. His fences and bridges were all swept away; and the road was so blocked up with logs that we had to wait for the labours of men and oxen before we could get to his house. Here we were told that the river was never before known to bring down any considerable quantity of earth, and were pointed to bare spots on the sides of the White Mountains never seen till that morning. As our road, for the remaining six miles, lay quite near the river, and crossed many small tributary streams, we employed a man to accompany us with an axe. We were frequently obliged to remove trees from the road, to fill excavations, to mend and make bridges, or contrive to get our horses and waggon along separately. After toiling in this manner for half a day, we reached the end of our journey, not, however, without being obliged to leave our waggon half a mile behind. In many places in these six miles, the road and the whole adjacent woods, as it appeared from the marks on the trees, had been overflowed to the depth of ten feet. In one place the river, in consequence of some obstruction at a remarkable fall, had been twenty feet higher than it was when we passed. We stopped to view the fall, which Dr Dwight calls "beautiful." He says of it—"The descent is from fifty to sixty feet, cut through a mass of stratified granite; the sides of which appear as if they had been laid by a mason in a variety of fantastical forms; betraying, however, by their rude and wild aspect, the masterly hand of nature." This description is sufficiently correct; but the beauty of the fall was now lost in its sublimity. You have only to imagine the whole body of the Amonoosuck, as it appeared at the bridge which we crossed, now compressed to half of its width, and sent downward at an angle of twenty or twenty-five degrees between perpendicular walls of stone. On our arrival at Crawford's the appearance of his farm was like that of Rosebrook's, only much worse. Some of his sheep and cattle were lost; and eight hundred bushels of oats were destroyed. Here we found five gentlemen, who gave us an interesting account of their unsuccessful attempt to ascend Mount Washington the preceding day. They went to the "Camp" at the foot of the mountain on Sabbath evening, and lodged there with the intention of climbing the summit the next morning. But in the morning the mountains were enveloped in thick clouds; the rain began to fall, and increased till afternoon, when it came down in torrents. At five o'clock they proposed to spend another night at the camp, and let their guide return home for a fresh supply of provisions for the next day. But the impossibility of keeping a fire where every thing was so wet, and the advice of their guide, made them all conclude to return, though with great reluctance. No time was now to be lost, for they had seven miles to travel on foot, and six of them by a rugged path through a gloomy forest. They ran as fast as their circumstances would permit; but the dark evergreens around them, and the black clouds above, made it night before they had gone half the way. The rain poured down faster every moment; and the little streams, which they had stepped across the evening before, must now be crossed by wading, or by cutting down trees for bridges, to which they were obliged to cling for life. In this way they reached the bridge over the Amonoosuck near Crawford's just in time to pass it before it was carried down the current. On Wednesday, the weather being clear and beautiful, and the waters having subsided, six gentlemen, with a guide, went to Mount Washington, and one accompanied Mr Crawford to the "Notch," from which nothing had yet been heard. We met again at evening, and related to each other what we had seen. The party who went to the mountain were five hours in reaching the site of the camp, instead of three, the usual time. The path for nearly one-third of the distance was so much excavated, or covered with miry sand, or blocked up with flood-wood, that they were obliged to grope their way through thickets almost impenetrable, where one generation of trees after another had risen and fallen, and were now lying across each other in every direction, and in various stages of decay. The camp itself had been wholly swept away; and the bed of the rivulet by which it had stood was now more than ten rods wide, and with banks from ten to fifteen feet high. Four or five other brooks were passed, whose beds were enlarged some of them to twice the extent of this. In several, the water was now only three or four feet wide, while the bed of ten, fifteen, or twenty rods in width, was covered for miles with stones from two to five feet in diameter, that had been rolled down the mountains, and through the forests, by thousands, bearing every thing before them. Not a tree, nor the root of a tree remained in their path. Immense piles of hemlocks, and other trees with their limbs and bark entirely bruised off, were lodged all the way on both sides, as they had been driven in among the standing and half standing trees on the banks. While the party were climbing the mountain, thirty "slides" were counted, some of which began near the line where the soil and vegetation terminate, and growing wider as they descended, were estimated to contain more than a hundred acres. These were all on the western side of the mountains. They were composed of the whole surface of the earth, with all its growth of woods, and its loose rocks, to the depth of fifteen, twenty, and thirty feet. And wherever the slides of the two projecting mountains met, forming a vast ravine, the depth was still greater.

Such was the report which the party from the mountains gave. The intelligence which Mr Crawford, and the gentleman accompanying him, brought from the Notch, was of a more melancholy nature. The road, though a turnpike, was in such a state, that they were obliged to walk to the Notch House, lately kept by Mr Willey, a distance of six miles. All the bridges over the Amonoosuck, five in number, those over the Saco, and those over the tributary streams of both, were gone. In some places the road was excavated to the depth of fifteen and twenty feet; and in others it was covered with earth, and rocks, and trees, to as great a height. In the Notch, and along the deep defile below it, for a mile and a-half, to the Notch House, and as far as could be seen beyond it, no appearance of the road, except in one place for two or three rods, could be discovered. The steep sides of the mountain, first on one hand, then on the other, and then on both, had slid down into this narrow passage, and formed a continued mass from one end to the other, so that a turnpike will probably not be made through it again very soon, if ever. The Notch House was found uninjured; though the barn adjoining it by a shed, was crushed; and under its ruins were two dead horses. The house was entirely deserted; the beds were tumbled; their covering was turned down; and near them upon chairs and on the floor lay the wearing apparel of the several members of the family; while the money and the papers of Mr Willey were lying in his open bar. From these circumstances it seemed almost certain, that the whole family were destroyed; and it soon became quite so, by the arrival of a brother of Mr Crawford from his father's, six miles farther east. From him we learnt that the valley of the Saco for many miles presented an uninterrupted scene of desolation.

The two Crawfords were the nearest neighbours of Willey. Two days had now elapsed since the storm, and nothing had been heard of his family in either direction. There was no longer any room to doubt that they had been alarmed by the noise of the destruction around them, had sprung from their beds, and fled naked from the house, and in the utter darkness had been soon overtaken by the falling mountains and rushing torrents. The family, which is said to have been amiable and respectable, consisted of nine persons, Mr Willey and his wife and five young children of theirs, with a hired man and boy. After the fall of a single slide last June, they were more ready to take the alarm, though they did not consider their situation dangerous, as none had ever been known to fall there previous to this. Whether more rain fell now than had ever been known to fall before in the same length of time, at least since the sides of the mountains were covered with so heavy a growth of woods, or whether the slides were produced by the falling of such a quantity of rain so suddenly, after the earth had been rendered light and loose by the long drought, I am utterly unable to say. All I know is, that at the close of a rainy day, the clouds seemed all to come together over the White Mountains, and at midnight discharge their contents at once in a terrible burst of rain, which produced the effects that have now been described."

The following is a notice of the same event by M. T. Baldwin, who saw the spot in May 1828, and who has stated some particulars of great interest.

" In its whole course before reaching Mill Brook, it swept through a dense forest, mostly of hemlock and spruce, and took off the entire surface, and every thing which it contained. The ground appeared to be as free from roots as if it had been tilled for fifty years. We observed some trees so firmly rooted in the rocks, that they could not be drawn out, which were pounded off upon a level with the surface of the ground, as if they had been but slender reeds. At some distance above the stream the mass parted, and left a few rods square of timber standing—but soon united again—and rushing on in all its tremendous power, struck obliquely against the opposite bank of Mill Brook, with a concussion that must have shaken the everlasting hills. This bank rises very precipitously and forms the base of another peak, which towers to a great height. At this place we judged the width of the desolation to be twenty-five or thirty rods. As the frightful moving mass now struck against an immoveable barrier, and its line of direction must be changed before it could follow the course of the stream, we should expect a greater accumulation of water, &c. at this place, than at any other; and just below the point where this wreck of the mountain tumbled into Mill Brook, I should not think it exaggeration to say, that a perpendicular, raised from the bed of the stream as it now runs, to a line drawn across the channel, and connecting points on either side where logs, sticks, &c. lie in such a manner, as to show that they must have been washed there by the current, would equal one hundred feet in length. It is certainly *surprising*, how, even on a mountain as precipitous as this—such a mass starting with a width of only four rods, could acquire sufficient momentum to carry before it an entire forest, and rocks of an enormous size: but gravity created that resistless power, which could so many times change its direction and urge it down the stream, in defiance of all the obstacles that opposed its progress, and where the elevation was constantly lessening. The principal and immediate agent was water, otherwise the mass would not have proceeded farther than where it struck Mill Brook—for it is easy to see that a mass composed merely of trees, and rocks, and sand, however enormous its bulk or tremendous its momentum, could not have gone much farther than the first two hundred rods. But how could the water accumulate on the sides of that precipitous mountain to the depth of thirty feet? This question arose as I stood gazing in astonishment, and I was strongly inclined to pronounce it impossible, notwithstanding facts which undeniably proved the contrary, that were staring me in the face. But it will not appear incredible when we consider that the timber above Mill Brook was principally hemlock and spruce, the boughs of which would be extremely well calculated to produce an obstruction of the flood. A dam might easily be formed of the logs, boughs, rocks and earth, which composed this mighty moving mass, and the upturning of thousands of trees with the soil adhering to their roots, would greatly aid in effecting the object. And this appears to have been its *modus operandi* throughout the whole course. The ground was desperately disputed, but whenever a check was given to its progress, the foaming torrent would accumulate behind, till it had gathered sufficient force to burst every barrier—and again the huge pile proceeded thundering down the mountain. The forest seems to have been prostrated with as much ease as if it had been but a field of grain. The mass evidently went down in the wildest confusion. The trees sometimes erect, or sweeping around their branchless trunks in "horrid circles," would level tremendous blows at those upon the banks of the stream—as appeared by the bark frequently taken off at a great height—now their tops and roots alternately projecting forward, and again lying across the current were shivered in an instant. They are left in considerable numbers throughout the whole course, some lying upon the banks, others in the channel, and wholly or in part buried in the sand and rocks. But the principal part of the timber swept from these twenty-five acres lies piled in a confused heap, covering perhaps an acre of ground, and four hundred and eighty rods, (one and a-half mile,) from the spot where the slide commenced! Here having already spent much of its force, and the mountain growing less precipitous, it struck into a cluster of firmly rooted trees and was compelled to stop. At this place it presents a perpendicular wall of logs, &c. across the entire channel, in some places ten or fifteen feet high. The upper end of the pile is buried beneath the sand and stones, and the stream now runs over the top. Perhaps those very logs will be dug out in after times as fossil wood.

Every thing in this mass bears the marks of the greatest violence. Almost every tree is as completely divested of its roots, branches, and bark, as could have been effected by man with the proper instruments. They are pounded, and splintered and broken into all imaginable shapes and lengths. We felt ourselves amply repaid for our labour. It is well worth the attention of the lovers of the marvellous, and especially of every one who has never witnessed such tremendous effects accomplished by the agency of water. I shall never more doubt, that water is adequate to the production of any of those effects, which are generally ascribed to the deluge.

2. *Account of Earthquakes on the Mississippi.*

From all the accounts, corrected one by another, and compared with the very imperfect narratives that were published, I infer, that the shock of these earthquakes, in the immediate vicinity of the centre of their course, must have equalled in their terrible heavings of the earth, any thing of the kind that has been recorded. I do not believe that the public have ever yet had any adequate idea of the violence of the concussions. We are accustomed to measure this, by the buildings overturned, and the mortality that results. Here the country was thinly settled. The houses fortunately were frail and of logs, the most difficult to overturn that could be constructed. Yet as it was, whole tracts were plunged into the bed of the river. The grave-yard at new Madrid, with all its sleeping tenants, was precipitated into the bend of the stream. Most of the houses were thrown down. Large lakes of twenty miles in extent were made in an hour; other lakes were drained. The whole country to the mouth of the Ohio in one direction, and to the St Francis in the other, including a front of three hundred miles, was convulsed to such a degree as to create lakes and islands, the number of which is not yet known,—to cover a tract of many miles in extent near the Little Prairie, with water three or four feet deep; and when the water disappeared, a stratum of sand of the same thickness was left in its place. The trees split in the midst, lashed one with another, and are still visible over great tracts of country, inclining in every direction, and at every angle to the earth and to the horizon.

They described the undulations of the earth as resembling waves, increasing in elevation as they advanced, and when they had attained a certain fearful height, the earth would burst, and vast volumes of water and sand and pit-coal were discharged, as high as the tops of the trees. I have seen a hundred of these chasms which remained fearfully deep, although in a very tender alluvial soil, and after a lapse of seven years. Whole districts were covered with white sand, so as to become uninhabitable.

The water at first covered the whole country, particularly at the Little Prairie; and it must have been indeed a scene of horror, in these deep forests, and in the gloom of the darkest night, and by wading in the water to the middle, to fly from these concussions, which were occurring every few hours, with a noise equally terrible to the beasts and birds, as to men. The birds themselves lost all power and disposition to fly, and retreated to the bosoms of men, their fellow-sufferers in this scene of convulsion. A few persons sunk in these chasms and were providentially extricated. One person died of fright. One perished miserably on an island, which retained its original level, in the midst of a wide lake created by the earthquake. The hat and clothes of this man were found. A number perished who sunk with their boats in the river. A bursting of the earth, just below the village of New Madrid, arrested this mighty stream in its course, and caused a reflux of its waves, by which in a little time a great number of boats were swept by the ascending current into the mouth of the *Bayou*, carried out and left upon the dry earth, when the accumulating waters of the river had again cleared their current.

There were a great number of severe shocks, but two series of concussions were particularly terrible, far more so than the rest. They remark that the shocks were clearly distinguishable into two classes; those in which the motion was horizontal, and those in which it was perpendicular. The latter were attended by the explosions and the terrible mixture of noises, that preceded and accompanied the earthquakes, in a louder degree, but were by no means so desolating and destructive as the other. When they were felt, the houses crumbled, the trees waved together, the ground sunk, and all the destructive phenomena were more conspicuous. In the intervals of the earthquakes, there was one evening, and that a brilliant and cloudless one, in which the western sky was a continued glare of vivid flashes of lightning, and of repeated peals of subterranean thunder, seemed to proceed as the flashes did from below the horizon. They remark that this night, so conspicuous for subterranean thunder, was the same period in which the fatal earthquakes at Caraccas occurred.

and they seem to suppose these flashes and that event parts of the same scene.

The people without exception were unlettered back-woodsmen, of the class least addicted to reasoning. And yet it is remarkable how ingeniously and conclusively they reasoned from apprehension sharpened by fear. They remarked that the chasms in the earth were in direction, from south-west to north-east, and they were of an extent to swallow up not only men but houses, "down quick into the pit;" and these chasms occurred frequently within intervals of half a mile. They felled the tallest trees at right angles with the chasms; and stationed themselves upon the felled trees. By this invention all were saved; for the chasms occurred more than once under these trees.—Flint's *Travels*.

3. *On the Motion of Large Stones, &c. in Lakes and Ponds.* By Mr N. Chissman.

There is in Tinmouth a pond about a mile long and half a mile broad. In 1775 I observed several large stones, some of which may be called rocks, lying in the edge of the water, which appeared to have been forced forward in a line inclining to the shore by some powerful cause, leaving behind them channels of considerable length, and the largest having the largest channels. Year after year I observed that they had been impelled in the same direction. In 1782 circumstances persuaded me that ice had been the agent; and in the spring of 1783, when the ice was moving to the north in a large field before a south wind, I placed myself by a large stone on the western shore, which appeared to have been much moved in preceding years. The ice approached almost imperceptibly. When it met the stone the thinner edge of the ice gave way a little and broke off, but it soon became strong enough for its task. As soon as the ice had taken a firm hold of the stone, I heard a grating noise of the gravel beneath, and plainly saw the motion of the stones, as well as of the gravel and the earth heaped up before it. I observed it while it was moved a foot or more, when its progress was arrested by the ice swinging round against the eastern shore of the pond.—Abridged from Silliman's *Journal*, No. 30, p. 303.

Art. XV.—*Additional Remarks on Active Molecules.* By Robert Brown, F. R. S.

About twelve months ago I printed an account of Microscopical Observations made in the summer of 1827, on the Particles contained in the Pollen of Plants; and on the general Existence of active Molecules in Organic and Inorganic Bodies.

In the present Supplement to that account, my objects are, to explain and modify a few of its statements, to advert to some of the remarks that have been made, either on the correctness or originality of the observations, and to the causes that have been considered sufficient for the explanation of the phenomena.

In the first place, I have to notice an erroneous assertion of more than one writer, namely, that I have stated the active Molecules to be animated. This mistake has probably arisen from my having communicated the facts in the same order in which they occurred, accompanied by the views which presented themselves in the different stages of the investigation; and in one case, from my having adopted the language, in referring to the opinion, of another inquirer into the first branch of the subject.

Although I endeavoured strictly to confine myself to the statement of the facts observed, yet in speaking of the active Molecules I have not been able, in all cases, to avoid the introduction of hypothesis; for such is the supposition, that the equally active particles of greater size, and frequently of very different form, are primary compounds of these Molecules,—a supposition which, though professedly conjectural, I regret having so much insisted on, especially as it may seem connected with the opinion of the absolute identity of the Molecules, from whatever source derived.

On this latter subject, the only two points that I endeavoured to ascertain, were their size and figure: and although I was, upon the whole, inclined to think that in these respects the Molecules were similar from whatever substances obtained, yet the evidence then adduced in support of the supposition was far from satisfactory; and I may add, that I am still less satisfied now that such is the fact. But even had the uniformity of the Molecules in those two points been absolutely established, it did not necessarily follow, nor have I any where stated, as has been imputed to me, that they also agreed in all their other properties and functions.

I have remarked, that certain substances, namely, sulphur, resin, and wax, did not yield active particles, which, however, proceeded merely from defective manipulation; for I have since readily obtained them from all these bodies: at the same time I ought to notice that their existence in sulphur was previously mentioned to me by my friend Mr Lister.

In prosecuting the inquiry subsequent to the publication of my Observations, I have chiefly employed the simple microscope mentioned in the Pamphlet, as having been made for me by Mr Dollond, and of which the three lenses that I have generally used, are of a 40th, 60th, and 70th of an inch focus.

Many of the observations have been repeated and confirmed with other simple microscopes having lenses of similar powers, and also with the best achromatic compound microscopes, either in my own possession or belonging to my friends.

The result of the inquiry at present essentially agrees with that which may be collected from my printed account, and may be here briefly stated in the following terms, namely,

That extremely minute particles of solid matter, whether obtained from organic or inorganic substances, when suspended in pure water, or in some other aqueous fluids, exhibit motions for which I am unable to account, and which from their irregularity and seeming independence resemble in a remarkable degree the less rapid motions of some of the simplest animalcules of infusions. That the smallest moving particles observed, and which I have termed Active Molecules, appear to be spherical, or nearly so, and to be between 1-20,000dth and 1-30,000dth of an inch in diameter; and that other particles of considerably greater and various size, and either of similar or of very different figure, also present analogous motions in like circumstances.

I have formerly stated my belief that these motions of the particles neither arose from currents in the fluid containing them, nor depended on that intestine motion which may be supposed to accompany its evaporation.

These causes of motion, however, either singly or combined with others,—as, the attractions and repulsions among the particles themselves, their unstable equilibrium in the fluid in which they are suspended, their hygrometrical or capillary action, and in some cases the disengagement of volatile matter, or of minute air bubbles,—have been considered by several writers as sufficiently accounting for the appearances. Some of the alleged causes here stated, with others which I have considered it unnecessary to mention, are not likely to be overlooked or to deceive observers of any experience in microscopical researches: and the insufficiency of the most important of those enumerated, may, I think, be satisfactorily shown by means of a very simple experiment.

This experiment consists in reducing the drop of water containing the particles to microscopic minuteness, and prolonging its existence by immersing it in a transparent fluid of inferior specific gravity, with which it is not miscible, and in which evaporation is extremely slow. If to almond-oil, which is a fluid having these properties, a considerably smaller proportion of water, duly impregnated with particles, be added, and the two fluids shaken or triturated together, drops of water of various sizes, from 1-50th to 1-2000dth of an inch in diameter, will be immediately produced. Of these, the most minute necessarily contain but few particles, and some may be occasionally observed with one particle only. In this manner minute drops, which if exposed to the air would be dissipated in less than a minute, may be retained for more than an hour. But in all the drops thus formed and protected, the motion of the particles takes place with undiminished activity, while the principal causes assigned for that motion, namely, evaporation and their mutual attraction and repulsion, are either materially reduced or absolutely null.

It may here be remarked, that those currents from centre to circumference, at first hardly perceptible, then more obvious, and at last very rapid, which constantly exist in drops exposed to the air, and disturb or entirely overcome the proper motion of the particles, are wholly prevented in drops of small size immersed in oil,—a fact which, however, is only apparent in those drops that are flattened, in consequence of being nearly or absolutely in contact with the stage of the microscope.

That the motion of the particles is not produced by any cause acting on the surface of the drop, may be proved by an inversion of the experiment; for by mixing a very small proportion of oil with the water containing the particles, microscopic drops of oil of extreme minuteness, some of them not exceeding in size the particles themselves, will be found on the surface of the drop of water, and nearly or altogether at rest; while the particles in the centre or towards the bottom of the drop continue to move with their usual degree of activity.

By means of the contrivance now described for reducing the size and prolonging the existence of the drops containing the particles, which, simple as it is, did not till very lately occur to me, a greater command of the subject is obtained, sufficient perhaps to enable us to ascertain the real cause of the motions in question.

Of the few experiments which I have made since this manner of observing was adopted, some appear to me so curious, that I do not venture to state them until they are verified by frequent and careful repetition.

I shall conclude these supplementary remarks to my former Observations, by noticing the degree in which I consider those observations to have been anticipated.

That molecular was sometimes confounded with animalcular motion by several of the earlier microscopical observers, appears extremely probable from various passages in the writings of Leeuwenhoek, as well as from a remarkable Paper by Stephen Gray, published in the 19th volume of the *Philosophical Transactions.*

Needham also, and Buffon, with whom the hypothesis of organic particles originated, seem to have not unfrequently fallen into the same mistake. And I am inclined to believe that Spallanzani, notwithstanding one of his statements respecting them, has under the head of *Animaletti d'ultimo or-*

dine included the active Molecules as well as true Animalcules.

I may next mention that Gleichen, the discoverer of the motions of the Particles of the Pollen, also observed similar motions in the particles of the ovulum of Zea Mays.

Wrisberg and Muller, who adopted in part Buffon's hypothesis, state the globules, of which they suppose all organic bodies formed, to be capable of motion; and Muller distinguishes these moving organic globules from real Animalcules, with which, he adds, they have been confounded by some very respectable observers.

In 1814, Dr James Drummond, of Belfast, published in the 7th volume of the *Transactions of the Royal Society of Edinburgh*, a valuable paper, entitled "*On certain appearances observed in the Dissection of the Eyes of Fishes.*"

In this Essay, which I regret I was entirely unacquainted with when I printed the account of my observations, the author gives an account of the very remarkable motions of the spicula which form the silvery part of the choroid coat of the eyes of fishes.

These spicula were examined with a simple microscope, and as opaque objects, a strong light being thrown upon the drop of water in which they were suspended. The appearances are minutely described, and very ingenious reasoning employed to show that, to account for the motions, the least improbable conjecture is to suppose the spicula animated.

As these bodies were seen by reflected and not by transmitted light, a very correct idea of their actual motions could hardly be obtained; and with the low magnifying powers necessarily employed with the instrument and in the manner described, the more minute nearly spherical particles or active Molecules which, when higher powers were used, I have always found in abundance along with the spicula, entirely escaped observation.

Dr Drummond's researches were strictly limited to the spicula of the eyes and scales of fishes; and as he does not appear to have suspected that particles having analogous motions might exist in other organized bodies, and far less in inorganic matter, I consider myself anticipated by this acute observer only to the same extent as by Gleichen, and in a much less degree than by Muller, whose statements have been already alluded to.

All the observers now mentioned have confined themselves to the examination of the particles of organic bodies. In 1819, however, Mr Bywater, of Liverpool, published an account of *Microscopical Observations*, in which it is stated that not only organic tissues, but also inorganic substances, consist of what he terms animated or irritable particles.

A second edition of this Essay appeared in 1828, probably altered in some points, but it may be supposed agreeing essentially in its statements with the edition of 1819, which I have never seen, and of the existence of which I was ignorant when I published my pamphlet.

From the edition of 1828, which I have but lately met with, it appears that Mr Bywater employed a compound microscope of the construction called Culpepper's, that the object was examined in a bright sunshine, and the light from the mirror thrown so obliquely on the stage as to give a blue colour to the infusion.

The first experiment I here subjoin in his own words.

"A small portion of flour must be placed on a slip of glass, and mixed with a drop of water, then instantly applied to the microscope; and if stirred and viewed by a bright sun, as already described, it will appear evidently filled with innumerable small linear bodies, writhing and twisting about with extreme activity."

Similar bodies, and equally in motion, were obtained from animal and vegetable tissues, from vegetable mould, from sandstone after being made red hot, from coal, ashes, and other inorganic bodies.

I believe that in thus stating the manner in which Mr Bywater's experiments were conducted, I have enabled microscopical observers to judge of the exent and kind of optical illusion to which he was liable, and of which he does not seem to have been aware. I have only to add, that it is not here a question of priority; for if his observations are to be depended on, mine must be entirely set aside.

July 28, 1829.

ART. XVI.—*Account of the extraordinary talent for calculation of Vincenzio Zuccaro, a child seven years old.*

SOME months since a child seven years old, named Vincenzio Zuccaro, excited at Palermo the attention of the public, by his remarkable talent for arithmetical calculation. This child, who has not received any kind of instruction, solves the most complicated problems in arithmetic with surprising facility. He seems to be endowed by nature with a sort of instinct, which makes him discover, as by intuition, the different relations of numbers.

The reports which were circulated on that point were very little believed. We resolved to make the child submit to a public examination, in order to exhibit his talent to perfection, in order that he and his family, who are very poor, may reap some advantage from it. This examination took place on the 30th of January 1829, in the hall of the Academy of *Good Taste*, at Palermo. More than 400 persons of the higher ranks were present at it, and two professors of mathematics were specially charged to interrogate the child, and to write down his answers and solutions.

A great number of questions were proposed, some of which were difficult and complicated. The child answered all of them with a readiness and confidence which excited general admiration among the audience.

We shall here give an account of one of these questions, not as one of the most difficult, but because a circumstance which took place in the time of its solution by the child corroborates what we have said above, that the child discovers, as by instinct, the mutual relations of numbers, and that with such a clearness to himself, that he supposed every body else has the same rapidity of intuitive conception. The following is the problem :—

" A steam boat departed from Naples to Palermo at midday, and sailed ten miles an hour. At the same time a ship departed from Palermo to Naples, and sailed seven miles an hour. Supposing the distance from Naples to Palermo 180 miles, we ask how many miles each of the ships will have sailed when they meet, and at what hour the meeting will take place?" The child, after having considered for a few seconds, replied, *the steam boat will have sailed* 105 *and* $\frac{15}{17}$*ths of a mile, and the other* 74 *and* $\frac{2}{17}$*ths.* Upon observing that he had forgotten to mention when the two ships met, he answered without hesitation, *that is self-evident, the meeting would take place* 10 *hours and* $\frac{10}{17}$*ths after their departure.* This second answer being in some measure comprehended in the former, the child did not think it necessary to state it, thinking that every body understood it as well as he.

In another question proposed to him, the child showed how confident he was in the exactness of the solutions given by him.

Problem. "In order to make 13 soldiers' uniforms, it requires 11 ells of cloth. How many ells will it require to make 245 uniforms?" In an instant the child replied, *it will require* 207 *ells,* 2 *palms and* $\frac{6}{13}$*ths.* One of the professors having found by calculation that it would require 207 and $\frac{4}{13}$th of an ell, the child, after having reflected again, insisted on the correctness of his solution. The professor having then compared the two fractions, he found them perfectly equal.

This trial, and many others, having proved the existence of an extraordinary talent in this child, it appears to be a matter of great interest to discover by what method he arrived at the exact results so quickly. An astronomer of Palermo, Signor Nicolas Cacciatore, proposed in consequence different questions to the child; and after each answer was obtained, he asked him by what means he obtained the solution? We shall give here some examples of it, such as have been published by M. Cacciatore in the journals of Palermo.

Question. What is the square of 429? *Answer.* 184041. *Question.* How have you calculated it? *Answer.* 400 by 400 makes 160000; 29 by 29 makes 841, and that makes altogether 160841; 29 by 400 makes 11600, which doubled makes 23200; and this last number added to the first makes 184041.

It is obvious that he divides the given number under the form of a binomial, 400 + 29 $(a + b,)$ and that he finds the square by the algebraical method.

$$a^2 + 2\,a\,b + b^2 = 160000 + 23200 + 841.$$

Question. What is the square of 123? *Answer.* 15129.

Question. How have you done it? *Answer.* 123 by 100 makes 12300; 123 by 20 makes 2460; 123 by 3 makes 369. These numbers added together make 15129.

This is the known method of decomposing the number according to the value of the figures. The algebraic form would be, $a\,(m+u+p) = 123\,(100+20+3.)$ *Question.* In three successive attacks, there perished at first a fourth, then a fifth, then a sixth of the assailants, who were reduced to 138 men. We ask what was their number at the moment of the attack? *Answer.* There were 360. *Question.* How have you found this number? *Answer.* If there had been 60, there would have remained 23; but 23 is the sixth part of 138, consequently the number of the assailants ought to be 6 times 60, or 360. *Question.* But how have you supposed at first the number 60, and not 50 or 70. *Answer.* Because 50 and 70 are neither divisible by 4 nor by 6.

We find here the hypothetical method, and see that the child followed the usual rule to avoid the fractions.

Signor Cacciatore closed the examination by the following reflections: This child, scarcely seven years old, without instruction, without acquired methods, discovers with perfect exactness the relations of numbers, and creates at the same time, for each question, the method of calculation which brings him best to the solution. Sometimes, however, he takes the longest way in the calculation; but then it appears still more astonishing, by the incredible rapidity with which he gets over it, as by the confidence which he preserves in the labyrinth of figures, never mistaking nor forgetting any of the numbers that he must form, retain, decompose, and combine, and arriving always at the exact solution. A talent so extraordinary deserves surely to be developed and encouraged. The Government is about to interest itself in the fate of this child, and to grant the necessary means in order to give him a complete education suitable to his surprising powers.—*Antologia di Firenze Av.* 1829.

Art. XVII.—*A description of a Microscopic Doublet.* By William Hyde Wollaston, M. D. F. R. S. &c. *

In the illumination of microscopic objects, whatever light is collected and brought to the eye, beyond that which is fully commanded by the object-glasses, tends rather to impede than to assist distinct vision.

My endeavour has been, to collect as much of the admitted light as can be done by simple means, to a focus in the same plane as the object to be examined. For this purpose I have used with success a plane mirror to direct the light, and a plano-convex lens to collect it; the plane side of the lens being towards the object to be illuminated.

With respect to the apparatus for magnifying, notwithstanding the great improvements lately made in the construction of microscopes, by the introduction of achromatic object-glasses, and the manifest superiority they possess over any single microscope, in the greater extent of field they present to view at once, whereby they are admirably adapted to make an entertaining exhibition of known objects, hardly any one of the compound microscopes which I have yet seen, is capable of exhibiting minute bodies with that extreme distinctnesss which is to be attained by more simple means, and which is absolutely necessary for an original examination of unknown objects.

My experience has led me to prefer a lens of a plano-convex form, even when made of glass; but the sapphire lens of this form, recently introduced into use by Mr Pritchard, has a decided superiority over every single lens hitherto employed.

The cost, however, of such a lens in comparison with glass, as well as the readiness with which any number and variety of the latter kind can be procured, led me to consider what simple combinations of them might perhaps equal the sapphire lens in performance, without great cost, or difficulty of construction; and though both Mr Herschel and Professor Airy have recently applied their superior talents to the analytical investigation of this subject, it seemed not impossible that the

* From *Phil. Trans.* 1829, p. 9.

more humble efforts of a mere experimentalist, might be rewarded by some useful results.

The consideration of that form of eye-piece for astronomical telescopes called Huygenian, suggested the probability that a similar combination should have a similar advantage, of correcting both chromatic and spherical aberration, if employed in an opposite direction as a microscope.

The construction which I found convenient in my trials, may be not unaptly compared to two thimbles fitted one within the other by screwing, and each perforated at the extremity. By this construction, two suitable plano-convex lenses fixed in these perforations, may, because of their plane surfaces, have their axes easily placed in the same line; and their distance from each other may be so varied, by screwing, as to produce the best effect of which they are susceptible.

As far as my trials have hitherto gone, I am led to consider the proportion of 3 to 1 as nearly the best for the relation of the foci of these lenses; and their joint performance to be the most perfect, when the distance between their plane surfaces is about $1\frac{4}{10}$ of the shorter focus. But as all the lenses I possess are not similar segments of spheres, or of the same relative thickness, I could not expect exact uniformity in the results.

The following is a description of the apparatus which I have employed.

T, U, B, E, (Plate III. Fig. 7,) represents a tube about six inches long, and of such a diameter as to preclude any reflection of false light from its sides; and the better to insure this, the inside of the tube should be blackened. At the top of the tube, or within it, at a small distance from the top, is placed either a plano-convex lens E T, or one properly crossed, so as to have the least aberration, about three-quarters of an inch focus, having its plane side next the object to be viewed; and at the bottom is a circular perforation A, of about three-tenths of an inch diameter, for limiting the light reflected from the plane mirror R, and which is to be brought to a focus at *a*, giving a neat image of the perforation A at the distance of about eight-tenths of an inch from the lens E T, and in the same plane as the object which is to be examined. The length of the tube and the distance of the convex lens from the perforation may be somewhat varied. The length here given, six inches, being that which it was thought would be most convenient for the height of the eye above the table. The diameter of the image of the perforation A, need not, excepting with lower powers than are here meant to be considered, exceed one-twentieth of an inch.

The intensity of illumination will depend upon the diameter of the illuminating lens, and the proportion of the image to the perforation, and may be regulated according to the wish of the observer.

The compound magnifier M, consists, as before-mentioned, of two plano-convex lenses; the proportion of the foci of these lenses being about as 3 to 1. They are fixed in their cells, having their plane sides next to the object to be viewed, their plane surfaces being distant from each other about $1\frac{4}{10}$ or $1\frac{1}{2}$ of the length of the shorter focus. This distance should be varied by trial, until the utmost possible degree of distinctness has been attained, not only in the centre, but throughout the whole field of view.

In order to determine the distance between the plane surfaces of the lenses, I have used the following contrivance. A wire is bent so as to form a spring, to the ends of which two small pieces of plate glass are attached. Between the surfaces of the pieces of glass is placed the interior cell, or that which carries the lens of the longer focus; and the distance between the exterior surfaces of the pieces of glass is to be measured with a pair of callipers: the cell is then to be screwed into its place, and the compound cell subjected to the same operation; when the increase of distance between the exterior surfaces of the pieces of glass will evidently be equal to the distance between the plane surfaces of the lenses.

The exterior cell of the compound magnifier should be formed with a flanch, so that it may rest upon the piece that receives it. This is a far more convenient method than screwing, and the magnifiers can be more readily changed.

The lens E T, or the perforation A, should have an adjustment by which the distance between them may be varied, and the image of the perforation be thus brought into the same plane as the object to be examined. This may perhaps be most conveniently done by two tubes screwing one into the other.

A stage for carrying the object, furnished with the requisite means for lateral adjustments, is fixed at *a*, between the magnifier and the lens E, T. The adjustment for distinct vision is applied to the piece carrying the compound magnifier.

For the perfect performance of this microscope, it is necessary that the axes of the lenses and the centre of the perforation A, should be in the same right line. This may be known by the image of the perforation being illuminated throughout its whole extent, and having its whole circumference equally well defined. For illumination at night, a common bull's-eye lanthorn may be used with great advantage.

With this microscopic doublet I have seen the finest striæ and serratures upon the scales of the Lepisma and Podura, and the scales upon a gnat's wing, with a degree of delicate perspicuity which I have in vain sought in any other microscope with which I am acquainted.

Before I conclude, I would point out one great advantage that has confirmed me in the preference I have given to the use of a plano-convex lens, properly employed; that is, having its plane side next to the object: namely, that if such a lens should touch a fluid under examination, the view is not only not impaired, but even improved by the contact of the two media; but if a double convex lens be used, and it should accidentally touch the fluid, which not unfrequently happens when the lens is of short focus, there is an end of the examination, until the lens has been taken out, wiped, and replaced.

London, *October* 28, 1828.

Appendix.

The instrument which has been described will of course admit of many varieties of form; I shall, however, add a description of that which has appeared to me to be convenient, and which is represented in Plate III. Fig. 7. A tube of sufficient length and diameter forms the body of the instrument; one end of the tube is closed by a piece having a screw S, by means of which it may be fixed in the top of the box intended to contain the instrument, which thus forms a support. A portion of the tube above this piece is cut away, as marked by the dotted line, for the purpose of admitting light to the small mirror which is attached to an horizontal axis passing through the diameter of the tube. The inclination of this mirror may be varied by means of a milled head fixed to the axis on the outside of the tube; the other adjustment at right angles being made by turning the box of the microscope.

Into the tube above the opening a conical piece is soldered, into which is screwed a small cylindrical tube carrying the perforation before described. The plano-convex lens is fixed in a spring tube, which slides into that which forms the body of the microscope. The position, consequently, of the lens may be varied so as to bring the image of the perforation into the same plane with the object to be viewed. A piece of plate glass about two inches square, or less if it be thought more convenient, is attached to the top of the tube, and serves to support a stage having lateral adjustments at right angles to each other. The piece into which the magnifiers fit, may be moved by a rack and pinion, and great care must be taken to arrange this adjustment, so that the magnifier may move precisely in the prolongation of the axis of the tube. The tube is divided into two pieces, of equal lengths, which screw into each other, and which when taken asunder will allow of the whole instrument being packed in a box about four inches square.

Supposing the plano-convex lens to be placed at its proper distance from the stage, the image of the perforation may be readily brought into the same plane with the object, by fixing temporarily a small wire across the perforation with a bit of wax, viewing any object placed upon a piece of glass upon the stage of the microscope, and varying the distance of the perforation from the lens by screwing its tube until the image of the wire is seen distinctly at the same time with the object upon the piece of glass.

ART. XVIII.—*An account of the preliminary experiments and ultimate construction of a Refracting Telescope of 7.8 inches aperture, with a fluid concave lens.** By PETER BARLOW, Esq. F. R. S. &c.

THE instrument I intend more particularly to describe in this paper has a clear aperture of 7.8 inches, exceeding, I think, by about an inch the largest refracting telescope in this country. Its tube is eleven feet, which, together with the eye-piece, makes the whole length twelve feet: but its effective focus is, on the principle explained in my former paper,† eighteen feet. It carries a power of 700 on the closest double stars in South's and Herschel's catalogue; and the stars are with that power round and defined, although the field is not then so bright as I could desire.

The telescope is mounted on a revolving stand, which works with considerable accuracy as an azimuth and altitude instrument. To give steadiness to the stand it has been made substantial and heavy, its weight by estimation being 400 pounds, and that of the telescope 130 pounds; yet its motions are so smooth, and the power so arranged, that it may be managed by one person with the greatest ease, the star being followed by a slight touch, scarcely exceeding that required for the keys of a piano-forte.

In my former paper (*Phil. Trans.* 1828: Art. VII.) I have endeavoured to show the effect which opening the lenses to different distances produces on the secondary spectrum; my first object, therefore, in these experiments, was to ascertain by actual observation the best position of the lenses for the diminution of this defect.

In order the better to classify my experiments on this head, it will be best to refer to the original formula for the destruction of colour, given in my paper in the *Phil. Trans.* 1827: Art. XV. in which I have shown, that with open lenses we

* Abridged from *Phil. Trans.* 1829, p. 32. See this *Journal*, No. xiv. p. 335; No. xv. p. 93; and No. xviii. p. 220.

† *Phil. Trans*, 1828: Art. vii.

have, when the colour vanishes, $\frac{(f-d)^2}{ff'} = \delta$.

Where f = focal length plate lens
f' = focal length fluid lens
δ = dispersive ratio
d = distance of the lenses

Or calling $f - d = nf$ = remaining focus of plate beyond the fluid, this becomes $\frac{nf^2}{f'} = \delta$ (1)

or $f' = \frac{n^2 f}{\delta}$ (2)

If now we call f'' the resulting focus from this combination, reckoning from the fluid, we have by common principles $\frac{1}{nf} - \frac{\delta}{n^2 f} = \frac{1}{f''}$

Whence $f'' = \frac{n^2 f}{n - \delta}$ = resulting focus (3)

Consequently $f''' = \frac{nf}{n - \delta}$ = equivalent focus (4)

$l = \frac{(n - 1 - n\delta)}{n - \delta} f$ = whole length (5)

From which equations all the relations between these six quantities, viz. f, f', f'', f''', n, and δ are readily determined; where it may be observed that f' is the focal length of a telescope on the usual construction to which this telescope is equivalent, and l the whole length of the tube.

If we consider l, n, and δ as given quantities, we have

$f = \frac{(n - \delta)\, l}{n - 1 - n\delta}$ = plate focus (6)

from which f', f'', and f''' may be determined.

It is obvious from this last equation, since n and l may be assumed at pleasure, (at least within all practicable limits,) that this form of telescope will admit of great variety of proportions between the different quantities, and that some classes of these have a practical advantage over others may be reasonably expected. From the experiments I have made, it appears to me that the secondary spectrum is reduced as the lenses are opened, or as n decreases, but that the general field is enlarged and improved by increasing the value of n.

I, however, directed my attention principally to the destruction of the secondary spectrum; and with this view I ordered two $4\frac{1}{2}$ inch tubes, five feet long, to be fitted up to receive in succession lenses of different focal powers, depending principally upon the value given to n, which I assumed as follows: viz. $n = .60$, $n = .55$, $n = .50$, $n = .45$, $n = .40$, the length in each case being sixty inches. Resting on these numbers, the following values were determined, the plate glass having an index .515, the fluid .634, and the dispersive ratio .308.

Tabular value of the different quantities.

$n = .60$	$f = 39.72$	$f' = 46.42$,	$f'' = 48.97$	$f''' = 81.6$
$n = .55$	$f = 35.53$	$f' = 34.67$,	$f'' = 44.11$	$f''' = 80.2$
$n = .50$	$f = 33.30$	$f' = 27.02$,	$f'' = 43.35$	$f''' = 86.7$
$n = .45$	$f = 30.30$	$f' = 19.91$,	$f'' = 43.20$	$f''' = 96.0$
$n = .40$	$f = 25.62$	$f' = 13.30$,	$f'' = 44.56$	$f''' = 111.4$

I soon found, however, that it was impossible to get all the lenses of equally good material and figure; and as, in consequence, one defect might be mistaken for another, I altered my plan, and availed myself of the two telescopes I had constructed before, in one of which $n = .50$, and in the other $n = .54$. These two I had fitted with other lenses carefully made, making in one the value of $n = .60$, and in the other $n = .40$. I had also a new one made with the value of $n = .47$; and after a careful and patient examination of all these five, I determined, and I was supported in that determination by others, that the best effect was produced, at least as regarded the object I had in view, when the distance of the lenses was about one-half the focal length of the plate lens, and with these proportions, therefore, I determined to construct my 8-inch telescope.

Construction of the Telescope.

Having, as above stated, decided that the distance of the lenses ought to be about half, or a little more than half, the focal length of the plate lens, I determined upon a focal length of 78 inches for my plate lens, and 59.8 inches for that of my fluid; which, at the distance of 40 inches, would produce a focal length of 104 inches, a total length of 12 feet, and an equivalent focus of 18 feet. For the curves of the parallel meniscus checks for containing the fluid, I proposed — 30 inches, and + 144 inches, the latter towards the eye; and then computing the proper curves for the plate by the formula given in my paper, *Phil. Trans.* 1827: Art. XV. I found the proper curves to be 56.4 and 144; and to these curves Messrs W. and T. Gilbert worked the several glasses and the circular ring. Mr Donkin undertook to draw the tubes, which I was desirous of having 8 inches in the interior diameter, but his nearest treblet was only 7.8 inches, to which size therefore I was confined. The tube was drawn in three pieces, each 3 feet 8 inches, making in all 11 feet; and to this the pipe for the eye-piece being attached, gave the full length 12 feet: two of the above pieces of 7.8-inch tube are strongly and accurately jointed by a lining piece, and the other part is made to screw on for more conveniently getting in and adjusting the fluid lens which is near this joint, and is inclosed in a cell which screws on to an interior tube 5 inches in diameter, and 3 feet 6 inches long, sliding in two collars properly turned for the purpose, having a notch in each to receive a feather attached externally to the tube to preserve a parallel motion.

The other end of this tube of course reaches to within about 4 feet of the eye and of the large tube, and to the former is fixed a brass nut properly fitted to receive a screw on the end of a brass rod $4\frac{1}{2}$ feet in length; this rod works in a coupling box or collar, fixed on the inside of the large tube about 1 foot 9 inches from the end, and the end of the rod passes through the front end of the large tube, where it is cut square to receive a milled head, or a universal joint key, by means of which the tube carrying the cell may be moved backwards or forwards; and the adjustment is thus made for colour in the first instance, and afterwards the focus is obtained by the usual rack motion.

The difficulty of centering two lenses at so great a distance from each other is considerable, if not properly provided for In this instance the front lens is placed in a thin detached cell, and confined by a counter cell. It is then placed with its first cell in another, which screws and unscrews at the object end of the telescope as usual; except that the last cell is sufficiently large to admit of adjusting the interior one carrying the lens by means of two pair of opposite pushing screws. These provisions being made, the telescope is placed opposite to a proper object, the centering is produced by trial, by means of these screws; and when every thing is right, the cell is made fast by four other screws to prevent any trifling blow, or other slight accident, putting the glass again out of adjustment. In this state the telescope may be said to be completed. It has of course to be furnished with a finder, proper eye-pieces, an apparatus for illuminating the field, &c. as in the usual cases.

With respect to inclosing the fluid, the following, after various trials, appears to me to be quite effectual. After the best position has been determined practically for the checks forming the fluid lens, these with the ring between them ground and polished accurately to the same curves are applied together, and taken into an artificial high temperature, exceeding the greatest at which the telescope is ever expected to be used. After remaining here with the fluid some time, the space between the glasses is completely filled, immediately closed, cooled down by evaporation, and removed into a lower temperature: by this means a sudden condensation takes place, an external pressure is brought on the cheeks, and a bubble formed inside, which is of course filled with the vapour of the fluid; the excess of the atmospheric pressure beyond that of the vapour being afterwards always acting externally to preserve contact; the extreme edges are then sealed by the serum of human blood, or, which I believe to be equally efficacious, by strong fish glue and some thin pliable metal surface: by this process I have every reason to believe the lens becomes as durable as any lens of solid glass.

At all events, I have the satisfaction of stating, that my first 3-inch telescope has now been completed more than fifteen months, and that no change whatever has taken place in its performance, nor the least perceptible alteration either in the quantity or quality of the fluid. I must think, therefore, that the advantages to be gained by this means of supplying the flint glass are such as to entitle the experiments to an impartial examination; and I cannot doubt, if the prejudice against the use of fluids could be removed, that well directed practice would soon lead to the construction of the most perfect and powerful instruments on this principle at a comparatively small expence. I am, for instance, convinced, judging from what has been paid for large object glasses, that my telescope, telescope stand, and the building for observation, with every other requisite convenience, have been constructed for a less sum than would be demanded for the object glass only, if one could be produced of the same diameter of plate and flint glass; and this surely is a consideration which ought to have some weight, and encourage a perseverance in the principle of construction.

The telescope, and the particulars relative to it, being thus described, it only remains for me to state the tests to which I have subjected it, and its performance in those cases.

The first observations of this kind are commonly on Polaris. The small star here is of course brilliant and distinct. It is seen best with a power of 120, but is visible with a power of 700.

The small star in Aldebaran is very distinct with a power of 120.

The small star in α Lyræ is distinctly visible with the same power.

The small star called by Mr Herschel Debilissima, between 4 ε and 5 Lyræ,—whose existence, he says, could not even be suspected in either the 5 or 7-feet equatorial, and invisible also with the 7 and 10-feet reflectors of 6 and 9 inches aperture, but seen double with the 20-feet reflector,—is seen very satisfactorily double with this telescope.

η Persei, marked as double in South and Herschel's catalogue at the distance of 28″, with another small star at the distance of 3′ 57″, both *n p*, is seen distinctly sixfold, four of the small stars being within a considerably less distance than the remote one of η marked in the catalogue. And, rejecting this remote star, the principal, and the other four small stars, form a miniature representation of Jupiter and his satellites, three of them being nearly in a line on one side, and the other on the opposite. There are also other small stars within the same distance, but the most remarkable are those arranged in a line as above stated.

A number of other small stars, which are spoken of as difficult to observe from their minuteness, are seen more or less distinctly with this instrument.

Amongst the closer and larger stars I have tried the telescope upon those commonly selected as tests, viz.

Castor, which is distinctly double with 120, and well opened, and stars perfectly round, with 360 and 700.

γ Leonis and α Piscium are seen, with the same powers, equally round and distinct.

In ε Bootis the small star is well separated from the larger, and its blue colour well marked with a power of 360.

η Coronæ Borealis is seen double with a power of 360 and 700. 52 Orionis, ζ Orionis, and others of the same class, are also well defined with the same powers.

Still, however, it must be admitted that the telescope is not so competent to the opening of the close stars, as it is powerful in bringing to light the more minute luminous points.

Of the planets, I have only had an opportunity of trying the telescope on Venus, Saturn, and Mars; and the latter is too low to furnish a good test. Venus is beautifully white and well defined with a power of 120, but shows some colour with 360. Saturn, with the 120 power, is a very brilliant object, the double ring and belts being well and satisfactorily defined, and with the 360 power it is still very fine. The moon also is remarkably beautiful, the edges and the shadows being well marked, while the quantity of light is such as to bring to view every minute distinction of figure and shade.

Art. XIX.—*On the Mode of Generation in the Mya Pictorum—in the Helix palustris—and in the Mulus gobio; and Notice on the Circulation of the Fœtus in Ruminating Animals.* By M. Prevost of Geneva. *

M. Prevost, along with M. Dumas, has been engaged for

* *De la Génération chez les Moules des Peintres.* Read at the Physical and Natural History Society of Geneva, March 17, 1825.—*De la Génération chez le Lymnée.* Read at the same Society in 1826.—*De la Génération chez le Sechot.* Read at the Physical Society of Geneva, 1825.—*Note sur la Circulation du Fœtus chez les Ruminants.* Geneva, 1828.

some time in investigating the phenomena of generation in various classes of animals; and the papers noted above, of which he has had the goodness to transmit copies to us, were read at various periods before the Physical and Natural History Society of Geneva. The object of these investigations is to support the opinion, that, among vertebrated animals, the developement of the embryo does not take place till after contact between the *cicatricula* of the female ovaries and the spermatic animalcules of the male, which they conceive to be the chief agents in effecting fecundation. The first memoir, on the generation of the *Myæ Pictorum*, shows that among molluscous animals the same law is followed. "If, towards the spring," says M. Prevost, "we examine the organs of generation in some individuals of this species, we are struck at the first glance with the different products which they emit. While we find in some individuals a true ovary, and ova in abundance, in others the analogous organs, and similarly placed, contain nothing but a thick liquid of a milky colour, which under the microscope appears to be crowded with animalcules in motion. These marked differences are neither the result of chance, nor of a subsequent change in the condition of the ovary. The *Myæ* in which ova are found present no trace of the thick and milky fluid; and, on the contrary, those which possess this liquid produce no ova." M. Prevost, after describing the state of the parts in these different individuals at different periods, by the aid of the microscope, comes to the conclusion, 1. That the white liquid in the organs of generation in one class of individuals has so much analogy with the spermatic apparatus of vertebrated animals that it may be considered as performing the same functions; and, 2. That since the seminal fluid and ova are never found in the same individual, it may be concluded, though contrary to the generally received opinion, that this genus of animals have the sexes in separate individuals. M. Prevost confirms this opinion by direct experiment; and the memoir is illustrated by an engraving of the different appearances.

The next memoir is on the generation of the *Helix palustris*; and here, though the animals are hermaphrodite, or possess both male and female organs of generation, M. Prevost shows the disposition of these to be such, that the animal cannot fecundate itself, and even that mutual impregnation between two individuals is impossible. To complete the purpose of nature, the animals require to arrange themselves in lines or chains in a certain position, so that the sexual organs may be in contact, one with the male organ in connection with the oviduct of the nearest adjacent animal on one side, and its own oviduct in a position to be impregnated by a third individual. In the ditches where they abound may often be seen long chains of these animals, in which, with the exception of the two at the extremities, all are alternately fecundated or fecundating. This memoir has also an illustrative engraving.

The third memoir, on the generation of the *Mulus gobio*, contains the result of the investigations of MM. Dumas and Prevost on this subject, which have led them to conclude, that the principal phenomena of generation among fishes is identically the same with what takes place in the other vertebrated animals. Fecundation is accomplished as among the Batrachian reptiles; that is, at the moment when the ova leave the oviduct the male discharges the spermatic fluid into the water. The ova which fall into this medium absorb a portion, and the current which results from this absorption carries the animalcules to the surface of the ova. "I verified this fact" (says M. Prevost) "by taking an ovum from the oviduct and placing it in spermatized water. If at this moment it be examined with the microscope, the animalcules are seen carried to the periphery of the ovum by a strong current, and the fœtus rarely fails of developing itself. It is necessary to the success of the experiment, however, that the impregnated ovum be placed immediately in running water. The fœtus is seen, as among birds, in the centre of the *cicatricula*, under the figure of a spot inflated at one of its extremities, and slightly narrowed at the other, which is the posterior one. The contents of the ova in these animals is analogous in chemical composition to the yolk of the eggs of the common fowl and the *corpora lutea* in the ovary of the cow. As in them there is found much albumen, and a thick yellow oil soluble in ether; and they differ chiefly in containing no gelatine, but some traces of mucus. The process of the growth of the fœtus of the *Mulus gobio* is exhibited in a plate containing magnified figures.

The fourth memoir is a notice on the circulation of the fœtus in ruminating animals. From the difference of diameter between the globules of blood in the fœtus and those of the blood of the mother, M. Prevost infers, that among the *Mammalia* there exists no direct communication between the sanguiferous systems of the embryo and the mother. His inference was confirmed by the following observations. The uterus of a sheep newly killed, and in which gestation had not gone far, was brought to him. He opened it in warm water, and withdrew the fœtus with its membranes, which was the more easily done, that at this period the chorion presented no adherence to the uterus. He perceived that the heart of the fœtus was still beating, and, profiting by the occasion to examine the circulation, he placed the ovum with precaution upon a heated square of glass, and exposed it to the rays of a summer sun. The heat and the contact of the air rapidly quickening the motions of the heart, M. Prevost with a microscope followed attentively the motion of the blood in the vessels. These he found to branch out into a very minute series on certain points of the chorion, destined to form at a future period the fœtal portion of the cotyledon or placenta of ruminating animals. After being thus subdivided, the vessels were reunited by innumerable anastomoses, and formed finally one or two veins, which carried to the fœtus the blood which had circulated in the vessels first noticed. This fœtal portion of the cotyledon in the rudimentary state possessed none of those prolongations or papillæ which afterwards are found to be connected with corresponding depressions in the maternal placenta. The transparency of the objects permitted him distinctly to perceive that the minute radiations were prolonged without interruption from the intermediate tissue into the minute returning veins. No hemorrhage in any part took place from the separation of the ovum from the uterus. If the cotyledon was pressed, some drops of a white liquid exuded from the small sieve-like cavities. This fluid does not naturally appear till a more advanced period of gestation; it is in great quantity; and it is designed, in M. Prevost's opinion, to nourish the fœtus. It is secreted by the surface of the cotyledon, and taken up by the vessels of the membrane of the chorion, which is prolonged in the form of papillæ into the cavities of the cotyledon, as mentioned above.

The necessary consequence of what he has observed leads M. Prevost to conclude, that the ovum is an isolated body in the uterus, and that the uterus secretes a substance which is absorbed by the vessels of the fœtus, and contributes to its growth: And he goes on to show, by analogous facts, the near resemblance of the manner in which the embryo is developed among *Mammalia* and birds. The difference in this respect between these two classes of animals consists in this, 1. That the ovary in the Mammalia does not contribute in any way to nourish the embryo. 2. That the uterus solely performs this function, and accomplishes it not at once, but by degrees, through the medium of the maternal placenta. "Adopting this view, we may regard," says M. Prevost, "the *corpora lutea* of the ovary in the Mammalia as analogous to the yolk in the eggs of birds, *first*, because the *corpus luteum* is secreted by the same series of vessels which secrete the yolk in oviparous animals; and, *secondly*, because the colouring matter which tinges the *corpus luteum* in the cow, comports itself with re-agents precisely as the colouring matter of the yolk of an egg."

Art. XX.—*Account of a new Cistern for Barometers.* By Mr John Adie. Communicated by the Author.

Dear Sir,

I take the liberty of sending you the description of a new construction of cistern for a barometer, which I conceive has considerable advantages over those generally in use; and it will give me much pleasure should you think it worthy of a place in your very valuable *Journal of Science.*

The construction is shown in Plate III. Fig. 10, where C is a cylindrical cistern of cast iron, made as thin and light as possible; into which A, the barometer, and B, the siphon tubes, are screwed. The iron is coated inside and out with a strong varnish to prevent its rusting. D is a cylindrical plunger of glass moved by the screw F, and passing through the collar of leather

E, which is tightened by the screwed ring H; so that by turning the screw F we can withdraw or advance the plunger, and cause the mercury to rise and fall in the tube. When the barometer is used, it is adjusted until the surface of the mercury cuts off the light at the opening G in the siphon tube. The size of the plunger is made equal to the quantity of mercury which will descend into the cistern by the greatest fall of the barometer, by which means the cistern may be made of the exact size required. For example, if the tube be .2 of an inch in diameter, and the required range from 32 to 10 inches, that is over a space of 22 inches, the quantity of mercury contained in such a tube is = .691152 parts of a cubic inch, which quantity the plunger is made equal to.

The advantages I conceive to attend this construction of cistern are, that it is not affected by moisture and heat in warm climates. The adjustment of the surface of the mercury is more easily made, and not liable to change from any motion of the instrument during the time of observation, the mercury forming with the cistern a compact mass. From the same cause it is not so liable to be broken in carriage from being carelessly turned up. With leather cisterns, any slight motion during the time of observation may cause some *lirk* (fold) in the leather to distend itself, and thereby require the adjustments to be gone over,—a circumstance I have experienced in using an instrument of that construction.

The only other construction of cast iron cistern that has come under my notice, is that forming a cylindrical box having its upper end of wood, into which the tube is fixed. This I conceive to have many objections, from the number of corrections required; *first*, the difference of capacity between the tube and cistern; *second*, for the capillary action; and *third*, a neutral point must be determined, so that any portion of mercury being lost in carriage or otherwise, the series of observation made under such circumstances, with such an instrument, become of no value; whereas with a siphon barometer the quantity of mercury contained in the cistern is in no way connected with the results given by that instrument, and besides, the corrections for capillary action and capacity are not required. Another great objection to the iron box construction of cistern, perhaps the greatest of all, is the method of stopping the tube for carriage. This is done by screwing up a cushion against the open end of the tube, which cannot fail to convey dust, air, and moisture with it, and thus render the filling of the tube imperfect; and the mercury not filling the cistern will be oxidated from the agitation it must receive in carriage.

I think it will be generally allowed, that the more perfect any instrument can itself be made, the more will the corrections and chances of error be reduced; the results will be more easily obtained, and become much more deserving of confidence.—I have the honour to be,

very respectfully yours,

JOHN ADIE.

EDINBURGH, *4th September* 1829.

ART. XXI.—HISTORY OF MECANICAL INVENTIONS AND OF PROCESSES AND MATERIALS USED IN THE FINE AND USEFUL ARTS.

1. *Mr Bevan's Experiments on the Modulus of Torsion.*

THE resistance of bodies to twisting has never yet been carefully examined, and practical men are under great obligations to Mr Bevan for his very valuable experiments on this subject.

In order to find the deflection δ or quantity of twisting in inches and decimals, Mr Bevan has given the following formula $\frac{r^2 l w}{d^4 T} = \delta$, l, being the length of a prismatic shaft strained by a given force w in pounds avoirdupois acting at right angles to the axis of the prism, and by a leverage of given length $= r$; the side of the square shaft being $= d$, and T being the modulus of torsion in the following table, l, r, δ, and d being in inches and decimals.

TABLE of the Modulus of Torsion.

Species of Wood.	Specific gravity.	Modulus of Torsion. Pounds.	Observations.
Acacia, - -	.795	28293	Not quite dry.

r, - -	.55	16221	Cross-grained.
le, - -	.726	20397	
, - -		20300	Of my own planting.
mountain, -	.449	13933	
h, - -		21243	
h, - -		17250	
, - -	99	30000	Old, and very dry.
il wood, -	1.05	37800	Old, and very dry.
, - -		21500	Influenced by the hard surface.
r, scented -		12500	
ry, - -	.71	22800	
tnut, sweet		18360	
tnut, horse -	.615	22205	
, - -	.763	22738	
son, - -		23500	
, Christiana,	.38	11220	
r, - -	.755	22285	
- -		13500	
Scotch, -		13700	
l, - -	.83	26325	Not quite dry.
y, -		20543	
beam, -	.86	26411	Not quite dry.
rnum, -		18000	Green, or fresh cut.
e-wood,	1.01	25245	
h, -	.58	18967	
or Linden,	.675	18309	
le, - -	.735	23947	Partly cross-grained.
English, -		20000	
Hamburgh,	.693	12000	
Dantzic,	.586	16500	
from Bog	.67	14500	
, - -		18700	
, - -	.72	18115	
, St Petersburgh,		10500	Fresh.
, St Petersburgh		13000	Four or five years old.
, Memel,		15000	
, American		14750	

Species of Wood.	Specific gravity.	Modulus of Torsion. Pounds.	Observations.
Plane, - -	.59	17617	
Plum, - -	.79	23700	
Poplar, - -	.333	9473	
Satin-wood, -	1.02	30000	
Sallow, -		18600	
Sycamore, -		22300	
Teak, -		16800	Old, and partially decayed
Teak, African		27300	
Walnut, -	.572	19784	

I have observed in a great number of my experiments, that the modulus of torsion bears a near relation to the weight of the wood when dry, whatever may be the species; and that for practical purposes we may obtain the deflection (δ) from the specific gravity (s.) Thus $\frac{r^2 l w}{30000 d^4 s} = \delta$

TABLE of the Modulus of Torsion in Metals.

	Specific gravity.	Modulus of Torsion. Pounds.
Iron, English (wrought)		1810000
Iron, English (wrought)		1740000
Iron, thin hooping -		1916000
Steel, - -		1984000
Steel, - -		1648000
Steel, - -		1618000
Iron cylinder, -		1910000
Iron cylinder, -		1700000
Iron square, -		1617000
Iron square, -		1667000
Iron square, -		1951000
Mean of Iron and Steel,		1779090

Iron, cast	- -		940000
Iron, cast	- -		963000
Iron, cast	- -		952000
Mean of cast-iron,	-	7.163	951600
Bell-metal,	-	8.531	818000

On comparing these numbers with the modulus of elasticity of the same substance, I find the modulus of torsion to be $\frac{1}{16}$th of the modulus of elasticity in metallic substances.—*Phil. Trans.* 1829, p. 129.

2. *Results of Mr Rennie's experiments on the friction and abrasion of the surfaces of Solids.*

The following are the results of a series of very valuable experiments on friction made by John Rennie, Esq.

The following table shows the amount of friction (without unguents) of different substances, the insistent weight being 36lbs. and within the limits of abrasion of the softest substance.

	Parts of the whole weight.
Steel on ice, - - -	69.81
Ice on ice, - - - -	36.00
Hard wood on hard wood, - -	7.73
Brass on wrought iron, - - -	7.38
Brass on cast iron, - - -	7.11
Brass on steel, - - - -	7.20
Soft steel on soft steel, - - -	6.85
Cast iron on steel, - - -	6.62
Wrought iron on wrought iron, - -	6.26
Cast iron on cast iron, - - -	6.12
Hard brass on cast iron, - - -	6.00
Cast iron on wrought iron, - -	5.87
Brass on brass, - - -	5.70
Tin on cast iron, - - -	5.59
Tin on wrought iron, - - -	5.53
Soft steel on wrought iron, - -	5.28
Leather on iron, - - -	4.00
Tin on tin, - - - -	3.78

Granite on granite, - - -	3.30
Yellow deal on yellow deal, - - -	2.88
Sand-stone on sand-stone, - -	3.75
Woollen cloth on woollen cloth, - -	2.30

These results are collected from the different Tables, but the comparison may be made by selecting other values within the limits of abrasion for a minimum.

General Conclusions.

From what has been stated hitherto it is obvious,—

1st, That the laws which govern the retardation of bodies gliding over each other are as the nature of those bodies.

2d, That with fibrous substances, such as cloth, &c. friction is increased by surface and time, and diminished by pressure and velocity.

3d, That with harder substances, such as woods, metals, and stones, and within the limits of abrasion, the amount of friction is as the pressure directly, without regard to surface, time, or velocity.

4th, That with dissimilar substances gliding against each other, the measure of friction will be determined by the limit of abrasion of the softer substance.

5th, That friction is greatest with soft, and least with hard substances.

6th, That the diminution of friction by unguents is as the nature of the unguents, without reference to the substances moving over them.

The very soft woods, stones, and metals, approximate to the laws which govern the fibrous substances.

In comparing the present experiments with those of Coulomb, the discordances found to exist relate principally to time. The limited pressures (varying from 1 to 45lbs. per square inch) under which his experiments were made, account in some degree for the anomaly. But in many of the minor, and in the general results, they will be found to coincide.—*Phil. Trans.* 1829, p. 169.

3. *On an Indelible Ink.* By M. Henri Braconnot.

To 20 grammes of Dantzic potash dissolved in boiling water, add 10 grammes of animal matter, (M. Braconnot used pairings of hides,) properly divided, and 5 grammes of flour of sulphur. Let the whole be boiled to dryness, and then strongly heated, being stirred all the while till the materials are softened, care being taken to prevent ignition. Then, after having added by degrees a proper quantity of water, let the whole be filtered through a piece of linen. The result of this will be a dark coloured fluid, which must be kept well corked in a bottle. A single penful of this ink is sufficient to write one or two quarto pages. It possesses, besides, all the qualities which are required in an indestructible ink. It runs from the pen much better than common ink, and does not load the pen with foreign matter suspended in it. It resists also, with some exceptions, the most powerful chemical agents.—This ink, which seems to be a valuable present to the arts, is also an excellent marking ink for linen, and may be employed in dyeing dark brown colours. Abridged from the *Ann de Chim.* February 1829, p. 220.

4. *Method of detecting the Adulteration of Flour with Potatoe Flour.* By M. Henri.

The method proposed by our author is to determine the quantity of gluten in the flour to be examined. Good unadulterated flour contains about $10\frac{1}{4}$ per cent. of gluten, as the mean of 30 different kinds of the crops of 1827 and 1828; whereas in the adulterated or mixed flour the gluten amounted only to 6 or $6\frac{1}{2}$ per cent.—*Journal de Pharmacie.*

5. *Description of Mr Fowler's Patent Thermosiphon.*—

This instrument, which derives its name from θερμος, hot, and σιφων, a tube, is intended generally for heating houses and buildings, and for all horticultural purposes requiring heat. The instrument in its simplest form is shown in Plate III. Fig. 8, where A, B are two open vessels. A is placed over a fire-place, and B at any moderate distance from it, united by the connecting tube D, (which may have a stop-cock E in any part of it.) The vessels A, B are placed on a level with each other, and partly filled, as here shown, with any fluid that will not corrode the materials employed. In the present case, I will suppose water to be the fluid used. C C is a tube, bent into the form of a siphon, and suspended so that its ends may be immersed about half way in the water in the two vessels.

This siphon is furnished with the stop-cocks F and F′, near the ends, and the filling cock G on its highest part. The end in the vessel A is bent with its orifice upwards, which should still be several inches below the surface of the fluid. This is done to prevent the air bubbles, that arise from the bottom of A, when heat is applied, from going into the tube, and lodging in its upper part.

In order to prepare this apparatus for action, (water being put already in the two vessels A and B,) stop the cocks F and F′; open G; and with a funnel fill the tube with water, until it overflows: stop G, and open F and F′; the air below F and F′ will immediately rush upwards, and be replaced by water. Stop F and F′, open G, and pour in more water, until the tube be again quite full; and, in most cases, where air plugs are not necessary, the whole will be fit for action. Should any doubt, however, remain, that the air is not all excluded, the process of stopping G, unstopping F and F′, and filling, must be again and again repeated.*

When the Thermosiphon C C is full of water, stop G, open F and F′, and also the cock E in the connecting tube; apply the fire to A, and the water will almost immediately begin to circulate through C C from A, to B; and return, by the connecting tube D, to A, for a fresh supply of heat: and as the heat of the water in A increases or diminishes, so the circulation will be faster or slower.

The rationale of this process is:—first, it is evident that the water, or other fluid, is kept up in the Thermosiphon by pressure of the atmosphere on the surfaces of the fluid in the two vessels. Secondly, the connecting tube D keeps the cold fluid to a perfect level in those vessels: also, when the tube F, G, F, is filled (should its height not exceed the limit of atmospheric pressure) it forms a communication between the two

* The extreme height of G must be regulated generally by the specific gravity of the fluid, and the degree of heat required. I find, by experiment, that when G is twenty feet high, water will rise and circulate through a tube sixty feet in length, and $\frac{3}{4}$ inch diameter, and produce a temperature of from 140° to 150° in B, particularly when the best form is adopted.

vessels, and would level the water in them, if the connecting tube D were stopped. The whole, therefore, must be in perfect equilibrium; and it will remain so as long as there is an equal temperature in the two vessels.

The fluid being thus at rest, unstop E, and if heat be applied to A, the water in this vessel will expand, and the very small quantity of fluid in the end of the tube in A, just above and below the surface, becomes specifically lighter than that in the other end in B; this (almost imperceptibly at first) destroys the equilibrium; the tube commences to act as a siphon; a small quantity of warm fluid is drawn higher in the tube, and cold water descends from the other end, which causes the water in B to flow into A through the connecting tube D; this further destroys the balance; and the circulation will now go on with a sort of rapidity that could hardly be anticipated; particularly if the descending part of the Thermosiphon be kept as cold as possible. That is, it will be in proportion to the quantity of heat abstracted or given out.

As a considerable quantity of air is given out by water when it is first heated, some of this air lodges in the upper part of the Thermosiphon, where it expands, and often stops the circulation. It will be necessary, therefore, to re-fill it in the way already described, viz. to stop F and F′, unstop G, and fill the Thermosiphon with the sort of fluid already used: also, when the height of G approaches fifteen or twenty feet above the level of the water in the vessels, and the water in the boiler is at 180° or 200°, steam may collect, or be formed, in the higher part, and expand, which will, after some time, prevent the circulation. This may partially be remedied by pouring a small quantity of oil into the cock G when nearly full, so that it may cover the water in the tube with a thin film. This film will always swim on the surface of the water, and, in some degree, prevent its being converted into an elastic vapour. These unavoidable imperfections are, however, very trifling, when it is considered that the operation of filling is so much simplified by the use of the cocks F F′ and G. In fact, this operation requires only the most common attention. It is done in less time than one minute; and this might not be required even once a day when G does not exceed six or eight feet perpendicular height, and the water in A is not suffered to boil violently. I know, experimentally, that when G is about four feet high, the water circulates more than a week in a Thermosiphon 2¼ inches diameter, (erected in a green-house under my superintendence,) without the least occasion to fill it, although the water in A is often boiling. But it would be advisable never to let the temperature of common water exceed 208° or 210°. For low elevations of G, and for heights of from fifteen to twenty feet, 160° to 180° in the boiler, is as much as the machine will well bear when common water is used, unless it has caloric rapidly extracted from its upper part: this will condense the steam which may arise. The highest useful temperatures of fluids for particular elevations can only be ascertained by experience and attention.

The boiler should have a recess in its side to receive the end of the tube. This recess may project several inches, according to the size of the tube, from the body of the boiler, so as not to be immediately subject to the action of the fire. The fluid in this part will not be much agitated by ebullition in the boiler, but will ascend tranquilly into the machine, and take but few air or steam bubbles with it.

Mr Fowler then shows how this contrivance may be applied for heating fluids for *dyeing, hat-making, washing, heating a bath, heating a hot plate for copper-plate printers, making infusions of malt, heating hot-houses, green-houses, and conservatories, heating the fronts of garden and other walls, &c. &c.*

The method of applying it to heating a bath is shown in Plate III. Fig. 9, where A is an open vessel two-thirds full of water, placed on the kitchen fire. I is the ascending leg of the Thermosiphon. W is the bath, with a double casing at the back and bottom. J J is the descending leg; and G being the highest point of the Thermosiphon, it will be seen that the bath which in this case is the object to be heated, is situated between the highest point and the lowest, which is the coldest part of the descending leg of the Thermosiphon. V is one of the inner walls of the house; and, as the Thermosiphon may be of almost any shape, however tortuous, of course the arrangement may be adapted to the premises. It is only necessary to state, that the highest point of the Thermosiphon should not in any respect exceed thirty feet, as it acts in this respect on the principle of the Torricellian column; and I prefer not to exceed *twenty* feet. Care must be taken also at all times to exclude the air completely, when filling the Thermosiphon; air-plugs being placed where necessary, to permit the air to escape when filling, and to prevent a return.

6. *Mr Derbyshire's Embrocation for preventing or alleviating sea-sickness.*

The inventor of this embrocation has secured it by patent. It is made thus:

" Take of *crude opium* two ounces avoird. two drachms of *extract of henbane*, ten grains of *powdered mace*, and two ounces of *hard mottled soap*. Boil them in sixty ounces of soft water for half-an-hour, stirring well: When cold, add one quart of *spirits of wine* at sixty degrees above proof, and three drachms of *spirit of ammonia*.

Rub a desert spoonful of this embrocation well in over the lower end of the breast-bone, and under the left ribs, the latest time you can conveniently do so previous to embarkation, and again on board as soon as you have an opportunity. The application must be continued till the sickness disappears.

7. *Method of preserving Fruit without Sugar.*

You must use wide-necked bottles, such as are used for wine and porter. Have the bottles perfectly clean. The fruit should not be too ripe. Fill the bottles as full as they will hold, so as to admit the cork going in. Make the fruit lie compact; fit the corks to each bottle, slightly putting them in that they may be taken out the easier when scalded enough; this may be done in any thing which is convenient; put a coarse cloth of any kind as the bottom of the vessel, to prevent the bottles from cracking; fill the vessel with water sufficiently high for the bottles to be nearly covered in it; turn them a little to one side to expel the air that is contained in the bottom of the bottle; then light the fire; take care that the bottles do not touch the sides nor the bottom of the vessel, for fear they will burst, and increase the heat gradually, until the thermometer rises to 160 or 170°. If such an instrument cannot be procured, you must judge by the finger; the water must not be so hot as to scald. It must be kept at that sufficient degree of heat for half an hour; it should not be kept on any longer, nor a greater heat produced than above mentioned. During the time the bottles are increasing in heat, a tea-kettle of water must be ready boiled as soon as the fruit is done. As soon as the fruit is properly scalded, take the bottles out of the water one at a time, and fill them within an inch of the cork with the boiling water. Cork them down immediately, doing it gently but very tight, by pushing the cork in, for agitation will be apt to burst the bottles; lay the bottles on the side, to keep the air from escaping. You must take care to let them lie on their sides until wanted, often turning them over, once in a week, or once in a month.—*American Journal of Arts and Sciences*, vol. xv. p. 381.

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

I.—*Principles of Natural Philosophy, or a new Theory of Physics, founded on Gravitation, and applied in explaining the general properties of matter, the phenomena of Chemistry, Electricity, Galvanism, Magnetism, and Electro-magnetism.* By Thomas Exley, A. M. Associate of the Bristol Philosophical and Literary Society. Lond. 1829. Pp. 510, and 4 Plates.

The patient philosopher, who has spent a long life in exploring a small portion of the material universe, and who considers himself nobly rewarded if he has discovered a few important facts, and succeeded in referring them to some general and well-established principles, stands appalled when he first opens a book like the present. He turns to the department of science in which he has himself laboured, to witness the solution of the difficulties which have baffled him, and to obtain possession of the great secret of which he has been in quest. But how great is his disappointment! The theorist is not acquainted even with one of his facts; and in place of having any real knowledge of the subject, he finds him ignorant even of the best established and most elementary truths.

Hitherto the authors of universal theories have been mere pretenders to science, or men of ill-constituted minds, who have neither patience nor talents for calm research, and whose only object is to gain a little temporary notoriety by the boldness and extravagance of their views; but it grieves us to observe, that this passion has been seizing the minds even of able men, and that it threatens to become an epidemic in the once salubrious fields of physical inquiry.

Within the last two or three years no fewer than three works of this kind have been given to the world. The first of these, published in 1827, by our able countryman Dr Blair, is entitled, *Scientific Aphorisms, being the outline of an attempt to establish fixed principles of Science; and to explain from them the general nature of the construction and mechanism of the material system, and the dependence of that system on mind.* This work evinces great knowledge and profound thought; and may be read with advantage by those who cannot adopt the hypotheses of jaculatory atoms, and molecules with spinous processes. The next work of this kind is the *Explication Universelle* by H. Azais, with the imposing motto of *Tout expliquer, c'est tout unir.* In this work, which is one of the highest pretensions, the author does not limit himself, like our grave countryman, to the constitution merely of compound bodies. The heavens above, and the earth beneath, and all things under the earth, present no difficulties to his reasoning imagination. The movements of the planets, the puzzles of animal and vegetable life, and the conformation of the soul itself, are all explained in a Paris garden, to crowded audiences, and admiring disciples.

The third work to which we have alluded, is that of Mr Exley, which is of an intermediate character, neither ballasted by the patient thought of the Scotch philosopher, nor buoyed up by the levities of the French lecturer. Mr Exley unites mathematical acquirements with an extensive knowledge of chemistry and physics, and as he describes every phenomenon in a separate paragraph, and then adds its explanation, the reader may collect a good deal of information by reading the details of the separate phenomena, even though he overlooks the speculative views which tread upon their heels. But while we make this admission in favour of Mr Exley, we must at the same time declare, that, like all theorists, he often states *only what he thinks he can explain,* and thus presents important physical facts in a meagre and a mangled form.

If we thought it would be either instructive to our readers, or useful to Mr Exley, we should give various specimens of his descriptions of phenomena, and point out not only their imperfections, but the absolute defects of the explanations which he has given of them. We shall content ourselves with a single specimen, which relates to one of the most curious and well known facts in science, and which embraces the consideration of properties, both chemical and physical, viz. the production of the prismatic colours on steel, by raising it to different temperatures.

"Phenomenon 50. In raising the temperature of the steel through various degrees, it assumes successively the prismatic colours.

"Explanation. As the temperature is raised, the superficial atoms are more and more separated, and the enclosed ethereal atoms become more and more diffused as the heat increases; hence the surface acquires different conditions for the reflection of different sorts of ethereal atoms, and the different colours, doubtless, arise from the differences in the forces and spherules of the ethereal matter which constitutes light, together with the greater or less velocity with which it is projected; and hence, according to the state of the surface, we shall have all the different colours." P. 105.

Now, if we admit that the fact of the production of colour on the steel is correctly stated, we cannot, for our part, see in all this the slightest glimpse of an explanation. The explanation is in reality a mass of hypotheses, far more difficult to understand than the fact itself; and we cannot conceive, how a man of sound judgment could allow his pen to record such unmeaning extravagancies. But, independent of this, the fact is erroneously described; and where it is correctly stated, the author's assumptions, even if he is allowed the use of them in all their generality, have no reference to it whatever.

The colours of the steel are assumed to be occasioned by an increase of temperature. But this is not the case; for *if the steel is heated out of contact with air, no colours are produced;* so that Mr Exley's explanation is completely overturned by this fact alone.

Again, if the steel surface is examined by *homogeneous* in place of compound light, it will be found, that a state of the surface, produced at the temperature of 500°, reflects the yellow rays copiously; and a state of the surface, produced at a temperature of 650°, also reflects the yellow rays copiously; while a state of the surface, at an intermediate temperature of 570°, will reflect no yellow light at all. But, according to Mr Exley, it ought to reflect yellow at only one state of the surface. We may add another fact, equally baffling to Mr Exley's hypotheses. Signor Fusinieri has found, *that these colours are produced on all metals except platinum.*

We shall now request our readers to compare the phenomenon and explanation of it, as given by Mr Exley, with the following simple statement:—

When a polished steel surface is heated in contact with oxygen, an oxide is formed in the state of a thin pellicle upon its surface. At a temperature of 430° the colour of the pellicle is straw yellow. At 500° it is a brownish yellow. At 550° it is a dark purple. At 570° it is a deep blue. At 630° it is a pale blue, with a tinge of green. In virtue of what law of affinity the metal combines with the oxygen in the case of steel, and not in that of platinum, we cannot tell, nor can Mr Exley; but we can prove by direct experiment that the pellicle produced at different temperatures has different thicknesses, and that the phenomenon of colour is simply a case of thin plates, the colours arising from the same cause as those of the soap bubble, or (to take a better example), of a thin film of fluid laid upon a steel surface. Chemistry can alone explain the chemical part of the phenomenon; and the present theory of recurrent colours affords a complete solution of the optical part of it.

We have thus analysed one of the many hundred phenomena which occupy Mr Exley's pages, and we are confident that he will himself see the incorrectness of his own views, and acknowledge the impossibility, that any one man should be capable of unveiling the mysteries of the natural world by such summary processes as those which we have been considering. Five hundred able men might execute a work like the present, and the result would be five hundred different systems of hypotheses, each of which would be constructed to suit the facts that took the deepest hold of the author's mind; but science would gain nothing by all this display of ingenuity or of knowledge. Nature would still be found in her strongholds, with her mysteries veiled, and her treasures unlocked. We would therefore strongly recommend it to Mr Exley, to devote his time and his talents to the prosecution of some department of science where the study of facts, and the investigation of their cause, will, we doubt not, place him among those men who have advanced the genuine interests of science, and have established for themselves an imperishable monument in the temple of fame.

II. *The Natural History of several new popular, and diverting living objects for the Microscope, with the phenomena presented by them under observation, &c. Conjoined with accurate descriptions of the latest improvements in the Diamond, Sapphire, Aplanatic, and Amician Microscopes: And Instructions for managing them, &c. &c. To which is added a Tract on the newly discovered Test objects. Illustrated by highly finished coloured Engravings from Drawings of the actual living subjects.* By C. R. Goring, M. D. and Andrew Pritchard. No. II. pp. 64 with three Plates.

Having already given an account of the first Number of this valuable work, and stated our opinion of the high qualifications of Dr Goring and Mr Pritchard, for so difficult an undertaking, we shall proceed, without any farther preface, to the analysis of this new Number, which consists of the following Chapters and Sections:—

Chap. IV.—Whether there is a best possible way of constructing the stand or mounting, &c. of Microscopes, (the specific purpose or purposes to which they are to be applied being first determined.)

Chap. V.—Description of an operative Aplanatic Engiscope, (Microscope.)

Chap. VI.—Manner of observing with, and managing the operative Aplanatic Engiscope.

1st, Manner of mounting, for viewing inanimate transparent objects, by pure intercepted day-light.

2d, Manner of mounting for viewing transparent objects by artificial light.

3d, Mode of mounting for viewing diaphanous bodies by reflected daylight, either in a horizontal or vertical position.

4th, Way of mounting for transparent living bodies.

5th, Method of mounting the diamond or sapphire microscopes for transparent objects.

6th, Method of observing opaque objects by day-light, plain or condensed.

7th, Method of observing opaque objects by artificial light, either plain, condensed, or reverberated by silver cups.

8th, Mounting for dissections, &c.

9th, Mounting the diamond and sapphire microscopes for opaque objects.

10th, The Amician Catadioptric Engiscope.

Chap. VII.—On the larva of a species of British Hydrophilus.

The various subjects which occupy these chapters are treated with great perspicuity, and with much practical knowledge of the microscope; and the descriptions are illustrated with three plates, one of which is finely coloured.

It would be impossible to convey to the reader any idea of this part of the work; but we shall make an extract from the description of the hydrophilus, which forms the subject of the Seventh Chapter.

"In examining the peculiarities of the structure and habits of this larva, the faculty which most attracts our attention is its ferocious and savage disposition, and the fitness of its organs for the exercise of its ravenous propensities. It may be expressly asserted, that no species of larva is known that is provided with weapons of destruction so powerful, so numerous, and well adapted to their end, as those which this creature possesses. It is on this account that it has been popularly called the Water Devil. Its size is but little inferior to that of the larva of any of the British Coleoptera, as it measures, when arrived at maturity, an inch and a-half in length, while the superior strength and courage manifested in its attacks on small fish, and other animals larger than itself, is truly surprising.

About the later end of April, and during the month of May, small nests of these insects are often found floating among the weeds and water plants in stagnant pools, and are frequently taken in the nets of those who are searching for the early species of animalcules. They are in the form of balls, of a dusky white colour, and a silky texture, and have each a small stem of the same nature as the rest, by means of which it is attached to the roots or stalks of reeds at the bottom of the water. In this situation it remains during the winter, and is then effectually preserved from the effects of intense cold. Early in the spring, the stem or cable to which we have referred, is detached from the reeds, by the winds which at that time prevail, and the nest rises to the surface of the water, and there floating imbibes the genial influence of the sun. These nests may be taken and placed in a basin of water, and, as the season advances, hatched by the heat of the sun. On the larvæ leaving the nest, which they accomplish by gnawing a hole in the side, the infant larva immediately descends to the bottom of the vessel with its jaws extended in quest of prey, and eagerly devours all the small aquatic insects that are within its reach; if, however, there is a scarcity of food in the immediate neighbourhood of the nest, the larvæ of the same brood may be seen to attack and devour each other.

In its infant state this larva is very transparent; hence its internal structure may be clearly distinguished. The circulation along the principal artery on each side the body can be distinctly observed, together with the violent alternate motion of the vermiform body near the lower extremity.

It is at this time about a quarter of an inch in length, and swims very nimbly. The colour of the head is a strong Indian yellow, with darker shadings of a bright chestnut. It is more sparingly covered with hairs than at a more advanced period of its age; and the head is larger in proportion to the size of the body, than when the creature is arrived at maturity. In this respect it resembles the mode of growth of many other creatures, in which the head comes to be developed and perfected before the rest of the system.

The manner in which the larva treats its prey evinces an extraordinary degree of instinct. Many of the creatures on which it feeds are crustaceous about the head and back; hence their most vulnerable part is the belly. This part, therefore, the larva attacks, and to accomplish its aim, swims underneath the intended victim, and bending back its head, which is even with the surface of its back, is enabled to reach its prey by means of its jointed antennæ. Its next operation is to pierce it with the mandibles. Having thus secured its object, it immediately ascends to the top of the water, and holding its prey above the surface, so as to prevent it struggling, shakes it as a dog would a cat. The prey, however, of this larva, is often larger than its destroyer. Its next operation is to insert the piercer and sucker, which is capable of being thrust out or withdrawn at pleasure. When the juices of the victims are not easily procured by suction and exhaustion, the serrated pair of forceps is employed to tear and masticate it, and thus cause the juices to be more easily obtained. If its food be plentiful, this larva arrives at its full growth in the course of three or four weeks, and is then nearly opaque and thickly covered with hair. It can be kept several days without food, and by this ex-inanition its structure becomes considerably more transparent, while its natural ferocity is greatly increased, so that it will attack and fight with creatures much larger than itself, and even with its own species. It may be remarked that it studiously avoids any contest with the *nepa* or Water-Scorpion.

On a fine sunny day the larvæ arise to the surface, and delight to bask in the sun, but if watched, they remain motionless, with their claws extended. If a stick, or any other substance, be presented to them, they will immediately seize it, and will sometimes suffer themselves to be cut into pieces before they relinquish their hold. Their bite has been considered poisonous by many persons, as it takes a greater time to heal than other wounds of the same extent, so that caution should be used in taking them.

Touching the anatomy of this creature, it may be observed, that the sucker is contained in a crustaceous sheath, and may be considerably protruded or completely withdrawn at the pleasure of the larva. The eyes are compound, but of a peculiar conformation, being composed of seven oval lenses arranged like leaves upon a branch. The whole of the head and thorax is curiously marked with a number of lines and spots. The legs are six in number; they are thickly set with rows of hair on their opposite sides, and each is furnished with a sharp claw. The number of swimmers on each side is seven; they are covered with hairs, and in the specimens examined a vast number of *vorticella* or bell polypi were attached. They sometimes infest this species of larva to such a degree, as considerably to impede its motions in swimming. On each side of the abdomen, which commences near the origin of the first pair of tracheæ, or swimmers, arise the great vessels, of a light blue colour; the rest are probably united near the tail, where an exceedingly curious process is also distinctly exhibited. The whole surface of the body is thickly covered with hairs, and several tufts are disposed in clusters, with some regularity, down the back and sides, are so much more distinct.

The flexible pulsatory organ before alluded to, is in perpetual motion. It resembles the letter S inverted: it, however, varies a little during its vibrating motions in the intestinal canal. The use of the curious appendages at the lower extremity of the body is unknown. Its tail is biforked and crustaceous. As it approaches maturity it casts its skin several times, from each of which it escapes by a rent formed down the back.

After this creature has remained for a considerable time in the larva state, it buries itself in a hole, which it forms for that purpose near the edge of the water, and after passing through the chrysalis state, it emerges in the form of a perfect beetle."

III.—*A Flora of Berwick upon Tweed.* By GEORGE JOHNSTON, M. D. Fellow of the College of Surgeons, &c. Vol. I. PHÆNOGAMOUS PLANTS. Edin. 1829. 12mo. Pp. 250.

A few years ago the Society of Arts for Scotland recommended a minute examination of the natural history of this country, and offered prizes for the best papers on the mineralogy, geology, or botany of counties or particular districts. One or two good memoirs, we believe, were transmitted to the society, but no zeal has been shown by the few who are qualified for the task to carry the Society's views into effect.

The present work of Mr Johnston on the Flora of Berwickshire is a model for memoirs of this description, and we anxiously hope that our clergymen and medical practitioners, who are peculiarly fitted for such inquiries, will follow the excellent example which has been set to them.

The unoccupied time, which, on his entrance into business, falls to the lot of almost every physician, was devoted by our author to the examination of the indigenous plants of his neighbourhood, and the catalogue of his discoveries gradually increased till it assumed the form which it now bears.

" The chief object of the book," says Mr Johnston, " is to give such a description of the plants growing wild in the vicinity of Berwick, as may enable any one acquainted with the elements of the science, to ascertain the names by which they are known; and it will likewise serve as a guide to conduct the inquirer to the places where the rarer species are to be found. The utility of a work of this kind, consists in its facilitating the investigation of species to those resident within the limits of which it treats, by lessening the objects of comparison; while others may find in it some facts illustrative of the geographical distribution of our native plants, and of the influence which particular situations exert in producing changes in their appearances.

"To relieve, however, the dryness of mere descriptive detail, and to point out the manner in which this study may be made most conducive to our amusement, if not to our instruction, various particulars have been added relative to the uses of our plants in agriculture, in the arts, and in medicine. And, in the Flora of a river so celebrated as the Tweed in pastoral poetry, and 'where flowers of fairy blow,' it seemed allowable to notice, at greater length than is usual in works of science, the purposes to which superstition has applied them in former times, and the illustrations which they have afforded to the poets of our own day. A few facts relative to the physiology of vegetable life have been also given; but of what I had collected by far the greater portion has been cancelled, lest our work should have exceeded its proper limits. I cannot, however, but strongly recommend to the young botanist the attentive observation of such phenomena; it will add greatly to the pleasure of the walks which he must take in search of the objects of his study, and will remove from him the reproach which has sometimes been cast upon us, of being mere collectors of vegetable curiosities, of which we seemed anxious to know nothing beyond the barbarous name that some dull systematist may have given them. I, indeed, cannot praise the botanist, who has no other object in his excursions than to add a specimen to his herbarium, and who confines his examination of it to those characters by which he ascertains its name in the system. I know well that such investigations are not void of interest, —it is akin to that which the mathematician feels in the solution of a problem,—but botany has other pleasures.

"There is not a flower which blows but has some beauty only unveiled to the minute inquirer,—some peculiarity in structure fitting it for its destined place and purpose, and yet not patent to a casual glance. Many are full of remembrances and associations, in which it is good for us to indulge. To the student 'a yellow primrose on the brim,' should be something more than a yellow primrose. He should, to borrow the words of the author of the '*Sketch Book*,' be continually coming upon some little document of poetry in the blossomed hawthorn, the daisy, the cowslip, the primrose, or some other simple object that has received a supernatural value from the muse. And, as his pursuit leads him into the most wild and beautiful scenes of nature, so his knowledge enables him to enjoy them with a higher relish than others. They are full of his 'familiar friends, with whom he holds a kind of intellectual communion; he can analyze the landscape, and assign to every individual its share in the general effect.'

The reader will see from this quotation, that Dr Johnston is alive to all those fine associations, which the study of nature never fails to excite in an accomplished and well constituted mind. In every part of his work this tone of mind is apparent, and we can safely assert, that we know of no similar botanical work, in which the necessary dryness and formality of technological description, is so agreeably enlivened by the most appropriate quotations from our classical poets, and by interesting observations relative to the uses and history of plants, and the phenomena of vegetable life.

The generic and specific characters of the plants described in this work are remarkably clear and precise, and the occasional discussions which occur respecting differences of species, evince much knowledge and acuteness. The botanist will find many new and important observations recorded in this little volume, which will not properly admit of being extracted, as specimens of the work; but we shall make no apology for copying the description of a new species discovered by Dr Johnston, viz. the *Melampyrum montanum*.

"*M. montanum*, leaves linear, floral ones quite entire; flowers axillary, in partly distant pairs, turned to one side; corolla about twice as long as the calyx, closed, lip direct. (*Nova species.*)

"*Hab.* On the south-east side of Cheviot, plentiful. June, July. ⊙

"Stem 3 or 4 inches high, square, pubescent, branched; branches opposite, simple. Cotyledon-leaves linear-obovate, entire. Leaves narrow, long, linear, often twisted, hairy all over, brownish-green. The floral leaves do not differ from the others. Flowers in pairs, turned to one side, on short stalks, pale yellow, with a white tube. Calyx striped with green and reddish-brown; the segments setaceous, rough, shorter than the tube. Upper lip of the corolla villose internally; lower lip straight, in 3 acute short segments, slightly projecting; the palate raised, orange. Anthers green and brown, pubescent, on smooth filaments. The flower is generally unspotted, but sometimes there are 4 small obscure spots on the lower lip, placed distantly, and not on the mouth.

"It is not without hesitation that I give this as a species distinct from the preceding, since the difference may be attributed to situation, for we know that an alpine station does alter the aspect of plants to a considerable extent. In estimating the force of this objection, we can only reason from what we observe to be the effect of a similar situation on plants of the same natural order. Now, the *Rhinanthus Crista-Galli* is a plant of this kind, and we find it growing with this *Melampyrum* undiminished in height, and unaltered in appearance, and, were the objection valid, we might expect the plant at the base of the hill to be much in its usual state, and gradually diverging from it as it attained higher limits; but this was not the case, for it was very uniform in character over a surface of many acres."

The nomenclature chiefly followed is that of Sir J. E. Smith in the *English Flora*, and Dr Johnston could not have chosen a better guide. Perhaps it would have been useful to have added to the generic name, the natural order under which it is arranged, as has been done by Dr Greville in his *Flora Edinensis*, as leading the student to the classification of vegetables into connected families.

This work is illustrated with two coloured engravings, of the *Veronica filiformis*, and the *Luciola Sudetica*; and the author has given in the preface a very clear and valuable outline of the geology of Berwickshire, furnished by a friend, who need not have concealed his name. An introduction of this kind to the Flora of any district is particularly valuable, as showing the connection of the vegetable productions with the soil, and their geographical distribution, and it adds a value to the botanical catalogue which it would not otherwise possess. We trust that Dr Johnston will be encouraged to go on with the second volume, and illustrate the Cryptogamic botany of the district with equal success; and we cannot help expressing the wish that this work, and others to which it will probably give rise, may excite a love of the science among that class of our gentry whose residence in the country gives them such excellent opportunities for its cultivation; and we strongly recommend it to the young botanist, as a valuable guide to a most delightful study.

IV.—*A Treatise on the Reflexion and Refraction of Light, being Part I. of a System of Optics.* By Henry Coddington, M. A. F. R. S. Fellow of Trinity College, and of the Astronomical and Cambridge Philosopical Societies. Camb. 1829. Pp. 296, and 10 Folding Plates.

Mr Coddington is already favourably known to the scientific world by an "*Elementary Treatise on Optics*," which was published in 1823. The present treatise is on an enlarged plan, and is intended to introduce the reader to those important theories which have lately extended the boundaries of optical science. Mr Coddington has executed this work with great ability, and it cannot fail to prove an acceptable manual to the mathematical student; but we fear that the formulæ are not presented in such a form that the practical optician, or those who have only a small portion of mathematical learning, will be able to derive any advantage from them. The following are the subjects treated of in this part:—

Introduction. On light in general, and of photometry.

Chap. I. Reflexion of light—Combined reflexions at plane surfaces.

Chap. II. Refraction of homogeneous light—combined refractions—prism—lens—refracting spheres—combined lenses.

Chap. III. Refraction combined with reflection.

Chap. IV. Images. Vision in mirrors, or through lenses.

Chap. V. Caustics.

Chap. VI. Chromatic dispersion of light.

Chap. VII. Atmospheric refraction.

The chapters which treat of achromatism, and the spherical aberration of eye-pieces, contain the substance of Professor Airy's valuable papers on these subjects which appeared in the Cambridge *Philosophical Transactions;* and as this eminent mathematician communicated to Mr Coddington the results of his unpublished researches, these difficult branches of the subject are treated with much ingenuity and talent.

If this work should meet with the approbation of the scientific world, Mr Coddington intends to devote the next year to the subject of optical instruments, and he has requested the communication of "any hints with regard to the real practical difficulties or requisites of persons engaged in the use or construction of them."

We look forward with high expectation to this part of our author's labours; and we trust that he will render it accessible to the practical optician.

V.—*An Essay on the use of the Nitrate of Silver in the cure of Inflammation, Wounds, and Ulcers.* By John Higginbottom, Nottingham, Member of the Royal College of Surgeons. 2d Edit. Lond. 1829. Pp. 220.

Although works on medical or surgical subjects are not within the scope of this *Journal*, unless when the subject of which they treat is of a general nature, yet, as the present relates to a remedy so simple, so easily applied, and so useful in every family, we have no hesitation in noticing it.

Medical practitioners had some indistinct notions of the benefits derived from the use of nitrate of silver; but these cannot be regarded as diminishing in the least the great merit of Mr Higginbottom's discovery of the universality of its efficacy, and of the proper mode of applying it.

In the several departments of *army*, *navy*, and *hospital* practice, its utility must be very great. Its application is so simple, and its operation so quick, that, by rendering unnecessary a multiplicity of dressings, the period of residence in hospital may be greatly shortened. Instead of daily dressings, attention to the patient every third or fourth day is frequently all that is required.

Mr Higginbottom has pointed out the prevailing error, that the nitrate of silver acts as a *caustic*. He considers it as the very reverse of a caustic, as it is impossible to destroy by it any but the most superficial parts. "I speak of it," says he, "in its *solid form*. Instead of destroying, it frequently preserves parts which would inevitably slough, except for the extraordinary *preservative powers* of this remedy. A new term is in fact required for the peculiar kind of influence which the nitrate of silver possesses in subduing and checking inflammation in phlegmon and erysipelas, —in inducing the adhesive inflammation in wounds,—in preserving the health of parts, which in cases of puncture or bruise are ready to take on the suppurative or sloughing process,—and lastly, in changing various specific actions, and inducing one of a more healthy and curative kind."

Mr Higginbottom's work is divided as follows:—

Chap. I. On the principle of the treatment by the nitrate of silver.

Chap. II. Of the use of the nitrate of silver in the treatment of external inflammation.

Chap. III. Of the treatment of punctured wounds.

Chap. IV. Of the treatment of bruised wounds.

Chap. V. Of the treatment of ulcers.

Chap. VI. On old ulcers of the legs.

Chap. VII. Of burns and scalds.

Appendix I. treats of the use of the nitrate as a blister, and contains various cases of its successful application.

Appendix II. contains letters from Mr Webster of Dulwich and Mr Browne, Camberwell, recommendatory of the nitrate of silver.

As a specimen of the work, we shall extract the section of Appendix I. *on the treatment of corns*, a subject of popular interest.

"The nitrate of silver is an old remedy for corns; but as the plan which I adopt is rather different from that usually employed, I will describe it briefly in this place.

"The patient should put the feet in warm water at bed-time for half an hour, to soften the corns: as much of the corn should then be removed, by means of a sharp knife, as can be done without making a wound: the corns and surrounding skin are then to be moistened with water, and the nitrate of silver is to be rubbed on the corn very freely, and lightly on the skin, so as not to occasion vesication: the part is then to be exposed to dry.

"Little advantage would be derived, if nothing more were done, as the black eschar would remain on the corn for some weeks, and during that time the corn would form a-new. About the fourteenth it will be observed that the cuticle is peeling off around the corn: this is the proper time for putting the feet in warm water again, and for removing the eschar, and as much as possible the corn underneath, by the knife. At this period there is a distinct mark between the surrounding healthy cuticle and the corn, so that the latter may be removed more effectually than at first. The nitrate of silver is to be again applied as before. This plan is to be repeated until the corn be perfectly destroyed." P. 177.

VI. *The Influence of Climate in the Prevention and Cure of Chronic Diseases, more particularly of the Chest and Digestive Organs: Comprising an account of the principal places resorted to by Invalids in England and the South of Europe; a comparative estimate of their respective merits in particular diseases; and general directions for Invalids while travelling and residing abroad. With an Appendix, containing a series of Tables on Climate.* By JAMES CLARK, M. D. Member of the Royal College of Physicians of London; Corresponding Member of the Royal Medical Society of Marseilles, of the Medico-Chirurgical Society of Naples, of the Medical and Physical Society of Florence, of the Academy of Sciences of Sienna, &c. &c. London, 1829. pp. 328.

THE author of this work has been already advantageously known to the public, by a small volume of "Notes on the climate and medical institutions of France and Italy;" but in consequence of enjoying additional opportunities of observation, he has been led to treat the same subject under a much more comprehensive and philosophical aspect. That such a work was much wanted, not only by the medical profession, but by the numerous invalids who seek the recovery of their health in foreign countries cannot be doubted, and while its author has endeavoured to accommodate it to the perusal of the latter, he has attempted to preserve its utility to the former.

Dr Clark's work is divided into two parts. The first part treats of the general physical characters of the milder climates of the South of Europe and of England, and the author has pointed out the manner in which the climate of different places is modified by local causes, and has compared these places relatively to their influence on diseases. In this part, the author considers the climates of England, France, Nice, Italy, and Madeira.

As an example of the relative influence of climate in cases of confirmed and incipient consumption, we shall quote the observations made by Dr Renton and Dr Heineken in Madeira.

According to Dr Renton's own observations, the following were the comparative results

Cases of *confirmed* phthisis, - - - -	47
Of these there died within six months after their arrival at Madeira,	32
Went home in Summer, returned and died, - -	6
Left the Island and died, - - - -	6
Not since heard of—probably dead, - - -	3
Total,	47
Cases of *incipient* phthisis. - - -	35
Of these there left the island much improved, and of whom we have had good accounts, - - -	26
Also improved, but not since heard of, - -	5
Have since died, - - - -	4
	35

With the preceding results the observations of Dr Heineken are in perfect accordance.

" Since the summer of 1821, says he, about 35 invalids (I speak from memory,) have either reached or sailed for this Island (Madeira). Of this number two or three died on shipboard, and three within a month of their landing; five or six just survived the winter, about an equal number lingered through the spring, and three or four entered upon and passed through a second winter. Of the whole number thirteen only, including myself, are now (1824) in existence. Two of those were cases of asthma, and two of chronic disease of the trachea and larynx; if these be excepted, and those be considered as dead who cannot be alive three months hence, the survivors of thirty-five or thereabouts, in the short space of 2½ years, and who so far from being cured can only make the best of a precarious existence, in a low latitude, will be reduced to six."

In the *Second* part of this work Dr Clark has given some account of the principal diseases which are benefited by a mild climate. These diseases are treated in the following order:—

1. Disorders of the digestive organs.
2. Consumption.
3. Disorders of the larynx, trachea and bronchia.
4. Asthma.
5. Gout.
6. Chronic Rheumatism.
7. General delicacy of constitution in childhood and youth.
8. Premature decay at a more advanced period of life.
9. Disordered health from hot climates.

The first part of the work is illustracted by a series of meteorological tables, drawn up by Dr Todd of Brighton. These tables are eleven in number, and evince much research and knowledge of the subject.

From this brief analysis of Dr Clark's volume, the reader will be able to form an idea of the importance of the subject of which it treats. It is written with great plainness of language; displays a very great degree of talent and research, and forms, what the author wished it to be, "a *manual* to the physician in selecting a proper climate for his patient, and a *guide* to the latter while no longer under the direction of his medical adviser." We would therefore strongly recommend it to the notice both of professional and general readers, and we trust that Dr Clark will be enabled, in subsequent editions, to avail himself of the latest information, which either his own observations, or those of his professional brethren, may from time to time supply.

Art. XXIII.—PROCEEDINGS OF SOCIETIES.

1. *Proceedings of the Society for the Encouragement of the Useful Arts in Scotland*

April 29, 1829.—1. An Orrery, constructed from new calculations, which exhibits the motions of those planets that are visible to the naked eye, with some of their satellites, which in their epochs and revolutions make a near approximation to the times of those bodies which they represent, invented by Mr Forrester, Teacher, Kirkaldy, was exhibited to the Society, and a description of it read. A committee was appointed to examine into the merits of the invention.

2. A Description and Sketch of an improved Turning-Lathe, in which the band is applied in a peculiar way, so as to increase the power of the lathe, and decrease friction of the mandril, by Mr John Henry, 49, Leith Wynd, were read and exhibited, and a committee appointed to examine and report

3. The Committee on Mr Brown's Mangle gave in their report, which was favourable.

4. The Committee on Mr Macdonald's Instrument for the use of Tailors, gave in their report, which was very favourable.

5. The Committee on Mr Aytoun's Lighthouse Machinery gave in their report,—recommitted, with instructions also to report on Mr Robert Stevenson's method.

6. A Letter from Charles Grey, Esq. was read, relative to a mode of preventing Collision of Steam Boats and other vessels, either on a river or on the open sea, by means of two lights of different colours, the one suspended forward and low, the other more aft, and much higher than the fore-light.

Mr Archibald Horne, Accountant, was elected an Ordinary Member.

May 27.—1. Mr Edward Sang read an account of the best form of the Grooves for the Pulley of the Foot-Lathe, so as to increase the friction of the band, and give additional power to the lathe.

2. The Committee appointed to ascertain the merits of Mr Henry's method of passing the band on the pulley of the foot-lathe gave in their report, which, upon the whole, is favourable; but from the rude manner in which the lathe to which it was applied was constructed, they found it impossible to judge of the full effects of the plan. The Society appointed the

Committee to experiment with Mr Henry's band upon a more perfect machine and report.

3. The Committee on Mr Forrester's Orrery gave in their report, which was highly favourable.

4. The Committee on Mr Aytoun's and Mr Stevenson's Lighthouse Machinery not being ready to report, the Committee was continued.

5. The Committee on Prizes to be awarded next month was continued.

6. James L'Amy, Esq. and Robert Forsyth, Esq. were elected Vice-Presidents.

The General Meeting of the Society for awarding the Prizes to successful candidates was appointed to be held on the 17th June.

2. *Northern Inverness Institution.*

April 24, 1829.—The concluding meeting for the season of this Society, was held in their Museum here on the evening of Friday last,—Neil MacLean, Esq. Civil Engineer, in the chair. After the usual routine business, and the letters received during the previous month had been read, a communication was laid before the meeting from Wm. Mackintosh, Esq. of Millbank, accompanied by a curious piece of carved wood, dug up in a moss in Badenoch, some years ago, with drawings of its appendages when found, which have since been lost.

A notice from Charles Cramer, Esq. Correspondent of the Society, of an Egyptian Mummy, recently opened in London, was next read, and portions of the linen folds in which the body was enveloped, were laid on the table.

The other donations presented at this meeting, consisted of an Indian Creece from a lady; copies of the St James's Chronicle published in 1764-65, found in Kilravock Castle, from Mr Macarthur, writer, Nairn; and a beautiful series, 22 in number, of flexible Corallines from the shores of the Frith of Forth, collected and named by the donor, John Coldstream, Esq. M.D. the Society's Correspondent at Leith.

The Essay appointed for this meeting, and with the reading of which the business closed, was by Mr Anderson, the General Secretary, being the first of a series of papers on the Elements of Geology. It treated of the uses and objects of the science, its principles of classification, and concluded with a view of the great classes of rocky and other deposits composing the upper portions or crust of the earth. The subject was illustrated by numerous charts and specimens.

Mr Anderson will read the rest of these papers in the course of the summer.

Art. XXIV.—SCIENTIFIC INTELLIGENCE.

I. NATURAL PHILOSOPHY.

ASTRONOMY.

1. *Comparison of Observations on the Solar Eclipse of November 29th 1826.* By Mr George Innes, Aberdeen.—Having had early communications of the observations of the Solar Eclipse of the 29th November 1826, which were made in Great Britain and Ireland, I made the necessary calculations, in order to deduce the longitude of each of the places where the obser-

vations were made, and communicated the results to the Astronomical Society of London, which the committee of the society published in their monthly notices, in the *Philosophical Magazine and Annals of Philosophy* for September 1827.—In addition to the observations made in this country, I have lately met with some others made in Germany and Italy; and although most of these have been already calculated by M. Wurm and M. Santini, and inserted in some of the numbers of the *Astronomische Nachrichten*, they could not be compared with those I had previously calculated, as it appears that M. Santini used Carlini's Tables of the Sun, and Burckhardt's Tables of the Moon; whereas I used Delambre's Solar, and Damoiseau's Lunar Tables; and M. Wurm does not state what set of tables he has used. Also, the ellipticity, $\frac{1}{309}$, which I have used for reducing the latitudes of the places of observation, and also the moon's parallax, differs from that used by M. Santini, which is $\frac{1}{330}$. It does not appear what ellipticity M. Wurm has used. Although the figure of the earth has not as yet been satisfactorily ascertained, I do not think it can very much affect the present results, where the same ellipticity has been used for each place of observation, whatever may have been the ellipticity used by M. Wurm, his difference of longitude of Bushy Heath and Aberdeen differs only 0″03 from mine, but the difference of our results for Abo is considerable. No correction was applied to the semi-diameter of the sun for irradiation, nor to that of the moon for inflection, as these have not been well ascertained by astronomers.

Having reason to think the Bushy Heath observations as good as most of the others, as its longitude has been well ascertained, and as both the beginning and end of the eclipse were observed, I have used it as the basis in the reductions. It is in longitude 10′ 42″,43, in time west from Paris.

…es of Observation.	Observations, Apparent Time.	Conjunction, Apparent Time.	Correction of Tables in Lat.	Longitude from Bushey Heath.	Longitude from Paris.
	d h m s	d h m s			
…Heath, Beginning	28 21 57 37,14	28 23 35 50,66			10′ 42″,43W.
—— End	29 0 9 50,26	— 23 35 47,64			
—— Beg. & End		— 23 35 50,19	— 2″,488		
…, End	29 0 12 25,25	28 23 37 38,08		1′ 50″,44 E.	8 51,99W.
25 inch. Achrom.	29 0 11 42,75				
46 inch. Achrom.	29 0 11 43,64				
5 feet Achrom.	29 0 11 44,65				
5 feet Equator.	29 0 11 47,75	28 23 37 13,21		1 25,57 E.	9 16,86W.
30 inch. Achrom.	29 0 11 47,75				
7 feet Newton.	29 0 11 49,25				
…, End	28 23 37 1,39	28 23 11 51,40		23 56,24W.	34 38,67W.
…, End	28 23 35 42,19	28 23 10 34,23		25 13,41W.	35 55,84W.
…en, End	29 0 1 16,42	28 23 28 48,30		6 59,34W.	17 41,77W.
Beginning	28 23 54 21,84	29 1 6 21,51		90 30,85 E.	79 48,42 E.
End	29 2 5 0,41	— 1 6 14,55		90 26,91 E.	79 44,48 E.
Beg. & End		— 1 6 19,07	— 5,168	90 28,88 E.	79 46,45 E.
Beginning	28 23 0 54,83	29 0 24 38,35		48 47,69 E.	38 5,26 E.
End	29 1 18 32,13	— 0 24 34,69		48 47,05 E.	38 4,62 E.
Beg. & End		— 0 24 36,81	— 1,824	48 46,62 E.	38 4,19 E.
Beginning	28 23 16 15,87	29 0 34 7,97		58 17,31 E.	47 34,88 E.
End	29 1 32 27,90	— 0 34 13,52		58 25,88 E.	47 43,45 E.
Beg. & End		— 0 34 10,27	+ 2,345	58 20,08 E.	47 37,65 E.
Beginning	28 22 46 21,83	29 0 13 56,85		38 6,19 E.	27 23,76 E.
…erg, End	29 1 59 42,16	29 0 58 15,67		82 28,03 E.	71 45,60 E.

There appears to be some error in the Naples observations, as the error of the tables in latitude comes out with a contrary sign. The Königsberg observation is taken from Mr Bessel's observations for the year 1826, but it seems that there is an error of one minute in printing.

The following are the elements which I have obtained from Delambre's Tables of the Sun, and Damoiseau's Tables of 1824 for the Moon. In adopting the tables of Damoiseau, I have reduced the results from the decimal, to the sexagesimal division of the circle.

		d	h	m	s
True time of ecliptic conjunction at Paris. Mean time, Nov.		28	23	34	44,89
Equation of mean to apparent time, at conjunction,	+			11	31,76
True time of ecliptic conjunction. Apparent time,		28	23	46	16,65
Longitude of the sun and moon at Conjunction,			246°46′		19″84
Apparent obliquity of the ecliptic,			23	27	36,86
Sun's right ascension,			244	55	38,92
—— declination, north,			21	27	34,17
—— hourly motion in longitude,				2	32,19
—— ———— in Right Ascension,				2	41,05
—— Semidiameter,				16	15,15
—— horizontal parallax,					8,93
Hourly decrease of the equation of time,					0,875
Moon's latitude, north increasing,			1	12	29,55
—— equatorial horizontal parallax,			1	1	23,84
—— horizontal semidiameter,				16	43,85
—— hourly motion in long., first order,				38	8,447
—— ———— second order,	—				0,064
—— hourly motion in latit., first order.	+			3	25,904
—— ———— second order,	—				0,277

George Innes.

Astron. Nachrichten, No. 161, p. 341.

2. *Occultations of Aldebaran and the Moon on the 15th October and 9th December* 1829. *Calculated by* Mr Henderson and Mr Maclear.

Place.	Immersion. Sidereal time.	Immersion. Mean time.	Immersion. Angle.	Emersion. Sidereal time.	Emersion. Mean time.	Emersion. Angle.
		October 15, 1829.				
Dorpat,	0 52	11 15	292	1 41	12 4	35
Konigsberg,	0 17	10 40	283	1 7	11 31	32
Vienna,	23 45	10 8	265	0 41	11 4	34
Naples,	23 22	9 45	243	0 22	10 46	41
Milan,	23 10	9 34	262	0 4	10 27	30
Paris,	22 51	9 14	280	23 35	9 58	19
Greenwich,	22 48	9 12	292	23 25	9 49	12
Edinburgh,	22 50	9 13	313	23 12	9 35	358
Dublin,	22 31	8 55	306	22 56	9 20	1
		December 9, 1829.				
Dorpat,	1 4	7 51	262	2 8	8 55	64

Kouigsberg,	0 29	7 16	253	1 33	8 20	65
Vienna,	0 0	6 47	236	1 3	7 50	66
Naples,	23 42	6 29	215	0 41	7 28	69
Milan,	23 25	6 12	233	0 25	7 12	61
Paris,	23 1	5 49	249	23 59	6 46	51
Greenwich,	22 57	5 44	257	23 53	6 40	47
Edinburgh,	22 53	5 40	271	23 44	6 31	41
Dublin,	22 36	5 23	268	23 27	6 14	39

PNEUMATICS.

3. *On the cold produced by the dilatation of air.* By M. Legrand.—The general law that air is cooled by its dilatation was controverted by MM. Gay-Lussac and Welter, in the particular case where it is blown out of an aperture under a constant pressure. This strange result, which was deduced from an experiment made with a fire engine at Chaillot, is published in the *Ann. de Chim.* tom. xix. p. 416. M. Legrand, Professor of Natural Philosophy at Besançon, has obtained very different results from the same engine. The following were his observations :—

Distances from the aperture or cock.	Temperature, Cent.	Cooling.
10 million.	22	7°.5
50	25.5	4
100	26.8	2.7
200	28.8	0.7
250	29	0.5

The temperature of the external air was 29°.5, and the third column is the difference between this number and the temperature in the second column. When the cock was taken out altogether, and the bulb of the thermometer put in its place, the temperature oscillated between 12°.5 and 13°.5, so that the cooling was here about *fifteen degrees* centigrade, or *twenty-seven* degrees of Fahrenheit.

The experiments were repeated in June 1829 by M. Saigey, who obtained analogous results.—*Annales des Sciences d'Observation*, No. i. p. 45. Par M. M. Saigey and Raspail.

II. NATURAL HISTORY.

MINERALOGY.

4. *Analysis of the Brochantite.* (Mohs's *Mineralogy*, translated by Haidinger, vol. iii. p. 81.)—Mr Magnus of Berlin has found in the variety of this ore from Transylvania, the specific gravity of which was = 3.78 ... 3.87, the following elements :—

Oxide of copper,	-	-	66.935
——— tin,	-	-	3.145
——— lead,	-	-	1.048
Sulphuric acid,	-	-	17.426
Water,	-	-	11.917

Formula: $\dddot{C}u^3\,S + 3\,A\,q$

Poggendorff's *Annalen*, vol. xiv. p. 141.

5. *Formulæ for the Manganese Ores.*—The species of *manganese ores* of which complete mineralogical and chemical descriptions were published in this *Journal*, No. vii. p. 41–51, and No. xviii. p. 304 and 349, have the following formulas:—

1. Manganite,	- -	$\ddot{M}n + \ddot{H}$
2. Braunite,	- -	$\dddot{M}n$
3. Hausmannite,	-	$\ddot{M}\dot{n} + \dddot{M}n$
4. Pyrolusite,	- -	$\ddot{M}n$
5. Psilomelane,	-	$\ddot{M}n + x\,\dot{B}a$

(Poggendorff's *Annalen*, vol. iv. p. 221.)

6. *Measurement of the Crystals of Adularia.*—Professor Kupffer at Kasan has measured crystals of *Adularia* from the Tyrol with the reflective goniometer, and has found the following angles:—

T on $t = 118° 48'.6$; T on $P = 112° 16'.0$; x on $T = 110° 40.'25$; x on $P = 129° 40.8$; P on the axis $= 63° 53'.0$; x on the axis $= 65°47'.3$. —Poggendorff's *Annalen*, vol. xiii. p. 209.

7. *Account of Davyne, a New Mineral Species.* By W. Haidinger, Esq.—The prismatic form of this mineral is a rhomboid of 112° 16′ whose axis is $\sqrt{1.59}$. It is colourless and transparent. Its lustre is feeble and sometimes pearly. Its hardness is a little greater than that of hepatite. Its specific gravity is 2.4. It forms a jelly with acids. It is accompanied with brown dodecahedral garnet. It is composed as follows:—

Silex, - - -	0.42.91
Alumina, - - -	33.28
Lime, - - -	12.02
Oxide of iron, - - -	1.25
Water, - - - -	7.43
	96.89

Hence its forumula is
$C S^2 + 5 A S + 2 Aq.$

Pogg. Ann. 1827. p. 471.

8. *On Specific Gravity as a Mineralogical Character.* By M. Beudant. M. Beudant has determined by many experiments, that the specific gravity of the same species when very pure varies perceptibly with the state of aggregation. It attains its maximum in small crystals, and its minimum in varieties of a compound structure. Thus the specific gravity varies in the following minerals:

Carbonate of lime, from	2.7234	to 2.5239
Arragonite,	2.9467	2.7647
Malachite,	3.5907	3.5673
Carbonate of lead,	6.7293	6.7102
Sulphate of lime,	2.3257	2.3121
———— strontian,	3.9593	3.9297
Pure galæna,	7.7593	7.7487
Pure quartz,	2.6541	2.6413

In all these substances it is the small crystals which possess the greatest specific gravity, whence it follows that they have the greatest homogeneity, and that large crystals have in their interior vacuities more or less considerable.

The lamellar structure diminishes the specific gravity 0.0173: The fibrous structure with parallel fibres about 0.0177: The structure with diverging fibres 0.0186: The structure with interlaced fibres 0.0319.

The lowest specific gravities appear to take place in the epigenous varieties of different substances.

But all the varieties of the same substance present the same specific gravity when they are reduced to powder. Hence it is clear, that, if we wish to make specific gravity a comparable character, it is the absolute specific weight that we must use, and not the weight relative to the external volume which the substance occupies, as has hitherto been done. The specific gravity of the powders is always a little less than that of the small crystals, which arises probably from the production of some fissures in the particles while pounding the body.

According to my experiments, the following are the comparable specific gravities of the above eight substances:

Carbonate of lime, - - -	2.7321
Arragonite, - - -	2.9466
Malachite, - - -	3.5904
Carbonate of lead, - - -	6.7290
Sulphate of lime, - - -	2.3316
——strontian, - - -	3.9592
Galæna, - - -	7.7592
Quartz, - - -	2.6540

ZOOLOGY.

9. *Captain Brown's new work on Horses.*—We are informed that Captain Brown has in the press, a work to be entitled *Biographical Sketches, and Authentic Anecdotes of Horses;* with a Historical Introduction, and an Appendix on the Diseases and Medical Treatment of the Horse. It is to be illustrated by figures of the different breeds, and portraits of celebrated or remarkable horses; these are to be engraved on steel by Mr Lizars in his *best style*. This Work is intended as a companion for the work on dogs, by the same Author, recently published, which has deservedly met with so favourable a reception.

10. *Notice regarding the Male and Female Orang-outang in the possession of* George Swinton, Esq. of Calcutta, *in a letter to* Dr Brewster, dated 13th June 1828.

"Last year I sent you an account of my orang-outang*. I have lately got a female companion for him, apparently of the same age. She wants the thumb nails of the lower extremities, which confirms me in the opinion that this is a distinction of sex, not of species. The young female

* See this *Journal*, vol. ix. p. 1.

carried here by Lady Amherst wanted these nails. My male, and the great Sumatran orang described by Dr Abel has them. The thumb of the foot in the female looks as if the upper joint had been chopped off below the nai and the skin had healed over the wound."

Mr Swinton goes on to mention the deportment of the two orangs on their first introduction to each other. They tumbled about like children, but without any symptoms of sexual desire, which he attributes to their being so very young. The following notice of the female in a letter from Captain Hull to Mr Swinton, with Mr Swinton's remarks, will be read with interest; and we hope Dr Grant, whose able description of the male appeared in this *Journal*, will find leisure to draw up a similar account of the female. In case of the death of one or both of the animals, their bodies are to be preserved and sent to England for dissection.

"This female stands two feet six inches in height; is extremely docile and playful; has been in the possession of Mrs B. for nearly twelve months, during which period it became the constant play-mate and companion of Mrs B's. children; and the only information I can give respecting the abode of this animal is, that it was sent here by Mr B. from Macassar, who is residing on the Celebes. I conclude that this animal is a native of Borneo, which island lies adjacent, distant only a few miles across the Straits; and most probably it came from the woods near Bangirwassin.

"This animal must be very young from the appearance of the teeth. The number of grinders in each jaw is four. In the adult described by Dr Abel the grinders are ten in each jaw. It differs in external appearance in some points from the orang-outang which I saw at Mr Swinton's. The head is more thickly covered with hair, and hangs down much longer on each side of the cheeks, and is more bushy. The nose is a more prominent feature *, and the lips are thicker, especially the under lip †, and turns more outward than in any other of the species which I have seen, one of the marked distinctions between this order and man. The nail on the great toe is wanting; this is an essential difference ‡. Its gait or mode of moving about the room is more generally at a walk in an upright posture, whereas the animal which I used to observe at Mr Swinton's scarcely ever attempted to move in an upright posture §. On the contrary, his manner of

* Very slightly when together, the female can only be distinguished by a more slender and feminine appearance. If any thing, she is rather taller.—G. S.

The nostrils are more defined and raised; but can hardly be called *prominent*. If any thing so flat can be called a *nose*, I would say that *her* nose is handsomer than *his*.—G. S.

† I see no difference in the under lip. It is perpetually varying in thinness or thickness from the action of the muscles, just as we can make the lip thick or thin by contracting or stretching it.—G. S.

‡ Not an *essential* but a *sexual* difference I am strongly inclined to believe. Dr Montgomery informs me that the female he dissected at Singapore wanted this nail. This then is the third female in which the nail has not been found.—G. S.

§ The female may have been taught to stand upright. In playing together, they move exactly in the same way; but she can balance herself better on her legs than *he* can.—G. S.

moving was in a stooping position, pushing himself along the ground with his hands like to a cripple bent double. It is worthy of remark, that in accelerating his motions in this manner, he always used the back of the hand; thus bending the wrist in a contrary direction to the human species.

"Anatomical subjects of the species *Simia Satyrus* will now be a desideratum, because the naturalists who have inspected the female subject which I sent to Sir Stamford Raffles from Sumatra, have described it to be of a different species to the animal already designated and described under the genus *Simia Satyrus* or Orang-outang of Borneo * in *Linnæus's System.* I have not seen the paper myself, which has been read before the Society in London in delineation of the specimen which I transmitted. But I believe one essential difference in the structure of the Sumatran animal which distinguishes it from the Borneo specimens which have hitherto been sent home for examination, is in the number of spinal bones being greater in the Sumatran ape. The naturalists in England have described the Sumatran animal † to be of a different species of *Simia*, which they allege Dr Abel, in his description of the animal brought to Calcutta by Captain Cornfoot, has erroneously classed with the orang-outang of Borneo. What a pity it is there is now so little prospect of obtaining another specimen of this wonderful inhabitant of Sumatra. I do not see how the difference of opinion can be set at rest without obtaining a perfect subject with all the fleshy parts and viscera for examination. ‡ If I meet with an opportunity of returning to Bengal by the way of Sumatra, I shall certainly endeavour to get up to the northern parts, and spare no trouble or expence to procure another subject."

A model of the Male Orang in the possession of Mr Swinton has been sent by that gentleman to the Royal Society of Edinburgh.

11. *Sagacity of Elephants.*—A few days before my arrival at Enon, a troop of elephants came down one dark and rainy night, close to the outskirts of the village. The missionaries heard them bellowing and making an extraordinary noise for a long time at the upper end of their orchard; but knowing well how dangerous it is to encounter these powerful animals in the night, they kept close within their houses till day-light. Next morning, on examining the spot where they had heard the elephants, they discovered the cause of all this nocturnal uproar. There was at this spot a ditch or trench, about four or five feet in width, and nearly fourteen feet in depth, which the industrious missionaries had recently cut through the bank of the river, on purpose to lead out the water to irrigate some part of their garden ground, and to drive a corn mill. Into this trench, which was still unfinished and without

* This is the one already alluded to in some of my former letters. She was about five feet high, and was killed near the same place where the great male described by Dr Abel was found.—G. S.

† Only the hand, foot, and lower jaw and skin brought to Calcutta.—G. S.

‡ I have given a commission to the Captain of a vessel trading with Sumatra to endeavour to get one dead or alive.—G. S.

water, one of the elephants had evidently fallen, for the marks of his feet were distinctly visible at the bottom, as well as the impress of its huge body on its sides. How he had got into it was easy to conjecture, but how, being once in, he had ever contrived to get out again was the marvel. By his own unaided efforts it was obviously impossible for such an animal to have extricated himself. Could his comrades, then, have assisted him? There can be no question but they had; though by what means, unless by hauling him out with their trunks, it would not be easy to conjecture: and in corroboration of this supposition, on examining the spot myself, I found the edges of this trench deeply indented with numerous vestiges, as if the other elephants had stationed themselves on either side, some of them kneeling, and others on their feet, and had thus, by united efforts, and probably after many failures, hoisted their unlucky brother out of the pit.—Similar instances of intelligence and affectionate attachment have been frequently related to me by persons of veracity familiar with the habits of the elephant in his wild state. The following is a specimen. On one occasion, a band of hunters had surprised two elephants, a male and female, in an open spot near the skirts of a thick and thorny jungle. The animals fled towards the thickets: and the male, in spite of many balls which struck him ineffectually, was soon safe from the reach of the pursuers; but the female was so sorely wounded, that she was unable to retreat with the same alacrity, and the hunters having got between her and the wood, were preparing speedily to finish her career, when, all at once, the male rushed forth with the utmost fury from his hiding-place, and with a shrill and frightful scream, like the loud sound of a trumpet, charged down upon the huntsmen. So terrific was the animal's aspect that all instinctively sprung to their horses, and fled for life. The elephant, disregarding the others, singled out an unfortunate man (Cobus Klopper I think was his name,) who was the last person that had fired upon its comrade, and who was standing, with his horse's bridle over his arm, re-loading his huge gun at the moment the infuriated animal burst from the wood. Cobus also leaped hastily on horseback, but before he could seat himself in his saddle the elephant was upon him. One blow from his proboscis struck poor Cobus to the earth; and, without troubling himself about the horse, which galloped off in terror, he thrust his gigantic tusks through the man's body, and then, after stamping it flat with its ponderous feet, again seized it with his trunk, and flung it high into the air. Having thus wreaked vengeance upon his foes, he walked gently up to his consort, and affectionately caressing her, supported her wounded side with his shoulder, and, regardless of the vollies of balls with which the hunters, who had again rallied to the conflict, assailed them, he succeeded in conveying her from their reach into the impenetrable recesses of the forest.—One of my own friends, Lieut. John Moodie, of the Scotch Fusileers, now a settler in South Africa, had an almost miraculous escape on an occasion somewhat similar. He had gone out to an elephant hunt with a party of friends; and they had already succeeded in killing one or two of a small herd, and the rest were retreating before them towards their woody fastnesses, when one of the females having been separated from her young one among the bushes, forgot all regard to her own safety in mater-

4

nal anxiety, and turned back in wrath upon her pursuers to search for it. Mr Moodie, who happened to be on foot at the time, was the individual that the animal first caught sight of, and she instantly rushed upon him. To escape from an angry elephant in open ground is often difficult enough for a well-mounted horseman. My friend gave himself up for lost; nor would the activity of despair have availed him—the animal was close at his heels. But just at the moment when she was about to seize or strike him to the earth with her upraised proboscis, he fortunately stumbled and fell. The elephant, unable at once to arrest her impetuous career, made an attempt to thrust him through with her tusks as he lay on the ground before her, and actually tore up the earth within an inch or two of his body, and slightly bruised him with one of her huge feet as she passed over him. Before, however, she could turn back to destroy him, Mr Moodie contrived to scramble into the wood, and her young one at the same instant raising its cry for her in another direction, the dangerous animal went off without searching farther for him.—*Juvenile Keepsake.*

III. GENERAL SCIENCE.

12. *Volcano in Australasia.*—The crater of a volcano has been discovered in the vicinity of Segenhoe, and it has been increasing daily. Huge heaps of pitchy and adhesive mould lying around the mouth, crushing and tumbling in incessantly, after smothering the flame for a little, serves to render the combustion more fierce and rapid. Few of the natives will venture to "sit down" nearer than within a mile of the volcano.—*Australian*, October 30th, 1828.

13. *Account of an Earthquake in New South Wales.*—An earthquake has been recently experienced up the country. Several smart shocks were felt amongst some of the mountain ranges distributed over the district of Argyleshire, somewhere about 25 miles from Lake George. The concussion is represented to have lasted some minutes. It was preceded by the springing up of a gentle breeze from the S. W. quarter, which swiftly increased to the velocity of a hurricane tearing up whole trees by the roots, and scattering their branches through the air like chaff. While the hurricane raged with the utmost violence, the earth in various places became convulsed, heaving up into changing billowy ridges, yawning and closing, and splitting here and there into destructive chasms. Some few stock huts were partially demolished, and others shifted from their former foundations. One side of a cattle fence was altogether upturned, but, from the isolated nature of the country, there being but few other inhabitants than the solitary grazier, his men, and herds, and still fewer fixed habitations, the injury effected to the property was but trifling, and the convulsion was wholly sparing of life. After the combined elements had raged in this way for some minutes, their roar gradually diminished for about an hour, when it again increased with stunning bursts of thunder, torrents of rain, and blasts of vivid lightning. Men stood aghast, and the cattle ran cowering for shelter to the hills. The storm for the short time it continued, is represented as having been almost unprecedented in violence.—*Sydney Gazette.*

ART. XXV.—LIST OF PATENTS GRANTED IN SCOTLAND SINCE MAY 20, 1829.

10. May 20. For certain Improvements in making, constructing, or manufacturing Cartridges for Sporting and other purposes. To JOHN DICKEN WHITEHEAD, county of York.

11. May 20. For certain Improvements in the machinery to be employed in making Nails, Brads, and Screws. To THOMAS TYNDALL, county of Warwick.

12. June 5. For Improvements in Evaporating Sugar. To WILLIAM GODFREY KNELLER, county of Middlesex.

13. July 1. For certain Improvements in machinery or apparatus for Propelling Ships or other Vessels on Water, &c. To ORLANDO HARRIS WILLIAMS, Esq. county of Gloucester.

14. July 1. For a Mode or Method of Converting Liquids into Vapour or Steam. To JOHN BRAITHWAITE and JOHN ERICSSON, county of Middlesex.

ART. XXVI.—CELESTIAL PHENOMENA,

From October 1st, 1829, to January 1st, 1830. Adapted to the Meridian of Greenwich, Apparent Time, excepting the Eclipses of Jupiter's Satellites, which are given in Mean Time.

N. B.—The day begins at noon, and the conjunctions of the Moon and Stars are given in Right Ascension.

OCTOBER.

D.	H.	M.	S.	
1	2			♀ ☌ 2 α ♎
1	14	26	30	☽ ☌ γ ♎ ☽ 7′ S.
2	16	43	35	☽ ☌ φ Oph. ☽ 20′ S.
2	15			♂ ☌ β ♍
5				☿ Greatest Elong.
5	11	49		☽ First Quarter.
6	21	31	50	☽ ☌ β ♍ ☽ 11′ S.
8	22	23	16	☽ ☌ θ ♒ ☽ 17′ S.
11	14			♀ ☌ κ ♎
12				♅ Stationary.
12	3	29		○ Full Moon.
13	21			♀ ☌ λ ♎
14	5	55	7	Em. I. Sat. ♃
15	4	5	32	☽ ☌ γ ♉ ☽ 60′ N.
15	5	19	55	☽ ☌ 1 θ ♉ ☽ 49′ S.
15	5	48	41	☽ ☌ 2 θ ♉ ☽ 41′ S.
15	5			♂ ☌ κ ♍
15	6			♀ ☌ θ ♏
*15	10	42	30	☽ Aldeb. Vis. Occul.†
17				☿ Stationary.
19	2	30		☾ Last Quarter.
23	4	29		☉ enters ♏
24	1	12	44	☽ ☌ β ♍ ☽ 64′ S.
25	5			☽ ☌ ♂
25	17	30		♅ □ ☉
27	7	44		● New Moon.
28	13			♀ Inf. ☌ ☉
*29	4	58	28	☽ ☌ θ ♎ ☽ 59′ N.

NOVEMBER.

D.	H.	M.	S.	
1	4			♀ ☌ B. Oph.
3	4	3	7	☽ ☌ β ♑ ☽ 20′ S.
3	21	51		☽ First Quarter.
5	6	30	11	☽ ☌ θ ♒ ☽ 24′ S.
5	8			♂ ☌ θ ♍
6				☿ Stationary.
10	0	30		♄ □ ☉
10	13	46		○ Full Moon.
11	14	49	12	☽ ☌ γ ♉ ☽ 65′ N.
11	16	2	27	☽ ☌ 1 θ ♉ ☽ 44′ S.
11	16	30	47	☽ ☌ 2 θ ♉ ☽ 36′ S.
11	21	19	46	☽ Aldeb. ☽ 40′ N.
12	22			♀ λ ♐
13	17			☿ κ ♍
14				☿ Greatest Elong.
17	20	51		☾ Last Quarter.
18	4			♀ ☌ σ ♐
18	23			♓ ☌ B. Oph.
20	8	12	44	☽ ☌ β ♍ ☽ 55′ S.
22	0	53		☉ enters ♐
22	5			♀ ☌ ⚵ ♐
23	10	48	39	☽ ☌ κ ♍ ☽ 1′ N.
26	0	32		● New Moon.
28	21			☿ ☌ 4 ζ ♎
29				♄ Stationary.
30	7			☿ ☌ κ ♎
30	9	13	40	☽ ☌ β ♑ ☽ 33′ S.

† See page 366.

DECEMBER.

D.	H.	M.	S.	
2	2			☿ ☌ λ ♎
2	12	3	57	☽ ☌ θ ♒ ☽ 39′ S.
3	6	33		☽ First Quarter.
3	9			♂ ☌ λ ♍
4	2			☿ ☌ 1 β ♏
4	2			☿ ☌ 2 β ♏
* 9	7	17	52	☽ ☌ Ald. Vis. Occul.†
10	1	38		○ Full Moon.
*14	16	18	38	☽ ☌ ξ ♌ ☽ 42′ N.
*14	20	51	19	☽ ☌ • ♌ ☽ 19′ N.
15	15			☿ ☌ θ Oph.
15	19			♂ ☌ 2 α ♎
15	21			♀ ☌ θ ♑
16	7			☿ B. Oph.
17	18	4		☾ Last Quarter.
18	2	30		♃ ☌ ☉
18	7	27	59	☽ ☌ ν ♍ 38′ S.
19	10	3	51	☽ ☌ θ ♍ 10′ N.
20	13	19		☉ enters ♑
22	11	13	12	☽ γ ♎ ☽ 5′ S.
*22	19	46	15	☽ θ ♎ ☽ 63′ N.
22	12	55	59	☽ φ Oph. ☽ 26′ S.
23	23			♀ ☌ γ ♑
24	13	15		☿ Sup. ☌ ☉
25	15	36		● New Moon.
25	17			♀ ☌ δ ♑
26				♀ Greatest Elong.
27	15	27	11	☽ β ♑ ☽ 43′ S.
29	17	13	56	☽ θ ♒ ☽ 55′ S.
31	18			♀ ☌ ψ ♐

Times of the Planets passing the Meridian.

OCTOBER.

	Mercury.		Venus.		Mars.		Jupiter.		Saturn.		Georgian.	
D.	h	′	h.	′	h	′	h	′	h	′	h	′
1	1	30	2	12	23	7	4	6	20	39	7	49
7	1	31	2	18	22	59	3	48	20	20	7	27
13	1	25	2	26	22	51	3	30	20	0	7	5
19	1	6	2	33	22	43	3	13	19	39	6	42
25	0	28	2	41	22	34	2	55	19	18	6	20

NOVEMBER.

1	23	25	2	50	22	24	2	34	18	52	5	54
7	22	56	2	58	22	14	2	15	18	29	5	30
13	22	47	3	4	22	4	1	57	18	6	5	7
19	22	50	3	11	21	54	1	37	18	42	4	43
25	22	58	3	16	21	44	1	18	18	17	4	19

DECEMBER.

1	23	9	3	19	21	33	0	58	16	51	3	54
7	23	21	3	22	21	22	0	38	16	27	3	29
13	23	34	3	22	21	11	0	17	16	58	3	4
19	23	49	3	22	21	0	23	53	15	31	2	39
25	0	2	3	19	20	49	23	33	15	3	2	14

Declination of the Planets

OCTOBER.

	Mercury.	Venus.	Mars.	Jupiter.	Saturn.	Georgian.
D.	° ′	° ′	° ′	° ′	° ′	° ′
1	14 52 S.	16 20 S.	3 34 N.	21 39 S.	16 59 N.	20 14 S.
7	17 25	18 43	2 1	21 48	16 50	20 14
13	18 56	20 50	0 28 N.	21 57	16 42	20 14
19	18 52	22 37	1 5 S.	22 6	16 35	20 13
25	16 25	24 3	2 38	22 15	16 28	20 13

NOVEMBER.

1	11 24 S.	25 13 S.	4 26 S.	22 25 S.	16 22 N.	20 11 S.
7	9 3	25 47	5 57	22 33	16 19	20 9
13	9 51	25 54	7 28	22 41	16 16	20 7
19	12 25	25 35	8 56	22 48	16 14	20 5
25	15 32	25 51	10 22	22 54	16 14	20 3

† See page 366.

DECEMBER.

D.	°	′	°	′	°	′	°	′		′	°	′
1	18	33 S.	23	44 S.	11	46 S.	22	59 S.	16	15 N.	19	58 S.
7	21	9	22	14	13	7	23	4	16	17	19	53
13	23	11	20	26	14	25	23	8	16	20	19	50
19	24	31	18	21	15	39	23	11	16	25	19	47
25	25	4	16	3	16	50	23	13	16	30	19	42

The preceding numbers will enable any person to find the positions of the planets, to lay them down upon a celestial globe, and to determine their times of rising and setting.

ART. XXVII.—*Summary of Meteorological Observations made at Kendal in June, July, and August* 1829. By MR SAMUEL MARSHALL. Communicated by the Author.

State of the Barometer, Thermometer, &c. in Kendal for June 1829.

June.

Barometer.	Inches.
Maximum on the 10th,	30.26
Minimum on the 28th,	29.44
Mean height,	29.85

Thermometer.	
Maximum on the 12th,	73°
Minimum on the 7th,	37.5°
Mean height,	57.78°

Quantity of rain, 4.204 inches.
Number of rainy days, 17.
Prevalent wind, south-west.

The barometer has varied less than usual in this month, and the mean is greater than in the corresponding month of last year. The greatest height of the thermometer in June last year was 81.5°, but in this year it has not been higher than 73°. In short, this month has not been so hot and sultry as June was in the last, and may be pronounced a very unsettled one. We have had 17 days on which rain has fallen, but, excepting in three instances, in small quantities. From the 12th to the end of the month we have had but 3 days on which rain has not fallen, and on the 16th, 1.383 inch of rain was measured. There have been 3 days on which we had thunder, but chiefly at some distance, and accompanied with very little rain. The total quantity of rain for the first half of this year amounts to 12.540 inches, being 9.768 inches less than in the first six months of last year.

July.

Barometer.	Inches.
Maximum on the 21st,	29.97
Minimum on the 2d,	29.16
Mean height,	29.59

Thermometer.

Maximum on the 25th, - - -	72°
Minimum on the 27th, - - - -	43°
Mean height, - - -	57.96°

Quantity of rain, 5.569 inches.
Number of rainy days, 21.
Prevalent wind, west.

There have been 21 days in this month on which we have had rain, though in several instances too small a quantity in the course of the day to be measured by the gauge. The weather has been very unsettled through the greater part of the month, and the hay harvest has, in consequence, been much retarded, and rendered difficult to proceed with. The barometer has varied very little during the month, and the temperature has never exceeded 72°, and from the prevalence of winds from the E. S. E., and N. E., in several parts of the month, the weather has been frequently cold for the season. There was a most beautiful display of the Aurora Borealis on the evening of the 25th. We have had two thunder storms, one on the 11th and another on the 24th; since the latter, the weather has been more settled than in any previous portion of the month.

August.

Barometer.	Inches.
Maximum on the 30th, - -	30.07
Minimum on the 27th, - -	29.10
Mean height, - - - -	29.69

Thermometer.

Maximum on the 8th, - -	68°
Minimum on the 30th, - - -	43°
Mean height, - - - -	55.78°

Quantity of rain, 9.383 inches.
Number of rainy days, 20.
Prevalent wind, west.

The barometer has fluctuated frequently, though the range for the month is not great. The weather has been very changeable, and the quantity of rain has greatly exceeded that of any other month in the year. The wetness of the season has been very unfavourable for the harvest. The last 3 days have been fine. The rain has frequently fallen in large quantities. On the 3d, 1.087 inch of rain was measured; on the 14th 1.188; and on the 23d, 1.235, which had fallen in the preceding 24 hours. In consequence of these sudden and heavy rains we have had higher and more frequent floods than are common at this season of the year. The wind has been frequently very changeable, and the changes have been sudden. That from the west prevailed 10 days, from the N. 9, S. W. 8, N. W. 2, E. 1, and S. 1; but the currents of air frequently varied so much as to render it difficult to decide from which quarter the wind prevailed most. On the 13th, we had a storm amounting almost to a hurricane, and on the 28th the wind resembled the equinoctial gales.

Art. XXVIII.—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 1½ mile to the south of Edinburgh Castle, about 3 miles from the sea at Leith, and about ¼ 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.

JUNE 1829.

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.
M.	1	60	55	56.5	51	61	57.5	30.00	29.98	
T.	2	59	56	57.5	52	60	56	29.97	29.86	
W.	3	63	48	55.5	57	68	62.5	29.75	29.83	
T.	4	55	45	50	43	58	50.5	29.72	29.72	.01
F.	5	55	39	47	39	59	49	29.86	30.05	
S.	6	51	41	46	36	56	46	30.12	30.14	
S.	7	58	53	55.5	48	64	56	30.17	30.14	
M.	8	63	55	59	52	65	58.5	30.22	30.26	
T.	9	65	51	58	52	69	60.5	30.22	30.29	
W.	10	62	50	56	48	67	57.5	30.26	30.21	
T.	11	63	56	59.5	47	71	59	30.21	30.13	
F.	12	68	57	62.5	54	71	62.5	30.10	30.12	.11
S.	13	63	58	60.5	52	64	58	29.98	29.90	
S.	14	65	59	62	52	67	59.5	29.87	29.63	
M	15	65	50	57.5	54	65	59.5	29.63	29.39	.29
T.	16	57	47	52	46	59	52.5	29.34	29.53	
W.	17	56	48	52	39	62	50.5	29.61	29.68	
T.	18	58	44	53.5	45	62	53.5	29.75	29.69	.02
F.	19	58	55	56.5	45	58	51.5	29.64	29.56	.05
S.	20	70	51	60 5	52	72	62	29.62	29.68	
S.	21	54	51	52.5	47	63	55	29.61	29.60	.18
M.	22	67	56	61.5	48	69	58.5	29.54	29.56	.45
T.	23	69	60	64.5	50	70	60	29.61	29.73	
W.	24	63	59	61	50	65	57.5	29.80	29.83	.16
T.	25	63	55	59	55	67	C1	29.83	29.73	.51
F.	26	54	55	54.5	51	59	55	29.62	29 48	.21
S.	27	65	56	60.5	52	68	60	29.55	29.53	.04
S.	28	55	51	53	50	55	52.5	29.48	29.52	
M.	29	55	56	55.5	51	57	54	29.53	29.50	
T.	30	60	54	57	47	62	54.5	29.49	29.41	
	Sum,	1819	1574	1696.5	1465	1916	16905	894.10	893.68	2.03
	Mean.	60.6	52.5	56.5	48.8	63.8	56.3	29.83	29.79	

JULY 1829.

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.
W.	1	55	60	57.5	47	60	53.5	29.20	28.87	.23
T.	2	61	53	57	52	65	58.5	28.98	29.13	
F.	3	60	55	57.5	50	68	59	29.23	29.23	
S.	4	55	51	53	50	58	54	29.20	29.22	.85
S.	5	55	52	53.5	47	54	50.5	29.28	29.41	1.09
M.	6	59	51	55	48	62	55	29.48	29.50	.24
T.	7	61	50	55.5	47	64	55.5	29.32	29.38	
W.	8	56	50	53	46	58	52	29.42	29.48	.23
T.	9	55	47	51	46	60	53	29.51	29.56	
F.	10	59	55	57	42	59	50.5	29.48	29.26	.11
S.	11	63	57	60	52	68	60	29.22	29.16	.20
S.	12	60	53	56.5	50	60	55	29.08	29.21	.62
M.	13	64	60	62	47	75	61	29.34	29.37	
T.	14	64	59	61.5	59	68	63.5	29.42	29.41	
W.	15	63	57	60	54	65	59.5	29.46	29.51	
T.	16	62	54	58	57	66	61.5	29.55	29.55	
F.	17	62	55	58.5	50	66	58	29.50	29.31	
S.	18	62	56	59	53	65	59	29.29	29.40	.20
S.	19	64	55	59.5	50	65	57.5	29.54	29.58	.25
M.	20	61	53	57	48	65	56.5	29.70	29.83	
T.	21	59	62	60.5	46	62	54	29.84	29.18	.06
W.	22	66	56	61	58	69	63.5	29.66	29.75	
T.	23	63	55	59	52	65	58.5	29.76	29.88	
F.	24	62	50	56	50	64	57	29.85	29.81	.30
S.	25	54	48	50	52	57	54.5	29.84	29.93	
S.	26	56	56	56	43	62	52.5	29.91	29.92	
M.	27	68	56	62	49	63	56	29.91	29.83	
T.	28	62	56	59	50	63	56.5	29.76	29.76	.10
W.	29	60	55	57.5	50	62	56	29.61	29.63	
T.	30	59	52	55.5	49	60	55.5	29.75	29.84	
F.	31	60	54	57	48	64	56	29.94	29.98	
		1870	1681	1775.5	1542	1964	1753	915.01	915.38	4.48
		60.3	54.2	57.2	497	63.3	56.5	28.87	61.78	

AUGUST 1829.

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	64	55	59.5	44	68	56	29.96	29.97	
S.	2	63	57	60	52	68	60	29.92	29.82	
M.	3	63	49	56	47	63	55	29.38	29.28	1.80
T.	4	59	50	54.5	46	61	53.5	29.49	29.60	.15
W.	5	63	52	57.5	51	67	59	29.81	29.82	
T.	6	64	60	62	49	69	59	29.83	29.83	
F.	7	65	56	60.5	55	65	60	29.94	29.94	
S.	8	67	62	64.5	59	71	65	29.83	29.60	
S.	9	67	57	62	55	70	62.5	29.53	29.45	.09
M.	10	62	48	55	47	65	56	29.46	29.73	
T.	11	56	51	53.5	40	62	51	29.82	29.91	
W.	12	54	51	52.5	44	56	50	29.91	29.88	.53
T.	13	51	50	50.5	44	49	46.5	29.76	29.66	.17
F.	14	49	51	50	48	54	51	29.61	29.62	
S.	15	55	48	51.5	48	56	52	29.68	29.81	
S.	16	54	51	52.5	36	60	48	29.85	29.80	.03
M.	17	56	62	54	48	60	54	29.78	29.65	.04
T.	18	55	48	51.5	48	59	53.5	29.50	29.41	.92
W.	19	51	51	51	46	56	51	29.06	29.10	.25
T.	20	57	49	53	44	58	51	29.24	29.46	
F.	21	56	48	52	41	59	50	29.57	29.61	
S.	22	51	58	54.5	41	57	49	29.57	28.96	1.29
S.	23	60	53	56.5	51	62	56.5	28.96	28.93	.03
M.	24	54	51	52.5	48	58	53	29.06	29.21	
T.	25	55	47	51	44	59	51.5	29.44	29.71	
W.	26	57	54	55.5	42	61	51.5	29.63	29.12	1.00
T.	27	54	52	53	47	57	52	28.69	29.29	.39
F.	28	56	53	54.5	48	55	51.5	29.63	29.88	.04
S.	29	56	49	52.5	43	59	51	30.08	30.10	.04
S.	30	61	60	60.5	47	62	54	30.01	30.03	.05
M.	31	65	59	62	56	66	61	30.02	30.00	.18
		1800	1632	17.16	1459	1891	1675.5	917.82	919.18	6.80
		58	52.6	55.3	47	61	54	29.60	29.65	

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NEW SERIES.

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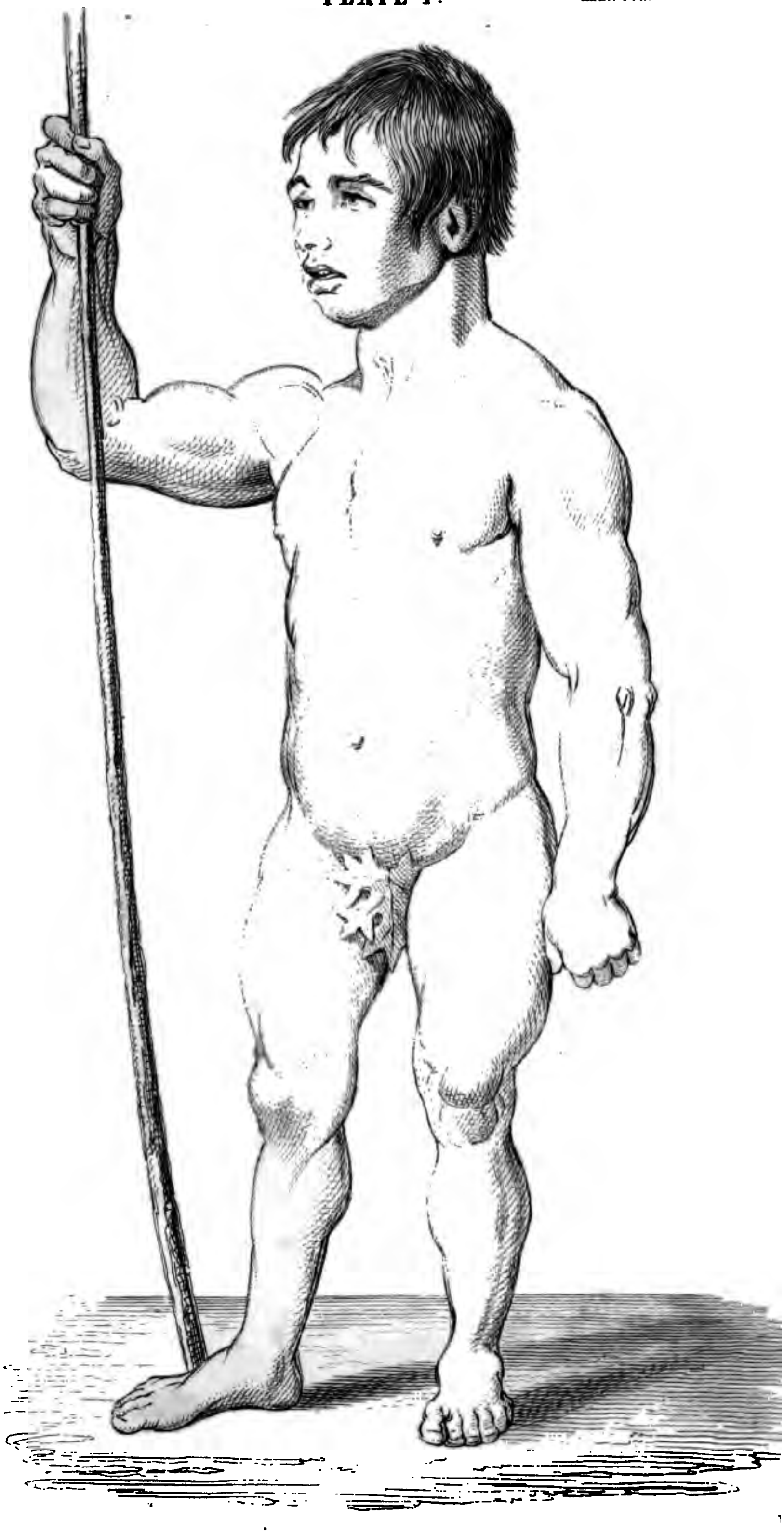
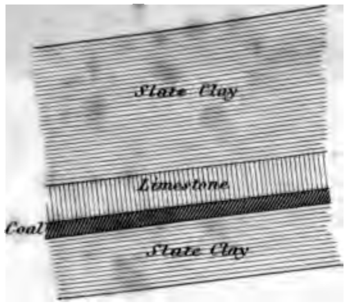


Fig. 2.



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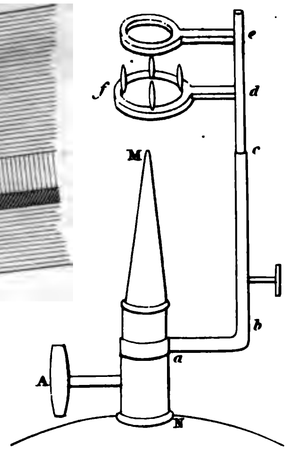

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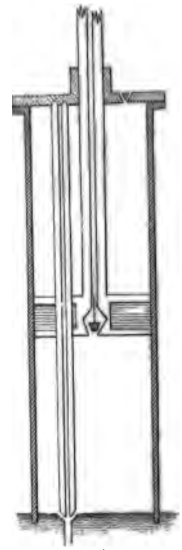

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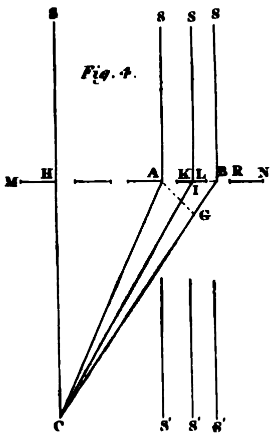

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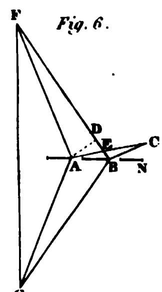

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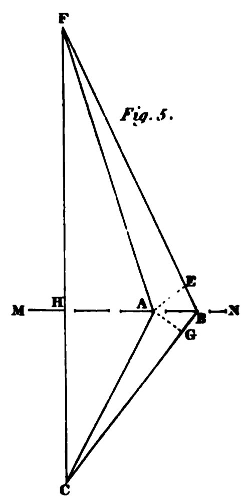

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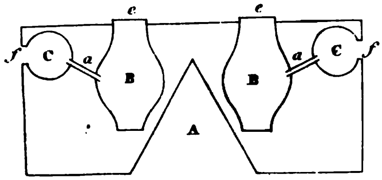


Fig. 7.

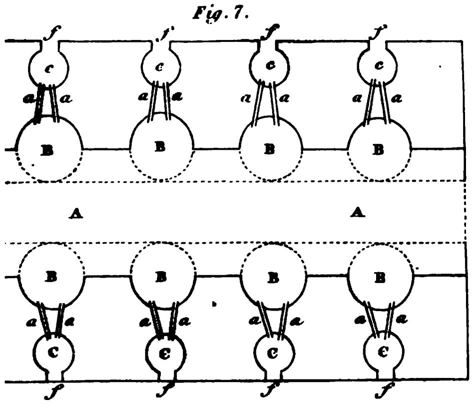


Fig. 9.

W.H.Lizars sc.

Fig. 3.

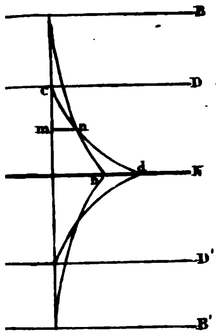
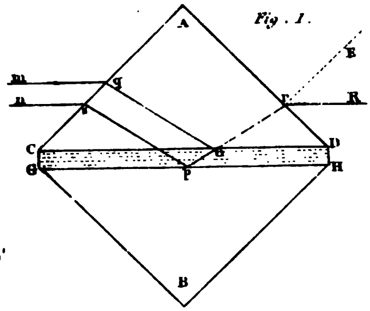


Fig. 1.



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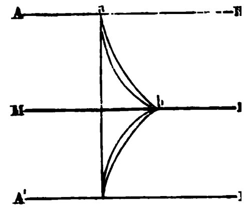

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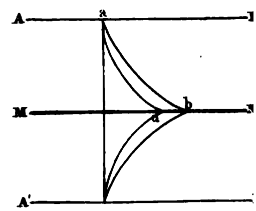


Fig. 2.

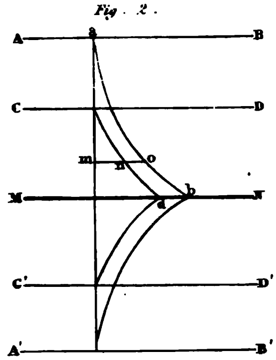


Fig. 4.

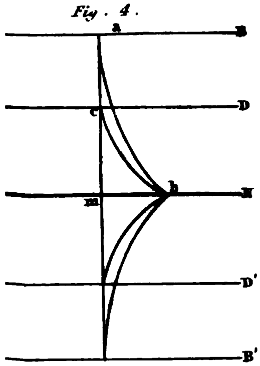


Fig. 7.

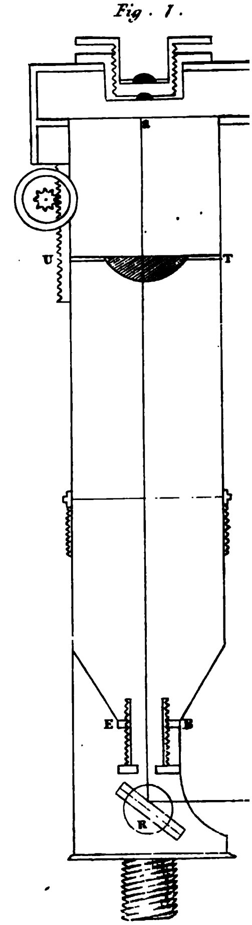


Fig. 8.

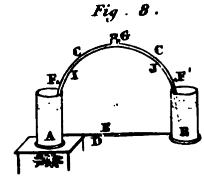


Fig. 10.

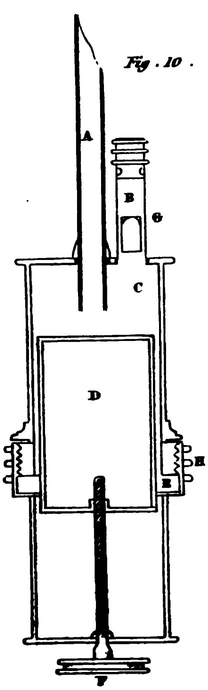
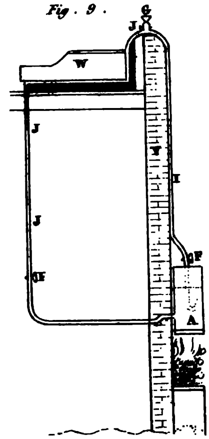


Fig. 9.



Lizars sculpt.

PLATE IV.