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THE EDINBURGH NEW PHILOSOPHICAL JOURNAL,

EXHIBITING A VIEW OF THE

PROGRESSIVE DISCOVERIES AND IMPROVEMENTS

IN THE

SCIENCES AND THE ARTS.

CONDUCTED BY

ROBERT JAMESON,

REGIUS PROFESSOR OF NATURAL HISTORY, LECTURER ON MINERALOGY, AND KEEPER OF THE MUSEUM IN THE UNIVERSITY OF EDINBURGH.

Fellow of the Royal Societies of London and Edinburgh; Honorary Member of the Royal Irish Academy; of the Royal Society of Sciences of Denmark; of the Royal Academy of Sciences of Berlin; of the Royal Academy of Naples; of the Geological Society of France; Honorary Member of the Asiatic Society of Calcutta; Fellow of the Royal Linnean, and of the Geological Societies of London; of the Royal Geological Society of Cornwall, and of the Cambridge Philosophical Society; of the Antiquarian, Wernerian, Natural History, Royal Medical, Royal Physical, and Horticultural Societies of Edinburgh; of the Highland and Agricultural Society of Scotland; of the Antiquarian and Literary Society of Perth; of the Statistical Society of Glasgow; of the Royal Dublin Society; of the York, Bristol, Cambrian, Whitby, Northern, and Cork Institutions; of the Natural History Society of Northumberland, Durham, and Newcastle; of the Imperial Pharmaceutical Society of Petersburgh; of the Natural History Society of Wetterau; of the Mineralogical Society of Jena; of the Royal Mineralogical Society of Dresden; of the Natural History Society of Paris; of the Philomathic Society of Paris; of the Natural History Society of Calvados; of the Senkenberg Society of Natural History; of the Society of Natural Sciences and Medicine of Heidelberg; Honorary Member of the Literary and Philosophical Society of New York; of the New York Historical Society; of the American Antiquarian Society; of the Academy of Natural Sciences of Philadelphia; of the Lyceum of Natural History of New York; of the Natural History Society of Montreal; of the Franklin Institute of the State of Pennsylvania for the Promotion of the Mechanic Arts; of the Geological Society of Pennsylvania; of the Boston Society of Natural History of the United States; of the South African Institution of the Cape of Good Hope; Honorary Member of the Statistical Society of France; Member of the Entomological Society of Stettin, &c. &c. &c.

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ERRATA IN LAST NUMBER.

In Professor Agassiz' paper, at p. 83, for *Phacolepis*, read *Rhacolepis* in four instances.
In Professor Bischof's paper, at p. 147, line 11 from bottom, for *like* water, read *lime*-water.

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*Historical Eloge on M. Frédéric Cuvier.** By M. FLOURENS, Perpetual Secretary of the Academy of Sciences.†

AMONG the individuals to whom our *Eloges* are dedicated, there are some who, by their brilliant labours, have early acquired distinguished celebrity, and with regard to them our voice is, to a certain extent, nothing more than an echo of public gratitude; whilst there are others who, having devoted themselves to humbler pursuits, are hence less known. In these latter instances, however, our Eloges are not on that account the less useful. They point out sources of information of which we should probably have remained ignorant, and preserve the recollection of respectable names it would be unjust to forget. These considerations become peculiarly applicable when they refer to an individual who, like the one on whom I am about to dwell, has past thirty years of a laborious life in profound study; and has died leaving behind him rather scattered observations than systematized doctrines, and detached materials rather than a finished work.

Frederic Cuvier, member of the Academy of Sciences, of the Royal Society of London, and brother of George Cuvier, was born at Montbéliard on the 28th of June 1773. His brother was born on the 23d of August 1769, so that Frederic was the younger, and by nearly four years. As soon as he had passed the years of boyhood, he, like his brother, went to

* Read July 13. 1840.

† We are indebted to J. B. Pentland, Esq. for this early copy of the Eloge.—EDITOR.

the College of Montbéliard; but the period of the development of his genius had not as yet arrived; of these his early studies, he saw only the dry and uninviting side; and, finally, he abandoned them altogether, and became apprentice to a watchmaker. He had a strong bias towards mechanics; and his curiosity, which the ordinary studies of college had not excited, was roused to the highest pitch by an experiment in physics, or the examination of a new machine. It is not easy to conjecture the celebrity to which this bent might have raised him, had not more influential circumstances diverted his attention from mechanics and directed it to natural history.

These circumstances naturally sprang from his connection with his illustrious brother George, who, after the most brilliant career, first at the college of Montbéliard, and afterwards at the university of Stuttgard, and also, after three years spent at Fécamp, a small town in Normandy, " au milieu," to use his own expression, already quoted in his Eloge,* " des productions les plus variées que le mer et la terre semblaient lui offrir à l'envi," arrived in the year 1795 in Paris. He at this date published his memoir upon the distribution of *White-blooded animals;* and the following year his memoir on *Fossil Elephants.* By the first of these he effected a revolution in zoology; and in the second he announced his grand views concerning the animals which had been destroyed by the revolutions of our globe, and this at the age of twenty-six and twenty-seven; manifestations of a genius which astonished as much by its precocity as its splendour, and whose first two efforts, at the close of the eighteenth century, called into existence those two branches of natural history which most engage the attention of the naturalists of the nineteenth, viz. *palæontology*, and the *anatomy of the invertebrate animals;* so that of this man we may say what Fontenelle remarked of Newton, " Qu'il n'a pas été donné aux hommes de voir le Nil faible et naissant." How rapidly these great works raised Cuvier to the most honourable distinctions of science, is well known. The *Académie*, the *Jardin des Plantes*, and the *Col-*

* See Eloge Historique de Georges Cuvier: Mémoires de l'Académie des Sciences, t. xiv.

lège de France, were all anxious to press him into their service; and he on his part no sooner found himself settled in these honourable situations than he lost no time in taking under his kind protection both his father and brother, in other words, his whole family, for he had lost his mother in the year 1793.

Gladly accepting the kind invitation of his brother, M. F. Cuvier came to Paris towards the end of the year 1797. Here a new world opened to his view. He now discovered how much time he had lost, and undoubtedly with regret, but without discouragement. He immediately attended lectures upon natural philosophy, chemistry, and natural history; and, in the year 1801, co-operated with M. Biot in researches concerning the properties of the voltaic pile, which led to important results. In the year 1802, he undertook the principal management of the *Journal de la Société d'encouragement pour l'Industrie Nationale.*

Influential circumstances, however, as I have already hinted, speedily infused a new and more permanent energy into his plans. For it was impossible to live with Baron Cuvier, constantly and familiarly, in fraternal intimacy, without becoming a naturalist; and it was by undertaking a work for his brother that M. F. Cuvier made his first essay in natural history. The museum of comparative anatomy which had been commenced by Buffon and Daubenton, was at the time receiving immense accessions under the fostering care of M. Cuvier, so as to become, in fact, a new creation; for this the great naturalist required a catalogue, and for its preparation he applied to MM. F. Cuvier and Duvernoy.

M. F. Cuvier undertook the description of the skeletons; and this was the origin of his great work upon *Les dents des mammifères*, a work of fundamental importance in zoology. It is, in truth, a most complete study of *characters* derived from the teeth, and the most able application of these *characters* used in the formation of *genera.* By this skilful application, M. F. Cuvier produced a great change in many of the orders of the mammalia, especially in the *carnassiers* and *rodents;* and those changes he introduced are now adopted by nearly all zoologists.

But a labour of another kind, and, unquestionably, both the most important and original, with which M. F. Cuvier engaged, was his observations upon the instinct and intelligence of animals; observations which interested the philosopher not less than the naturalist, and to which he devoted thirty years of consecutive and conscientious study. In connection with this, it should be remarked, that, in the year 1804, M. F. Cuvier was intrusted with the immediate direction of the Menagerie of the museum of natural history; and no situation could possibly have been more favourable for the study of the faculties and the actions of animals. At the same time, no naturalist could have done more for this subject of inquiry than did M. F. Cuvier.

The ancients were in the habit of forming great collections of wild beasts; but it was only to exhibit them in the public games. When the menagerie was established at Versailles a different and nobler object was contemplated; for it was intended that the animals which were there congregated should subserve the purposes of science. It was from the menagerie of Versailles that Perrault and Duvernoy derived the first materials of that edifice of comparative anatomy, which, begun twenty centuries before by Aristotle, was by them recommenced upon a new foundation, and has been since raised to a lofty elevation by the successive labours of Daubenton, Camper, Vicq d'Azyr, and George Cuvier.

In the year 1794, when the menagerie of Versailles was transported to Paris, and connected with the *Jardin des Plantes*, it became still more useful. At that time three celebrated naturalists, George Cuvier, Lacépède, and M. Geoffroy St Hilaire, combined in publishing, under the title of *Menagerie du Muséum National*, the first work in which French naturalists exhibited their zeal to maintain in natural history the admirable method of Buffon, which, till then, had been imitated only by a single foreign naturalist, namely, the celebrated Pallas. Finally, when the menagerie of Versailles, now become the menagerie of the museum, was intrusted in the year 1804 to M. F. Cuvier, he speedily elicited from it new sources of usefulness. First of all, after the example of the three naturalists I have just named, he continued the positive history of species ; and this has been all along the main object of his great work *Histoire des Mammifères*, a work in which more than five hundred quadrupeds are represented and described with a minuteness and accuracy of which there is not perhaps a similar example in the records of zoology, at least in a work reaching to the same extent.

But I return to that novel species of useful information which M. F. Cuvier perceived might be derived from the menagerie of the museum of natural history ; I allude to that study of the faculties and actions of animals which was begun by Buffon ; was prosecuted by George Leroy, the ingenious author of the *Lettres Philosophiques sur les Animaux*, which were first published under the name of *A natural philosopher of Nuremberg ;* and has finally reached its present precision by the assiduous labours of M. F. Cuvier.

During more than a century, from the time of Descartes to Buffon,* the question of the intelligence of animals had been solely a question of pure metaphysics. It was with Buffon and George Leroy that it first became a positive inquiry of experience, and throughout it has so continued with M. F. Cuvier. Instructed by his former labours, and by his talent of observation, M. F. Cuvier devoted himself to the discovery of facts. He desiderated, however, such only as were clear and distinct, and those which were separated by precise limits. And this, in fact, supplies us with the most characteristic trait of the genius which directed his career,—he sought for facts and for limits. He sought for the limits which distinguished the intelligence of different species,—the limits which separate instinct from intelligence,—and the limits which separate the intelligence of man from that of the brutes. And these three limits once determined, every thing in this long agitated question concerning the intelligence of animals has assumed a new aspect. Descartes and Buffon would not concede intelligence of any kind to animals ; they were averse, and very properly, to assign to animals the intelligence of man, and did not perceive the limit which separates his intelligence

* That is to say, the time between the *Discours sur la méthode*, published in 1637, and the *Discours sur la Nature des Animaux*, published in 1753.

from that of the brute. On the other hand, Condillac and George Leroy conceded to animals intellectual processes of the highest order; these concessions they grounded upon certain observed actions, which, if they belonged to intelligence at all, certainly required these processes; and they did not, any more than the others, perceive the limit which separates instinct from intelligence.

The first result* of the observations of M. F. Cuvier determines the limits of intelligence in the different species of animals. Buffon had already supplied a general idea of a graduated scale of the *internal faculties* of animals, but he considered only the principal divisions of the animal series. M. F. Cuvier went much farther. Even in the class of mammals he perceived that intelligence rose and augmented from one order to another,—from the *Rodents* to the *Ruminants*,—from the *Ruminants* to the *Pachydermata*, and from these last to the *Carnassiers* and the *Quadrumana*. Of all animals the one which shews the most intelligence is the *Orang-outang*. The one studied by M. F. Cuvier delighted to climb trees; and if any person manifested an intention to mount the tree he had ascended for the purpose of catching him, he immediately shook it with all his might for the purpose of alarming the individual who was approaching him. If he were shut into a room, he soon opened the door; and if he could not reach the lock, for he was quite young, he stood on a chair for the purpose of reaching it. Finally, when he was refused any thing he was anxious to obtain, he knocked his head upon the ground, and actually injured himself, that he might excite the greater interest and compassion. This is precisely what man himself does when a child; it is what no other animal does but the *Orang-outang;* he is the only and solitary exception.

But we have to add something still more remarkable. It is that the intelligence of the orang-outang, this intelligence which is so much and so early developed, decreases with his age. The orang-outang, when young, astonishes us by his

* For the development of these results, I must refer to my *Résumé analytique des observations de M. F. Cuvier sur l'instinct et l'intelligence des animaux.* (Journal des Savants. Année 1839). I can here do little more than enumerate them.

penetration, his cunning, his address ; but when he reaches his adult state, he is nothing else but an animal, gross, brutal, and untractable. And it is the same with all *Monkeys* as with the orang-outang. In the whole of them intelligence diminishes in proportion as their strength increases. An animal, considered then as a perfectible being, has a limit marked not only according to his kind, but also as to his individuality. The animal of the greatest intelligence is possessed of this intelligence only in its younger years.

After having fixed the limits which distinguish the intelligence of the different species, M. F. Cuvier investigated the limit which separates instinct from intelligence : and it was particularly in respect to the beaver that he prosecuted his observations on this point.

The beaver is a mammal of the order *Rodentia*, that is to say, of that order which, as we have seen, has the smallest share of intelligence ; but he has a wonderful amount of instinct, whereby he constructs a hut, builds it in water, guards it with dykes, and supplies it with causeways, and all this with an industry which would undoubtedly lead us to assign a very superior degree of intelligence to this animal, if that industry depended upon intelligence. The important matter was to prove that it had no such dependence, and this M. F. Cuvier accomplished. He took beavers while still very young, and reared them far from their parents, where of course they could receive no instruction from them ; and these beavers, thus isolated and solitary, confined to a cage, for the very purpose that they might have no occasion to build, notwithstanding did build, impelled by a mechanical and blind force ; in a word, by a pure instinct.

The most complete opposition distinguishes *instinct* from *intelligence*. In *instinct* all is blind, necessary, unchanging ; in *intelligence* all is elective, conditional, and modifiable. The beaver who constructs his hut, and the bird which makes its nest, act solely by instinct. The horse and dog, which readily learn the meaning of many of our words, and which obey us, do so in virtue of intelligence. Every thing in *instinct* is innate ; the beaver constructs without having been taught : all is ordained ; the beaver builds overpowered by a force which is constant and irresistible. Every thing in *intelligence* results from experience and instruction: the dog obeys only because he has been taught; and all is free, for the dog obeys, because he so wills it. Finally, in *instinct* every thing is particular: the industry we so much admire in the beaver while constructing his hut, can be exercised by him only in building his hut: whilst in *intelligence* every thing is general; for this same flexibility of attention and conception which the dog exercises in obeying, he can exercise in the performance of any other task.

There is therefore in animals two distinct and primitive powers—*instinct* and *intelligence.* So long as these two powers continued to be confounded, every thing in the actions of animals was obscure and contradictory. Among these actions some exhibited man as in every respect superior to the brute, and others again appeared to lead to the very opposite conclusion. This was a contradiction as deplorable as it was absurd. By the distinction which separates blind and necessary actions from those which are elective and conditional, or, in a single word, *instinct* from *intelligence*, all contradiction ceases, and order takes the place of confusion; whatever, in animals, belongs to *intelligence*, in no respect approximates to the intelligence of man; and whatever in them, passing for *intelligence*, appears superior to that of man, is nothing more than the effect f a mechanical and blind power.

It now only remains to fix the limit which separates the intelligence of man from that of brutes. Here the conceptions of M. F. Cuvier became exalted, and withal appear to be no less certain.

Animals, by means of their senses, receive impressions similar to those which we receive through ours, and, like us, they preserve the traces of these impressions; these preserved impressions form in their understanding, as in ours, numerous and varied associations; they combine them, they draw inferences from them, they deduce conclusions from them,—in short they have intelligence. But this is the limit of their intelligence. The intelligence which they possess does not ponder concerning itself, nor perceive, nor know itself. In short, they are destitute of *reflection*, that supreme faculty, the endowment of the human mind, which retires within itself, and studies its own workings.

Reflection, then, thus defined, is the limit which separates the intelligence of man from that of animals. And all must in truth agree that here, in fact, is the line of a very distinct demarcation. This thought which reflects upon itself,—this intelligence which perceives its own existence, and makes itself the subject of study,—this knowledge which knows itself, these, one and all, evidently form an order of determinate phenomena of a distinct nature, and to which no animal can attain. Here we find, so to speak, a world which is purely intellectual, and this world belongs to man alone. In a word, animals feel, know, think; and to man alone, of all terrestrial beings, has it been given to feel that he feels, to know that he knows, and to think that he thinks.

Being able on this occasion to dwell only upon the principal results which follow from the observations of M. F. Cuvier, I hasten to the most novel and important facts, and these I can introduce only in a manner the most rapid and succinct. The influence of the senses upon the intelligence has been very much exaggerated. Helvetius went the length of saying that man owes his superiority over the brutes only to his hands. M. F. Cuvier demonstrates, from the example of the seal, that, even among animals, it is not the *external senses*, but an organ much more profound and internal, in fact the brain, on which the development of intelligence is dependent. The senses of the seal (sight, taste, smell, hearing) are all very imperfect; instead of hands it has but fins, and notwithstanding, in comparison with the other mammalia, it is largely endowed with intelligence.

It is well known how much Buffon has declaimed about the magnanimity of the lion, on his pride and courage, and upon the violence of the tiger, his insatiable cruelty, and his blind ferocity. But in spite of all that Buffon has said, M. F. Cuvier has always observed the same character in these two animals; that both are alike susceptible of affection and gratitude, and alike terrible in their rage. Helvetius, a philosopher, sought for a principle, and he announced one, which was the result of a forced generalization. Buffon, an author, describes in animals all the phases of the passions of man. The simple observation of M. F. Cuvier has supplied us with the fact such as it really is, and has placed it on the only solid basis of science.

All naturalists, the predecessors of M. F. Cuvier, had recognised, in the *domestication of animals*, nothing more than a very general result of that power which man exercises over the beasts. But he has shewn that the *domestication of animals*, that fact so exceedingly important in the history of our race, is dependent on a very peculiar circumstance, viz. on their *sociability*. There has not, in fact, a single species become domestic which, naturally, does not live in society and in troops ; and, of the numerous solitary species which it would unquestionably be man's interest to associate with himself, there is not a single one which has been *domesticated*. Man, therefore, in forcing animals to obey him, does not change their *natural condition*, as Buffon has affirmed ; on the contrary, all that he does is to avail himself of their *natural character*. In other words, man found animals *sociable*, and he made them *domestic*.

And even here I must take the liberty of pointing out a trait which characterizes far better than any words I can use, the kind of sagacity which was peculiar to M. F. Cuvier, and which was so useful to him in the delicate analysis of the complicated phenomena which were the subject of his studies. I allude to the distinction which he established, a distinction at once so true and so profound, between a *domesticated* animal and a *tame* one. Man may tame species even the most solitary and the most savage. Thus he tames the bear, the lion, the tiger, and, notwithstanding this, not one of these solitary species has ever been found to supply a *domesticated race*. The domestication, therefore, of an animal, is simply a consequence of its very nature, and of that which is most inherent in its nature—its instinct.

From all this it may readily be conceived, and that without prompting, how much these enlightened views of the primary causes of the *domestication of animals* have a bearing on matters of practical utility, and especially if applied to agriculture and domestic economy. M. F. Cuvier believed that many new species might readily be added to the domestic races we now possess; and more especially the Hottentot *Dauw* and the *Dziggüetai* among the Solipedes, the *Tapir* among the Pachydermata, and the *Lama Vigogne* among the Ruminants. I only throw out these hints to men so enlightened as those I now address, who are alive to the public welfare, who feel that agriculture has been too much neglected among us, and who are beginning to perceive that one of our most urgent agricultural wants, if not the first of all, is to have stronger and more varied domestic races amongst us.

M. F. Cuvier had been appointed in the year 1810 to the office of Inspector of the Academy of Paris, and in 1831 he became Inspector-General of the University. In the discharge of this office he carried along with him the same integrity of an honourable man, the same comprehensive views, and the same habits of practical reflection, of which he has left an admirable proof in his elegant work concerning "The Teaching of Natural History in our Colleges"—*L'Enseignement de l'Histoire Naturelle dans nos Colléges.*

Rollin, that able man who had meditated so much upon the instruction of youth, and whose memory cannot be too fondly cherished, proposed, as early as the commencement of the last century, to introduce Natural History into the curriculum of our colleges. He wished to engage children in the study of those phenomena "about which," he remarked, "their astonishment will be excited in the same proportion as their understandings are enlightened." Soon after the work of Pluche appeared. This was the first fruit of Rollin's thoughts, and perhaps the only result; for, before we can see natural history pervade public instruction, at least in a way that will be at all general, central schools must first be established. At present, however, the natural history which is introduced into our schools, is natural history with whatsoever it contains of the great and the difficult; it is natural history with its learned nomenclatures, and its abstract methodical classifications. But, as M. F. Cuvier has well remarked, first of all, our present colleges, even in their highest classes, do not at all correspond to the central schools, and then, this teaching according to the scientific method, which is so useful for advanced classes, is evidently but little suitable to the inferior ones. It is necessary, therefore, after more than a century of gropings and experiments, to return to the suggestion of Rollin, who wished to have *two Natural histories*, one for the upper classes, and another for the lower. In a word, we require to proportion the studies to the ages of the learners. But the art of proportioning is that of graduating and subordinating; and this graduation and subordination of studies is perhaps all that is at present necessary as it respects the teaching of the natural sciences, and the only means, at all events, by which we can banish those two vices inseparable from every system which is not accurately arranged, I mean many repetitions and many voids.

I have thus traced the labours of M. F. Cuvier, and these labours have made us acquainted with his whole life. Never, in fact, could it be said with greater truth of any one, that the life of a scientific man consists of his reflections and his works. Appointed in 1804 the Keeper of the Menagerie of the Museum, he spent thirty-four years in this peaceable retreat, where he found those two circumstances without which there is no profound labour—time and meditation. It was here that he prepared, without noise or parade, but also without distraction or relaxation, the basis of that study of the faculties and actions of animals which, previous to his time, was scarcely regarded as a portion of natural history, and out of which he has formed a distinct and profound science, a science which, subjecting to experiment questions which had hitherto been reputed purely philosophical, has really extended the domains of observation.

M. F. Cuvier exhibited in society an easy temper, great tact in the intercourse of life, a rare kindness, and a benevolence which seemed to flow from sympathy, and which inspired it. His disposition, at once gentle and energetic, his elevated character, and his upright heart, have left indelible recollections in all those who were acquainted with him. His friends, whom I regret I cannot mention here, mourn for him now as acutely as the day when they lost him. His modesty especially had a peculiar charm. It was so natural that it appeared as if his merit was unknown to himself. It will readily be credited, that he never thought of his own glory, but was enthusiastic for his brother's. It was for this brother that he lived, and for him he became a naturalist. It was an interesting spectacle, and never failed to inspire general respect; on his part it was so complete and devoted, that of all the recollections which the name of M. F. Cuvier awakens, it is still perhaps the most prominent.

M. F. Cuvier was appointed, on the 24th of December 1837, Professor of the Museum of Natural History. The Chair to which he was called had just been instituted, and it was instituted for him. M. de Salvandy, at that time Minister of Public Instruction, thus nobly repaid, by a useful appointment, the debt which the Museum and Science had incurred.

But it was also at that very moment, at the time that he was finally about to engage in that duty which had always been the most cherished of his heart, viz. to teach the science which he had founded, and to which he had devoted his life, that he was attacked with that frightful malady by which he was cut off. He died at Strasburg on the 24th of July 1838. He was engaged, with his accustomed zeal, in the inspection of the colleges of that city, when, on the evening of the 19th, he felt the first stroke of that dreadful malady, which, four days afterwards, closed his eyes on all below.

It has been remarked, that there was a striking analogy between his malady and that of his illustrious brother. These two men, who had been so closely united, and so seldom separated, died at the same age, of the same disease, and with the same serenity of mind and intrepidity of soul. It was thus that M. F. Cuvier carefully marked, as his brother had done, the successive steps of his malady; he foresaw and announced them; he preserved in the solemn act of dying all his penetration as an observer, and all his fortitude as a man.

His son quitted Paris on the first intimation of his malady, but on his arrival at Strasburg, M. F. Cuvier had expired. He died in the arms of M. Dutrey, his fellow-traveller and colleague as Inspector, and of M. Duvernoy, his friend throughout life.

His last words were these,—"Let my son inscribe upon my tomb,—*Frederic Cuvier, brother of George Cuvier.*"

Thus he expressed his two most cherished affections, and in his last words associated the two most powerful sentiments of his soul,—his tenderness for his son,—his admiration for his brother.

Farther Reasons against the Chemical Theory of Volcanos. By Professor GUSTAV BISCHOF of Bonn. Communicated by the Author.

Professor Daubeny* has replied to my objections to the chemical theory of volcanos, with that fairness which might be expected from a scientific man, who only looks to the topic of the discussion, not to the opponent. Venturing to continue the discussion on this subject a little farther, I by no means wish to forget the merits of a naturalist, who has distinguished himself so much by his description of active and extinct volcanos, and by a variety of valuable memoirs on similar subjects. I feel happy to meet with such a man in the very same field of science, and I hope that the differences in our opinions will become less considerable, the more they are explained.

The chief question is, whether the internal heat of our globe is sufficient to account for volcanic phenomena? or, whether the supposition of intense chemical actions explains them in a more satisfactory manner? As for the internal heat of the earth, I am of opinion that it may not be considered as an hypothesis, but as a fact well grounded on numerous phenomena. Granting this, it must be imagined that the rocks of the earth are, at a certain depth, in a liquid ignited state. Indeed, it is well known that, by volcanic action, melted masses (lava) arise from the interior, and that, on slowly cooling, they produce rocks such as basalt, trachyte, &c., forming a considerable part of the external crust of the surface of the earth. The chief explanation of volcanic phenomena depends, therefore, according to the theory alluded to, upon demonstrating the means which nature makes use of to bring such melted masses to the surface of the earth. In my

* Jameson's Philosophical Journal, vol. xxvi. p. 291.

former publications in this Journal concerning the natural history of volcanos and earthquakes, I flatter myself that I have shewn, as far as it is possible to elucidate a subject which is beyond the limits of direct observation, and which can for the most part only be mentally appreciated, that watery vapours may be supposed to be the means by which nature is able to carry melted masses from the greatest depths to the surface of the earth. Any hypothesis endeavouring to explain the volcanic action, must embrace watery vapours or other elastic fluids, in order to account for the rising up of lava. The theory in question, consequently, reduces the explanation of the phenomena presented to the most simple point.

Let us now consider, somewhat minutely, the theory which ascribes volcanic actions to chemical processes taking place between bodies having a powerful affinity for each other, and by which so considerable a heat is produced as to melt lava. First, the advocates of this hypothesis must be asked, whether they are inclined to deny a high internal heat of the globe increasing with the depth to the point of melted lava, or whether they suppose chemical changes supported by this intense temperature? Admitting for an instant the former case, all the heat required to melt lavas must be produced by these actions. Professor Daubeny seems to be inclined to suppose this; for, to my objections that these metals are not sufficiently oxidizable to kindle on the access of water, he replies, that silicon is combustible when united either with a little hydrogen or with alkaline carbonates, and that calcium and magnesium appear to be still more inflammable. The Professor seems, therefore, not to consider the internal heat as a cooperative power, but to think it possible that the metals, even at a common temperature, generate heat enough, by combining with oxygen, to melt the produced earths.

It is scarcely possible to confirm or to refute this assertion in an experimental way. Indeed, the combustion of a mixture of the earthy and alkaline metals, in such proportions as are contained in lava, would give a satisfactory conclusion only by applying these metals in such large quantities, that the heat absorbed by surrounding bodies should be insignificant in proportion to that which is generated. Supposing, on the other hand, that the advocates of the hypothesis presented, allow the existence of a central heat supporting chemical actions, it would not be difficult to conceive the melting of lavas.

I shall not pass from this subject without adding, that this hypothesis does not necessarily require the volcanic actions at very considerable depths below the surface of the earth, but merely where the earthy and alkaline metals were present. This circumstance may perhaps be viewed as more favourable to the hypothesis in question than to the other, which assumes a central heat, and consequently considerable depths, for volcanic actions. But are there grounds for supposing that the place where these actions are developed is near the surface of the earth? On the contrary, all volcanic phenomena, both the eruptions themselves and earthquakes, afford decided proofs that volcanic actions are peculiar to a spot very deep below the surface. As to this subject I refer to my former publications.

The theory of Professor Daubeny supposes the existence, near and about the focus of a volcano, of vast caverns, caused originally by the heaving up of the softened rocks, owing to the elastic vapours disengaged, and consequently filled in the first instance by these matters. Granting the passages between these caverns and the external atmosphere not to be hermetically sealed during the state of rest of the volcano, air must enter into them. But it is obvious that, on the beginning of the volcanic action, particularly on the heaving up of the lava, those passages must be completely interrupted; therefore, only that air which formerly entered the cavities can act on the earthy and alkaline metals supposed to be in their vacuity. This effect will consequently be limited, and finished as soon as the oxygen of this air is consumed. In order to gain a distinct notion of the quantity of air requisite to produce a certain quantity of lava by oxidation of the earthy and alkaline metals, let us make the following calculation: Let us take, like Professor Daubeny, for instance, the analysis of the lava from *Etna* given by Dr Kennedy. According to the calculation given by the former, 58 parts of the metallic bases of this lava require 42 parts of oxygen to give 100 parts of lava. 100 parts of atmospheric air, containing 23.32 parts of oxygen by weight, 100 parts of lava will require 180 parts of air by weight to be produced. Lava being 3.2 times heavier than water, and water being 770 times heavier than atmospheric air, lava is consequently 2464 times heavier than the latter. Therefore, the production of 1 volume of lava will require 4435 volumes of air. In my former papers, I have mentioned that the violent eruption in the low country of *Skaptar Jokul* in *Iceland*, in 1783, brought up a mass of lava so considerable as to surpass in magnitude the bulk of *Mont Blanc*. According to that calculation, such a mass, if constituted like the lava examined by Dr Kennedy, would have required a quantity of atmospheric air equal to the bulk of 4435 times the size of *Mont Blanc*, had that lava been generated by oxidation of the metals contained in it. Thus a bulk of nitrogen gas of 3503 times as great as that of the mountain mentioned, must have been evolved during the formation of that mass of lava.

It is beyond all probability to suppose caverns of such an enormous extent in the interior of the earth for containing such huge quantities of atmospheric air. Therefore, the advocates of the chemical theory will be obliged to assume very long periods for the formation of such masses of lava, during which the passages between the internal caverns and the external atmosphere must have been always open. Thus, they must suppose that atmospheric air must enter these caverns and quit them, after having been deprived of its oxygen, and that this entrance and exit of air took place unceasingly during the whole time of these actions. But it is obvious that these streams of air must be supposed to exist in such proportions as to support these actions, as soon as melted masses require to be kept in the liquid ignited state. There can, therefore, be no question that very considerable quantities of nitrogen gas ought to appear in the neighbourhood of such subterraneous actions on the surface of the earth. Are we to suppose that the scanty exhalations of nitrogen gas from thermal springs connected with volcanos proceed from such actions? Such exhalations are quite insignificant when compared with the formation of the masses of lava alluded to.

Professor Daubeny, however, remarks, that the nitrogen proceeding from these actions may reach the air not only in a separate condition, but also united with hydrogen in the form of ammonia. It is true, ammonia, especially sal-ammoniac, occurs among volcanic matters in craters or fumaroles, but, in general, in such small quantities as not to be sufficient to account for the huge quantities of nitrogen supposed to be separated according to the chemical theory. But, even granting all the nitrogen separated from atmospheric air to be united with hydrogen occurring in the focus of a volcano, chemists ought to ask, if a union of these gases can be supposed possible in the direct way? It is well known that chemists have hitherto been unable to unite directly these gases, even by letting down a mechanical mixture of nitrogen and hydrogen gas under the sea, to a depth of 540 metres, where the pressure is equal to 50 atmospheres. It cannot be doubted that the same laws of combination which take place in our laboratories also prevail in volcanos. But it may be replied that, for effecting combinations in the focus of a volcano, there are two powerful agents, not presented together in our laboratories, at least not in such a strong degree, viz., an enormous pressure, and heat. The former of them seems not to favour the combination of nitrogen with hydrogen, as we have above seen; and as for the latter, it is well known that ammonia is resolved into its elements by heat. Therefore, it is scarcely probable that an enormous compression in the focus of a volcano, assisted by a considerable heat, can effect a chemical combination between nitrogen and hydrogen.

However, the defenders of the chemical theory of volcanos will adduce the known fact that a mechanical mixture of hydrogen in excess, of oxygen, and of nitrogen, when ignited, produces ammonia, or rather nitrate of ammonia, and that, according to their theory, these substances ought to be present in the focus of a volcano. Supposing, for a moment, the formation of this salt, it is obvious that it would instantly be decomposed by heat into water and the protoxide of nitrogen. But, they will ask, how can you explain the occurrence of sal-ammoniac among the products of volcanic action, unless by the combination of the nitrogen with hydrogen? In reply-ing to this question it must be considered that many volcanos, like *Vesuvius*, are met with in sedimentary rocks containing organic matters, and that by their being heated, sal-ammoniac is formed. Therefore, the small quantities of this salt met with in craters or fumaroles may be sufficiently explained by supposing that rocks containing organic remains reach the focus of a volcano. In support of this I allude to the known facts, that among the masses thrown out of *Vesuvius*, there are not seldom large masses of limestone, which have likely occasioned the formation of sal-ammoniac, and that, during volcanic eruptions, bituminous odours are frequently perceived in the vicinity of the crater. Therefore, the salt just mentioned is, I think, formed in the same way in volcanos as in coal-strata which have been in a state of ignition for a long time. Thus, near *Duttweiler*, in the neighbourhood of *Saarbrücken*, I have found this salt in clefts proceeding from coal-strata which have been burning about 145 years.

In general, I by no means doubt that powerful chemical actions must take place in the focus of a volcano, in a spot where the strongest agents, such as an enormous compression and heat, are in operation. It is beyond a doubt that, under such circumstances, by the action of different substances on each other, proceeding both from the focus and from sedimentary rocks, chemical actions of the most different kinds must be supposed. Therefore, each theory ought to imagine chemical changes in the focus of a volcano; but the dispute lies between the two suppositions, whether these changes are a consequence of the effects generated by the internal heat of the earth, or whether they cause them. It is only the latter supposition I contest, from considering the lava as a mass already formed in the interior of the earth. The theory suggested by myself and by many naturalists, has only to explain the rising of the lava from beneath; this is the chief question. On the contrary, all chemical actions taking place at the contact of lava with vapours, sedimentary rocks, &c., are considered by this theory only as accidental effects, affording certainly the most diversified phenomena, but without influencing in an essential manner the volcanic actions themselves. All imaginable chemical changes must ensue as soon as organic remains in sedimentary rocks come in contact with the liquid ignited matters in the focus of a volcano.

Passing from this matter, I shall turn to the supposition of an oxidation of the earthy and alkaline metals at the expense of water. Professor Daubeny alleges to my objections concerning the disengagements of enormous quantities of hydrogen in such a case, that this gas could hardly be expected to escape in a free state from a spot which contains so many elements, for which it possesses a strong affinity, and to which it would be presented under the influence of the pressure and temperature so well calculated to promote its combination with them.

Venturing to offer some remarks against these consequences, I shall first endeavour to calculate what quantities of hydrogen ought to be separated, on the supposition that a certain quantity of lava would be generated by oxidation of their metallic principles, at the expense of water. I shall then examine into the possible combinations which hydrogen may be assumed to present in the focus of a volcano.

As for the calculation, I shall found it on that given by Daubeny, above quoted. 100 parts of lava contain 42 parts of oxygen, and these are contained in 47.6 parts of water. We may consequently suppose that lava, when formed by the oxidation of its metallic principles at the expense of water, requires about 50 per cent. of it. Lava being 3.2 times heavier than water, 1 volume of the former will require about 1.5 volumes of the latter to be produced. The lava ejected from the *Skaptar Jokul* would, of course, have required a bulk of water 1.5 times as much as that of *Mont Blanc.*

I feel disposed to believe that the explanation, as to how such considerable masses of water may gain admittance to the focus of a volcano, is less difficult than the supposition that there are substances in sufficient quantity for uniting with so great an amount of hydrogen as would be separated by the decomposition of that water.

Professor Daubeny mentions the sulphur and chlorine we know to be generally present in volcanos. As 6.188 parts of hydrogen unite with 93.812 parts of sulphur to produce 100 parts of sulphuretted hydrogen, 5.6 parts of hydrogen separated from 47.6 parts of water requisite for the oxidation of the metallic bases in 100 parts of lava, would suppose the presence of about 84 parts of sulphur. In fact, were lava to be formed in this way, and were the hydrogen separated to be united with sulphur, the quantity of the latter ought to be 84 per cent. of that of the lava generated. Applying this result for calculating the bulk of sulphur requisite for producing the lava ejected from *Skaptar Jokul*, we find this bulk about 1.4 times as much as that of *Mont Blanc*, for the specific gravity of sulphur is 1.99, and that of lava 3.2. Indeed, such a considerable quantity of sulphur can scarcely be assumed in the focus of a volcano. But even let us suppose it, on the other hand it must be asked, how comes it that the earthy and alkaline metals, having a strong affinity to sulphur, are not rather united with this substance ? Though these combinations do not take place at a common temperature, yet they were to be expected, as soon as heat is evolved by the commencing oxidation of the metallic bases, at the expense of water or atmospheric air. Therefore, the advocates of the chemical theory cannot suppose the presence of sulphur in the focus itself, where these metals are imagined to be, but in higher places, where the hydrogen arising from beneath would unite with it. Granting even the sulphur to be in these higher places, the formation of sulphuretted hydrogen, nevertheless, cannot be supposed, as it is known that hydrogen and sulphur cannot be united directly, and scarcely even by melting and volatilizing sulphur in hydrogen gas.

Finally, it must be well considered that, according to the chemical theory, the decomposition of water by metallic principles would be the first phasis of volcanic action ; that, consequently, the second one would be the combination of the hydrogen separated with sulphur. These two phases of volcanic action would of course precede by a long period the eruptions of lava. But volcanic phenomena present, in general, the contrary of these suppositions. Disengagements of sulphuretted hydrogen gas do not take place before the issuing of lava, but, for the most part, after all other volcanic phenomena having ceased. It is well known that these disengagements take place, in general, when a volcano is in the state of a solfatara. The solfatara near *Puzzuoli*, and many in the *Andes*, present sufficient instances of this kind.

This circumstance seems to me the most fatal to the chemical theory. I cannot imagine how its advocates can remove this difficulty. They will not surely be disposed to assume that the hydrogen separated by the decomposition of water, in the first phasis of volcanic action, is reserved in vast caverns near and about the focus of the volcano, and that it is only a long time afterwards that the combination with sulphur takes place?

I am quite at one with Professor Daubeny, in admitting the large beds of sulphur which exist in most volcanic districts (viz. *Sicily*), to be the result of the decompositions of the sulphuretted hydrogen evolved. Many years ago I stated in German journals, and, I think, on good grounds, that these beds, both in volcanic and neptunian districts, may for the most part have been generated by the slow oxidation of sulphuretted hydrogen at the common temperature, at the expense of atmospheric air, whereby only the most oxidisable ingredient of it, the hydrogen, was oxidised, whilst sulphur was deposited. Regarding the formation of sulphuretted hydrogen, there is, however, a difference of opinion between Daubeny and myself. He remarks, that I pass over without any attempt of explanation, among other things, the evolution of sulphuretted hydrogen, in quantities far exceeding what are to be explained by the reaction of carbonaceous matter upon sulphates, or any of those other processes which sometimes produce it on the surface of the earth.

Though intending to discuss this subject in my future publications, yet I may take this opportunity of answering these remarks of Professor Daubeny.

Sulphuretted hydrogen seems generally to be formed by the reaction of carbonaceous matter upon sulphates. There are many instances which appear to lend considerable weight to this supposition. Thus mineral waters containing sulphates disengage sulphuretted hydrogen, when for a long time in contact with any organic matter, for instance straw. I once put a tea-spoonful of sugar in such a water, placed in a pitcher. After having allowed it to remain four years in a cellar, such a considerable quantity of sulphuretted hydrogen was generated as to form a strong sulphurous water. The sulphate of soda formerly contained in it had disappeared, and iron pyrites was deposited. On the bottom of a mineral spring containing only a very small quantity of sulphate of soda, I once found pieces of iron pyrites, including organic matters, such as branches and stalks of plants. There is no doubt but this iron pyrites was formed by the decomposition of sulphate of soda and carbonate of the protoxide of iron, at the expense of the organic matter.

We can accordingly have no difficulty in admitting most sulphurous springs to be formed in the same way. In support of this, it may be mentioned, that all sulphurous springs contain sulphates and organic matters. I allude only to those in the *Pyrenees*, which, it is known, abound in vegetable matter, the so-called *Baregine*.

Many exhalations of sulphuretted hydrogen in volcanic districts may be formed in the same way. At least organic matters are not wanting in sedimentary rocks which have been broken through by volcanic actions, and sulphates exist in many volcanic products.

Assuming that melted lava comes in contact with sulphur at any part of a volcano, and that the so-called liver of sulphur is produced from alkalies and alkaline earths; supposing, farther, that the liver of sulphur meets with water and carbonic acid gas, sulphuretted hydrogen will be evolved. We may easily imagine exhalations of this gas during the course of many ages, when liver of sulphur abounds. Indeed, the immense disengagements of carbonic acid gas in many districts of former volcanic action, within the memory of man, appear to lend considerable weight to the theory suggested.

I shall not pass from these considerations without alluding to Daubeny's supposition that the chlorine also, which we know to be generally present in volcanos, may be united with the hydrogen evolved by the decomposition of water. But the occurrence of a considerable quantity of chlorine in a free state in a volcano is hardly probable. We know this substance to be merely evolved when a peroxide is present. In the interior of the earth there is only that of manganese which is rather abundant. When present at the locality of a volcano, it can merely be supposed to exist in such places where there is no red heat. An enormous quantity of chlorine must be present to unite with the hydrogen evolved. For instance, 5.6 parts of hydrogen, calculated above, require 187 parts of chlorine to be present for producing 100 parts of lava, when the hydrogen separated from water is to unite with it.

Whatever be the importance of these reflections, every one must come to the conclusion, that the production of lava, by oxidation of its metallic principles, requires immense quantities of atmospheric air or water, and that the hydrogen separated by the decomposition of water, supposes the presence of sulphur or chlorine for uniting with them in such quantities as to exceed those of the lava itself.

It is true, the quantities of gaseous matters rising from the crater of a volcano during its eruption cannot be estimated in proportion to the lava issuing from it at the same time. However, as soon as these matters should exceed the lava in quantity, the atmospheric air round about the volcano, to a considerable distance, would be quite fatal to animal life. Accounts of the eruptions of *Vesuvius* and other volcanos do not mention such phenomena. Gaseous exhalations, it is true, take place in abundance in the environs of *Vesuvius*, but only after the cessation of the eruption and the issuing of lava. These exhalations, consisting of carbonic acid gas, cannot be connected with oxidations of the metallic bases supposed to exist in the interior. Professor Daubeny himself assumes that these disengagements cannot be derived directly from chemical processes which produce the phenomena supposed by him, but that they are only caused by the heat which these processes tend to diffuse through the adjacent rocks.

Among the simple bodies for which hydrogen possesses a strong affinity, oxygen occupies a high rank. Professor Daubeny speaks of this gas uniting with the hydrogen separated by the decomposition of water. However, it seems to me rather difficult to suppose its presence in a place where, according to him, mighty oxidations take place. But even granting atmospheric air, after having acted upon the metallic principles, to reserve a sufficient quantity of oxygen for the ignition of the hydrogen evolved, this supposition leaves the explanation generally on the same footing. Indeed, admitting this, it is the same as if atmospheric air alone were to oxidise the metallic principles of lava, viz. the same quantity of oxygen, formerly furnished to them by water, must now be afforded to the hydrogen by the atmospheric air.

To these considerations it may be added, that Sir Humphry Davy could not, in any of the several instances in which he experimented on lava, when freshly poured out, and in a liquid ignited state, detect any traces of pure or uncombined alkaline or earthy inflammable bases.* Dr John Davy properly adds that the absence of iron in its metallic state amongst the products of volcanos, so abundantly oxidated in the first degree of oxidation, is very unfavourable to the idea that large quantities of inflammable gas are evolved in volcanic eruptions, or even disengaged. Were this oxide acted on by hydrogen at a high temperature, what is there which could prevent its decomposition ? and if reduced, we might expect to discover it in this state, at least occasionally, enveloped in and protected by lava.

Before finishing my remarks, I cannot help asking the defenders of the chemical theory, whether they suppose that the volcanic actions by which the immense mountains of granite, trachyte, basalt, porphyry, &c., have been elevated, resulted from the same chemical processes ? The question is, did these masses occur in the interior of the earth in a metallic condition, or as oxidised bodies ? In the latter case the advocates of that theory would be obliged to embrace the very same explanation of volcanic actions I am venturing to defend. On the contrary, by assuming the former opinion, the very same difficulties would be placed in opposition which I have established above, but, as we cannot doubt, only much greater in degree. It must be asked where are to be found the substances separated by the oxidation of the metallic bases of the liquid ignited masses that have arisen from the interior, whether it was that atmospheric air or water was decomposed, whether it was that nitrogen or hydrogen was separated, or that ammonia, or sal-ammoniac, or sulphuretted hydrogen was produced ? These substances, the equivalents of the ejected liquid

* Memoirs of the Life of Sir Humphry Davy, by his brother, vol. ii. p. 122; and Collected Works, vol. i. p. 259.

ignited masses raised up, amount to quantities equal to the volcanic rocks, and even surpassing them, as we have seen above. But nowhere on the surface of the earth, or in the atmosphere, are such immense masses found. If the defenders of such a hypothesis were to suppose that in former times the proportion of oxygen in the atmospheric air was greater than now, the possibility of such a supposition cannot be denied without hesitation; however, it may be remarked, that there are other grounds for inferring the opposite opinion.

The further details I have to offer on the subject must, however, be reserved for publication in a future number of this Journal.

Summary of the most important Geognostical Phenomena with which it is necessary to be acquainted in Preliminary Mining Operations. By the late FREDERICK MOHS, Councillor of Mines at Vienna, and Knight of the Royal Saxon Order of Civil Merit, &c. (Concluded from Vol. xxix. p. 21.)

Repositories of Simultaneous and of Posterior Formation.—It is now time for us to direct our attention for a short space to the object of the search, to the manner in which repositories of useful minerals are contained in the various rocks which have been considered, and to their different relations. Two kinds of these repositories are generally distinguished, of which the one is assumed to be of *simultaneous* formation* with the including mountain-mass, and the other is regarded as of *posterior* formation.† To the first belong beds, and lying masses (*liegende Stöcke*), and under the other are included veins of a great variety of descriptions. We can easily judge of this classification, when we find that geognosts, and with reason too, speak of *contemporaneous* veins, that is, of such as are of simultaneous origin with the mountain-masses. A contemporaneous vein is a repository which has been produced as a vein at a later period than the rock, and which, nevertheless, as a contemporaneous vein, is of simultaneous formation. As this is a contradiction, and as the contemporaneous veins

* See Vol. xxix. of the Journal, p. 4·.

† D'Aubuisson, Traité de Geognosie. 1re edit. t. ii.

can be distinguished by no decisive character from those which are assumed to have been produced subsequently to the rocks they traverse, but, on the contrary, present an uninterrupted series with them,* it is better to leave this classification aside, and to distinguish the repositories of useful minerals, when it is necessary to do so, entirely by their form, and by their relations to the surrounding rocks, and to their structure, that is, without reference to their period of formation, or to their relative age.

Disseminated Ores and Stockwerks, &c.—It is the case with some useful minerals, that they are not confined to a space of determinate form, but that they occur disseminated or distributed in smaller or larger imbedded portions, in certain portions of mountain-masses. Tinstone and native gold are remarkable examples of this mode of occurrence. The rocks in which these metals are met with, are, in respect to the former, granite and porphyry, and more rarely gneiss; for the latter, granite and some sandstones, and probably also

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* Some geognosts, probably from noticing this contradiction, and from having perceived the undeniable simultaneous formation of these veins with the rocks themselves, endeavour to separate the contemporaneous veins from the true veins, that is, from tabular repositories, which, as is assumed, have been formed at a later period than the surrounding rocks, therefore in actual previously existing open fissures; and, in order to establish this distinction, they avail themselves of various characters, which are partly derived from the form of these repositories, partly from their contents, and partly from their relations to the rocky masses.

It is true that these characters belong to several of these so-called contemporaneous veins, but they are also met with in many of those regarding which it is asserted that they have been produced at a later period than the traversed rocks; while, *vice versa*, those properties by which the latter are distinguished, are to be found in the former in greater or less perfection. When, in a series of veins of this description, we compare the neighbouring members, we find but few differences, but, when we compare more distant ones, the difference is so great that we can hardly discover one character in which they agree. This is the nature of these repositories, and in this consists the character of the uninterrupted series which they exhibit. The attempt to discover decided marks of distinction between beds, contemporaneous veins, and real veins, that is, those presumed to be of subsequent formation to the rocks in which they occur, will probably, therefore, never succeed, if we except from the latter those which have been formed in more or less considerable tabular hollows, of *actual* fragments and rolled masses, of bone fragments, &c. or which have been produced on active volcanos in actual fissures, all of which are not to be included among the original repositories of minerals now under consideration.

some varieties of greenstone. Both present themselves sometimes in such small particles, that the eye cannot at all distinguish them, or at least can only do so with difficulty. Hence, when we have reason to suspect the existence of such metallic substances in a rock, we must subject the stony mass to a more minute investigation of its composition, by means of mechanical separation, and we must endeavour to become acquainted with the extent of the repository, which, as already remarked, is not limited to a certain form by means of fixed and recognisable limits. Sometimes the space in which such minerals occur is pretty equally extended in all directions, and this is the case in some of what are termed *Stockwerks;* sometimes one of the dimensions is much smaller than the two others, and when, therefore, we include the zone or region in which the useful minerals occur between two plane surfaces parallel to its greatest dimensions (which are neither present as seams of stratification, nor as surfaces of distinct concretion, or at least the circumstance is quite accidental, if the latter be the case), it assumes the form of a bed, or generally of a tabular repository, as is frequently observed in the gold occurring in granite.

Kidneys, Nests, Putzen, and Lying Stocks.—Another kind of occurrence of the useful minerals consists therein, that they present themselves either in single considerable irregular masses, or that there are several at short distances from one another; and, in the latter case, they may either occupy or not a recognisable region. This form of occurrence belongs to some iron-ores, and is observable partly in slates, and partly in limestones. The smaller masses of ore are generally termed kidneys or nests, and *Putzen*, while the larger are denominated *Lying Stocks.* In the slate rocks, the structure is sometimes abruptly terminated by these masses, and sometimes it winds around them, without altering generally or to any extent the direction. When such small masses of ore are confined to a certain region, they give the idea of a bed, and then, not unfrequently, there is a clue which leads from one to the other.

Layer of Foreign Matter between the Kidneys, &c., and the inclosing rock.—The kidneys, nests, and *Putzen,* sometimes exhibit a phenomenon in respect to the rocks in which they occur, which is also observable at the contact of two different rocks, and which is not unlike that of which we have spoken above. When the mass of ore passes directly into the rock, or is intimately united with it at the line of junction, this phenomenon is scarcely to be expected; when, however, this is not the case, there is frequently found, between the ore and the rock, a layer of another substance, of a clayey or other composition, which, as it were, incloses the mass of ore in greater or less quantity, inasmuch as it generally, without interruption, surrounds it on all sides, and passes as well into the mass of rock as into the mass of ore. In cases like those hitherto considered, this substance often affords the clue just alluded to, and attention to it may, under these circumstances, be of direct use, so that we should not in any instance omit its investigation.

Filons de Contact.—When two different rocks, the one perhaps in the form of a bed, the other in the form of a lying *stock*, or whatever may be the mode of occurrence, meet under the above circumstances, viz., that they do not directly pass into each other, or are not intimately united with each other at the line of junction, but that they are either separated by mere masses of distinct concretion, or by a foreign substance, the latter, whatever may be its nature, and however unimportant it may at first sight appear, deserves particular attention. It has frequently happened that this substance has not only proved metalliferous, but also that it has consisted chiefly or entirely of ore, and that sometimes it has been of considerable magnitude. We cannot term such appearances veins or beds,* and cannot compare them with any of the repositories mentioned by authors; and they would thus seem to have attracted less attention than they deserve, although it is known to miners that junctions are important places for their operations. But this does not hold good for all junctions, for when gneiss and mica-slate, or clay-slate, or greywacke, &c., are in contact, the junction is generally accompanied by the appearance of transitions in the direction of the thickness; and hence there is little or nothing to be expected. When, however, granite, especially in amorphous masses, is in contact with one

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* The French term such occurrences *des filons de contact*.—D'Aubuisson, 2ieme ed. t. ii. p. 575.

of the slate rocks, or, still more, when limestone is in contact with granite, syenite, porphyry, greenstone, without being united by direct transitions, or even when it is separated by traces of a foreign substance, these are circumstances in which there is something to be hoped for, and which therefore render a more close examination advisable.

Venigenous Stockwerks.—In the modes of occurrence of useful minerals already considered, the structure of the rock, as well as of the mountain-masses, is generally, though not always, intersected; but there are phenomena where this frequently happens without our being able to say that, by this means, the actual character of the repository is determined; for there are appearances of this kind, and of perfectly similar nature to one another, which are sometimes conformable to the structure throughout the whole extent, and sometimes are only partially so. These are small, irregular, chiefly tabular masses, which, like the so-called contemporaneous veins of calcareous spar in limestone, extend in all possible directions, and, therefore, also in directions parallel to the structure, and which, on account of their insignificance, do not deserve separate consideration. But still there are in certain mountain-masses whole tracts, which, without being confined to fixed forms and boundaries, are traversed by similar small veins; and these are frequently so numerous that they become valuable to the miner, especially since, just as in some of the previously described phenomena, the neighbouring rock is frequently metalliferous (impregnated with ore), and hence they undoubtedly become important objects of examination. A part of what are termed *Stockwerks* likewise belong to the same category; and tinstone, native gold, and probably some copper-pyrites and glances, are the minerals found in such repositories. We can easily imagine that, towards the boundaries of such a mountain-mass, traversed by similar veins, these veins appear more subdivided than near the centre; and hence, when we discover single veins of this kind, we must carefully ascertain if these are not connected with a tract which contains them in a larger number, and of a richer description.

Like all the other phenomena of nature, this one does not stand alone. In the slate-rocks, in which the phenomena aiready discussed occur, though not exclusively, the venigenous (*i. e.* small, irregular, tabular) repositories are sometimes confined to certain zones or regions, which, when limited by two plane surfaces, bounding them entirely or almost entirely, and when these tabular regions lie conformably to the structure of the rock and mountain-masses, may be viewed as beds more or less branched, or, when they intersect that structure, may be regarded as more or less branched veins. Sometimes such a region or zone is distinguished in the mountain-mass in which such appearances present themselves, by the nature of its rock, inasmuch as variations in its structure may be remarked, or it occurs in such a condition as is termed a more or less advanced state of decomposition, disintegration, or weathering, and which is attributed to a change produced subsequently to the original formation. When such a region, especially when it is not metalliferous, lies conformably to the general structure, it is generally termed a rottenness *(eine Fäule)*, a clay fissure *(ein Lettenkluft)*, &c. (at least what are termed promising appearances come under this denomination); but if it intersect the structure, it is regarded, when without ore, as a barren rock *(taube Gestein)*, or clay vein *(Lettengang)*, of which the slate mountains afford remarkable examples. Sometimes, however, such veins ought not to be neglected; for it has often been observed *that from them there are developed vein-like formations, and even extensive and wide veins, which not unfrequently prove rich and of mining importance.* The *Erzgebirge*, both on the Bohemian and Saxon sides, contain extremely remarkable instances of this feature. In mountain-masses in which there is no slaty structure present or perceptible (which are not stratified, according to the usual mode of expression), the nature of these repositories is determined by the difference of their direction, and by the general relations of superposition, that is to say, we so determine whether they are to be regarded as beds or veins. Various copper-ores (that is minerals generally which contain copper, and which are used as ores of copper), sometimes cinnabar, more rarely some others, are the metallic substances found in these repositories, here and there accompanied by the so-termed vein or bed-stones, especially when they are in the form of beds; and the above observations, in so far as individual small veins (*Trümmer*) of this kind are met with, find also their application here.

Passage of Stockwerks into beds and veins proper.—Sometimes the small veins (*Trümmchen*) of Stockwerks, slender at first, increase in width, and so approach one another, that not only do the repositories thus become richer, but likewise entirely change their nature and constitution by several uniting, and at last forming a single individual. On the one hand, there thus arise regular beds, at first still provided with their lateral and companion veins, but ultimately as tabular masses contained between roof and floor; and, on the other, regular veins, at first also with their accompanying offsets, but afterwards as masses contained between their *salbandes* (*Saalbändern*); and the first intersect the structure, just as often as the others lie partly conformable or parallel to it. There are individual examples, as well of beds as of veins, in which we meet *simultaneously* with all the differences hitherto spoken of, and which, in one part, consist of mere single small veins (*Trümmern*), but in another, form a more connected mass, only here and there accompanied by such little veins, and which, finally, not unfrequently appear as repositories which are completely bounded, not unfrequently, by distinct concretion-surfaces and separating layers (*Bestege*); and we may quote as such, some of the copper-ore repositories in the Tyrol for the bed-like forms, and some of the lead-glance repositories of the Hartz for the vein-like forms. But repositories of both kinds, which are for their whole extent of uniform characters, are the most usual, and of these, those which appear as connected masses are the most important. These are the *beds* and *veins* in their proper and usual mining and geognostical sense.

Beds, and the materials of which they are composed.—The general properties of beds already mentioned in the preceding pages having been premised, I wish more particularly now to notice that all these repositories do not consist entirely of ores, but that what is contained between the roof and floor as the actual mass of the repository, often appears as a combination of ores and other minerals. Sometimes the mountain-rock itself forms the mineral substance of the bed, as is very often the case with limestone and slaty rocks;

and the ores, or, properly speaking, the matrix of the ore (*Erzmittel*), appear sometimes as separate kidney and *putzen* shaped portions in them, and are connected with the above mentioned kidneys and *putzen;* sometimes they form continuous but irregular masses, which enlarge or contract in different directions, and which are so branched that it is impossible to assign them any definite form, and even in many cases any definite direction, and all we can say is, that they lie between the roof and floor, although even these can sometimes be recognised with difficulty. In limestone rocks they sometimes appear as what the older English geologists termed *pipe-veins.** In very many instances, however, the mineral substances of the bed are different from the mountain-mass, although, nevertheless, they sometimes still stand in a certain connection with it. Thus, should the bed occur in a slate rock, it is found that the mineral substances of that bed are chiefly quartz and calcareous spar; more rarely, though sometimes, as accompaniments to magnetic iron-ore, we have garnet, hornblende, &c. If the mountain-mass be greenstone or hornblende-slate, the component parts of the bed are frequently hornblende, augite, pistacite, garnet, felspar, and calcareous spar, without mentioning those of more rare occurrence; and if the rock be limestone, the beds often contain calcareous spar, fluor spar, heavy spar, &c. However we ought not to regard all these as universal, but only as generally occurring circumstances; and we ought not to neglect the careful study of the substances forming these beds, when we are occupied with the discovery of bed-like repositories. The ores are generally firmly and intimately united in larger or smaller massive portions in the constituent mineral substances of the beds; frequently they are found perfectly fresh, without traces of weathering, disintegration, or any other change, and we can thus often determine as to a mass which has been removed from its repository, if it has been derived from a bed or a vein, for in the latter some of these phenomena are often of a different description, as the sequel will shew. But even these characters are not always to be trusted to, for the minerals obtained sometimes

* Williams's Natural History of the Mineral Kingdom, vol. i. p. 331.

possess perfectly the above indicated features, and yet we perceive that the repository intersects the structure of the rock and of the rocky masses, and is consequently a vein and not a bed.

Among the useful minerals which occupy conformably the space of whole repositories, although also mixed here and there with the mineral substances occurring in these beds, are more particularly to be mentioned iron-ores, including sparry iron, iron-pyrites, and calamine. Lead-glance, likewise, and some antimony-glance and manganese-ores, belong partly to this division. Among those, on the other hand, which are generally obtained in combination with the mineral substances forming beds, are to be reckoned, as the most general, the copper-ores, especially copper-pyrites and grey copper-ore (*Fahlerz*), and likewise sparry iron, inasmuch as it often occurs along with copper-ores as they do with it, in one and the same repository. True ores of silver, without including mere argentiferous ores, seem to be rare in beds; but still, little can be said with certainty on this head, because it is often difficult to decide positively whether a repository is to be considered as belonging to the beds or to the veins; any gold which may occur in veins is generally associated with quartz, and is frequently so intimately and finely united with it, that we can only recognise the actual presence of the gold by the colour passing into grey, and by the other properties of the quartz, which generally has less transparency and lustre than it is wont to possess in similar or analogous repositories, where it is not auriferous.

In many beds, the ores and mineral substances are arranged together as it were in groups, and thus form metalliferous and barren points (*erz und taube mittel*) After one has carefully observed these metalliferous and barren portions, regarding which no general statement can be made, he can recognise their appearance and disappearance, before reaching or traversing them by means of boring or mining. This is important for the investigation and excavation of repositories, and a great portion of the peculiar skill of the practical miner is founded on observations and determinations of this kind; it does not, however, belong to a place where the objects under consideration are merely the search for and discovery of these repositories. However, it leads to another subject which can be of use also in the latter case. I allude to the arrangement of certain ores and mineral substances belonging to beds in repositories of the nature above described.

It is easy to be remarked that all ores which occur in beds are not associated with one another, and that they are not indiscriminately accompanied by all the substances forming these beds. Moreover, we find that beds of a certain composition, namely, such as contain certain ores and certain mineral substances, are as it were united to certain mineral masses, and do not present themselves in others. This sometimes goes to such a length in slate-rocks, that we only find such beds in those places or regions which are composed of particular varieties of this or that slate. Hence has arisen the idea of *bed-formations* (*Lagerformationem*), which, although it cannot be referred to fixed and certain characters, and generally, therefore, cannot be spoken of definitely, yet in the search for such repositories, can afford useful assistance. Hence, in districts where several of these repositories are known, it is of consequence in this occupation to study as accurately as possible what may be the formations of these repositories, and the phenomena of their arrangement, inasmuch as experience has taught us that when the *general*, and hence *essential*, features are properly understood, a great constancy is found to exist, which leads not only to the discovery, but also to the determination, of the mining value of such a bed, although, as already remarked, it cannot be reduced to universally applicable characters or distinct rules, and must be left to the study of those to whom is intrusted the important duty of searching for repositories of useful minerals, and who really have this object at heart.

Bed-depots and Bed-districts.—When metalliferous beds of any kind whatever occur in a mountain district, they generally do not present themselves singly, but several lie over and near one another at indeterminate distances. Such a district is generally properly termed a *bed-depot* (*Lager-depot*). It is at the same time to be remarked, that beds of a certain formation do not exclude those which belong to a different formation, and that all taken together constitute what is denominated a *bed-district* (*Lagerreviere*). The ideas included under the terms bed-depots and bed-districts, include in their details much which may serve as a good introduction to the search for individual repositories, but are not of a nature to be introduced in this place.

Veins.—A great part of what has been said on beds is equally applicable to veins. The most marked difference which can here be taken into consideration between these two kinds of repositories of useful minerals, consists, as has been often stated, in the phenomena of their positions. The beds of a *bed-district* are (keeping peculiar circumstances out of view) parallel to one another, and lie, without exception, conformably to the structure of the mountain-masses, for they are just on that account beds, because this parallelism and this position, in reference to the structure of the rock and the mountain-masses, are predominant in them. The directions of the strike and dip of the veins of a vein-district may cut one another under all possible angles, and are in no respect connected with the structure of the rock or of the mountain-masses. They may even be continued from one mountain-mass to another, for these repositories are just on that account veins, because they are independent, in regard to their position, of the mountain-masses, and of their structure. It is assumed regarding veins of one and the same formation, which, when several are present in one district, form a magazine of veins *(Gangniederlage)*, that they possess at least a like strike, although their dip may be different in amount or even in direction. The veins are generally more complicated in their composition than most beds are (for there are also beds which contain the varieties of a large number of species of different minerals, and thus surpass many veins, even such as do not contain merely iron-ores, &c.), and the distinguishing several formations is thereby rendered the more effectual, the more the knowledge of them is extended. Experience has shewn that in an extensive district, in which a large number of vein-formations had formerly been distinguished, these have been proved by further observations to be so united with one another, or are connected and pass into one another in such a manner, that it is no longer possible to assign them boundaries with any precision ;* and, although this

* Vide Kühn's "*Geognosie.*"

is in favour of the above assertion, that the idea of vein-formations does not admit of being referred to definite principles, and these formations themselves cannot be characterized by fixed characters, yet, as so great a diversity of differences does not occur in all places, this idea ought always to be kept in view in the investigation of a district for the presence of repositories of this kind, the known veins ought always to be studied thoroughly in this point of view, and we ought not to doubt but that, in this way, much may be acquired and learned, and that much instruction may be obtained for approaching our desired object, of which we should have been deprived without this information.

Besides the greater collections of veins which, in what has now been said, have been termed vein-magazines or depots, and vein-districts, there are others of smaller extent which are well deserving of enumeration. The first is that in which the large principal vein is accompanied by narrower, often also less extensive repositories of the same formation which are termed companions *(Gefährten)*, just as in the vicinity of a large mass of granite, we not unfrequently find smaller portions of the same rock, in every variety of forms. The second consists in this, that sometimes several small veins, of similar constitution, occur so near one another, that they are only separated by slender stripes of the containing rock, and thus admit of being mined together ; and that of such collections, several are found together in a parallel position, and at small distances apart. The intermediate rocky matrix often includes the ores which occur in the veins, either disseminated or in small massive portions, and this also sometimes happens when the veins contain but little, or are altogether barren. The third of these collections, finally, contains a great number of small, chiefly irregular veins of one formation, whose thickness still amounts to something, which so run through one another in all directions, that they cross in the most diversified manner, seldom penetrating, but sometimes causing shifts. The spaces which such veins occupy in rock-masses are completely without determinable limits, and the whole phenomenon is therefore specially remarkable, because it places in the most intimate connection the real and characteristic veins with those in the previously described so-termed *stockwerks*, and which consist of small veins (*gangtrümmern*), and contributes generally to the ascertainment of the near and uninterrupted connection which subsists among the whole series of repositories of useful minerals.

As veins are not confined to a particular mountain-mass and its structure, they may, as already stated, be continued from one mountain-mass into another ; and, as experience shews, may actually do so several times. It is at the same time remarkable, that, in the different rocks through which they pass, they not only exhibit a change of character and of thickness, but likewise of the minerals they contain, so that they might be regarded as different repositories in one mountain-mass from what they are in another, were we not able to follow them from the one into the other. But it must be added, that this is not always the case, but that several veins are known which traverse different rock-masses, and that without undergoing the slightest change. There is another phenomenon connected with this subject which deserves especial attention in a search after veins. It consists in this, that veins often occur near the boundary between different mountain-masses, partly in such a manner that they only occur in one or the other of these mountain-masses ; and partly in such a manner, that they pass from the one into the other, and thus exhibit the appearances just before mentioned. There are several remarkable instances of this kind which are well known, and more might be discovered if the relations of superposition and of mountain-masses were better investigated and better understood.

Changes in Vein-stones and in neighbouring rocks.—There are various important peculiarities of repositories of useful minerals which have not hitherto been mentioned in this essay, and which may remain unconsidered in this place. One of these, however, requires to be shortly discussed, as it may have an important influence on the discovery of such repositories, and more especially of veins. This consists in the changes that the minerals, of which the vein masses are composed, and even the mountain-rocks in their vicinity, undergo.

There are often found in veins, impressions of crystals of minerals which no longer exist, in other minerals that still occur in the original place of their production. There occur minerals converted into pulverulent substances, or, partly while retaining their peculiar form, into varieties of a different species; and, lastly, minerals are met with, regarding which, from the circumstances under which they occur, we must assume that they have been produced by the destruction of others. All these are proofs of the changes which have taken place in veins, and, in so far as regards the destruction, especially in the upper portions. The outgoings, and even considerable portions of the upper regions of veins, are thus frequently so altered, that we are scarcely able to recognise them; inasmuch as the outgoings, and not unfrequently the upper parts of veins, probably originally of considerable breadth, being pressed together, and deprived more especially of their metallic contents, often appear as mere small, barren fissures, containing clay, or entirely empty. But, at the same time, the neighbouring rock has generally suffered considerable alterations, which often extend from the sides of the veins for considerable distances into the hanging and lying sides, and clearly distinguish such a rock from the fresh one. These alterations of the rock are denominated weathering or decomposition, and are always regarded as a sign which betokens the approach to ore. These appearances are particularly important in the search for vein-like repositories; and, therefore, places in mountain-masses, which, without any particular cause, occur in a state of decomposition, just so much the more deserve attention and minute investigation; because a mere barren, empty, or sterile fissure may be the outgoing of a vein, which may prove very rich, only when it reaches a certain, frequently a considerable depth. Hence it is necessary, in districts in which veins are already known, and have been mined, to investigate expressly their outgoings, and to study them well, in order that, with their assistance, we may be able to recognise other veins when they occur. We must not depend too much on the coloured and peculiar looking *Schweife*; for few veins are of such a nature that they appear with those *Schweife* at the surface of the rock. In bed-like repositories, these appearances occur less frequently, although even they are not entirely without examples of this kind. Their position, the mode of arrangement of their minerals, and even the nature of these minerals, may be the cause of this difference. The phenomena which, according to what has been formerly said, can be regarded as nothing else but original productions, must be well distinguished from those, which are consequences of changes that have taken place in the original condition of the repositories themselves and of their minerals. To the former belong the more or less considerable beds of a clayey nature, in which only here and there, either detached imperfectly-formed portions of the rock, or at least of the structure of the mountain-masses in which they occur, can be recognised ; and there are vein-like repositories, pretty much of the same description, which seem to be filled with, as it were, softened and compressed fragments of the neighbouring rock, and which are generally regarded as such like upfillings. Although the connection in which they stand with the phenomena already mentioned points to a different origin, we find, almost exactly as was there shewn, repositories of both kinds, which consist chiefly of clayey materials, from which here and there more compact masses have as it were been separated, which are occasionally metalliferous, and sometimes, indeed, very rich. It is very easy to judge incorrectly regarding the true nature of such formations, to regard them as mechanical productions, and thus to be deterred from prosecuting their farther investigation ; and hence, also, these require a careful study.

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Surface relations of the district to be examined.—Since the operations directed to the search for valuable repositories of useful minerals can only be prosecuted on the surface of rocks, that surface, in which such investigations are to be carried on, becomes an important object of consideration ; and is even the very object with which the whole business is to commence. The general relations of the surface of mountain-groups are very easily made out. They are chiefly founded on the position and characters of the valleys and the elevations included between them. Most valleys, or at least the most important ones, take their origin in the highest parts of the mountain-groups, frequently in the numerous ravines, and their direction follows the declivity of the group, until an obstacle occurs, produced, either by the nature of the rock, or by the structure of the mountain-masses and their position, and which does not allow them to proceed farther in that direction, or until a special relation occurs to cause a turning, by means of which the previous *transverse valley* is converted into a *longitudinal valley*, and now the structure of the rock or mountain-masses proceeds in a parallel manner, instead of intersecting the valley as formerly. But every longitudinal valley does not derive its origin from a transverse one. The longitudinal valleys, in which these transverse valleys open, that do not issue at the foot of the mountain-group do not always, throughout their whole course, retain the character peculiar to them, of running parallel to the structure of the mountain-masses, but sometimes again become transverse valleys ; and hence we cannot conclude from the course of a valley as it occurs in one tract of country, as to its course in another, because, as has been shewn, that course is susceptible of change in some cases. It will appear in another part of this treatise,* how far an acquaintance with the valleys can be made available in the search for repositories of useful minerals.

Of the elevations, which, if not produced, are yet separated by the openings forming the valleys, the *high mountain-ridge* is the most important. It determines not only, as the water shed, the chief stretch, but also the principal form of the whole mountain-group, and besides, for the most part, affords a very good opportunity of forming an opinion of its structure. It must not be expected that what has been previously termed the central mass constitutes every where the high mountain-ridge, for this would only be the case if it could be assumed that the present external form of the mountains had been the original one. We frequently find that the high mountain-ridge is composed of slaty mountain-masses, while the granite makes its appearance in extensive mountain-masses, in the ridges of one or some times of both declivities of the mountain-group. Nor does the high mountain-ridge always include the greatest heights. The ridges of the lateral chains likewise afford an opportunity for useful observations, and they not unfrequently present exposures of those mountain-masses which are covered on the de-

* Already published in this Journal, vol. xxviii. p. 334.

clivities and in the valleys. Particularly striking eminences, whatever form they may possess, generally exhibit a composition different from that of their vicinity, and are therefore worthy of notice; and on the other hand, trough-shaped hollows, basin-like widenings of valleys, contractions of valleys, and similar phenomena, are often serviceable in enabling us to draw useful conclusions. In order to apprehend at one glance the necessity of the accurate consideration of the superficial relations of a mountainous tract, which is in the course of investigation for the discovery of useful minerals, we have only to reflect that the external form is founded on the internal constitution of the district, and is the consequence of the uniform operation of a general cause.

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Most mountainous tracts possess so great an extent, that an investigation like that before us can with difficulty be extended over the whole at the same time, and hence, in most cases, it must be confined to a particular portion. We have not only to obtain a knowledge respecting this particular part as regards all the features already mentioned, but also to inform ourselves fully as to its relations to the whole, and to ascertain sufficiently its position relatively to the high mountain-ridge and the extent it occupies of the mountain-ridges which proceed from it. It is not only useful, but in many cases necessary, to go over, with sufficient minuteness, the neighbouring ridges to a certain extent, and also the valleys belonging to them, or by which they are separated from the particular district under investigation, in order by this method, either through the means of already existing mining operations, or a particularly good opportunity, to institute observations, to obtain indications, and to direct attention to objects, which, without it, might probably have been overlooked. It may be remarked generally, that it is very advisable, before the commencement of the actual examination of a particular district of a mountainous tract of country, to make ourselves as fully acquainted as possible with the whole existing mining operations, while these reveal *a series* of phenomena, regarding which we can, even in the most fortunate circumstances, acquire a knowledge of only the first member, by the study of the external relations. It is superfluous, with a view to the preliminary information necessary for the investigation of a separate part of a mountainous tract, to go into greater detail, and it only remains for us to mention shortly some particulars respecting the characters of such pieces of country as form the separating boundaries of different mountain-tracts, at least in their longitudinal limits.

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Characters of such pieces of country as form the boundaries of different Mountain-tracts.—It is generally a difficult matter to define with precision where a mountain-group ends and the bounding plains begin. The rocky masses which constitute the one and the others, often seem to be connected in such a manner, that even in their phenomena of superposition, no striking or fixed determinable distinction exists; but on the contrary, the latter follow the former with almost the same features of arrangement, although generally with a smaller inclination; and the superficial relations are so gradually changed, that, notwithstanding their great difference, they pass as it were imperceptibly into each other. Sometimes, however, these relations are quite of another description. There is not only an interruption as it were in the sequence of the rocky masses, inasmuch as there are certain masses awanting between those that are present, but there are likewise altered relations of superposition, of such a nature that the mountain-masses of the plains which are uniformly arranged in respect to one another, appear in a certain discordant arrangement as regards the equally uniformly arranged masses of the mountains, and the superficial relations are suddenly altered in such a manner, that soft low districts, only traversed by flat elevations, are contiguous to tracts which are precipitous and lofty, and not unfrequently include naked rocks, just as the sea borders on abruptly rising cliffs. But this determination of boundaries, as well as the more detailed account and explanation of the difference of the relations on the one and the other side of the limit, which perhaps is greater than at first sight appears, may reasonably be passed over here, as they only relate to a series of mountain-masses, viz., what are termed the secondary rocks *(Flötzgebirge**)*, which, leaving aside what cannot come into consideration on account of its constitution, consist of a small number of very slightly different mountain rocks, and a knowledge of whose relations of superposition can moreover be very easily acquired, in so far as it relates to the discovery of useful minerals.

The secondary rocks consist of sandstones, limestones (including what are termed marls), gypsum, and clay. The porphyries, syenites, greenstones, amygdaloids, basalts, and other rocks, which were formerly included among the secondary rocks, but are at present regarded by most geognosts as formations of a peculiar nature, are here excluded because there is little or nothing to be sought for in them.

The sequence of the various beds constituting the so-termed rocks of the secondary series has been enumerated with extreme minuteness, and in order to discover in this sequence a universal regularity, it has been arranged in different formations, whose examination and thorough investigation form the most important business of modern geognosy. There is, however, certainly no general agreement upon the subject; and whole formations and rocks are referred by some geognosts to the secondary series, which by others are included in the transition class. In order to reconcile other discrepancies, what are termed *equivalents* have been discovered or assumed; but, as it would appear, this plan has rather removed the object more from the eye than brought it nearer.

The following is the real state of the matter. The sequence of the individual layers of the secondary rocks is constant, in so far as one or other does not wedge out, or in so far as the rock in the course of its continuation does not become converted into another, as is very often the case with what are termed primitive and transition rocks; and they are never found, either entirely or partially, much altered or reversed.

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* It must not be supposed from this, that the secondary rocks are confined to the plains, and the transition and primitive rocks to the actual mountains, although, certainly, the former occur more frequently in the lower tracts of country, and the latter in the higher; and it must also not be forgotten that the division of mountain-masses into primitive, transition, and secondary rocks is quite arbitrary.

But this constancy is not universal, but is confined to individual, often very extensive, *basins*, which separate the mountainous tracts from one another, and consist of those plains which extend between them; and it sometimes also undergoes modifications even in these. It is possible that in neighbouring, or even in widely separated, basins, one kind of sequence may prevail in the there formed mountain-rocks, but experience teaches us, that, even in adjoining districts of this kind, this sequence is more or less different; and it will be found to present even greater differences in this respect as our knowledge of it becomes more extended, and when the eagerness to find again in other tracts, what had been found in the normal ones, shall have lost its influence on observations. This eagerness has produced a mode of reconciling the discrepancies, and of establishing a general agreement of formations, of which it is necessary to say something in this place, because a regard to it, in the search for useful minerals, can be as useful on the one hand, as it can be hurtful on the other. I allude to the application of the remains of organic beings, *i. e.* petrifactions of the animal and vegetable kingdoms, to the determination of formations, or generally of the rocky masses, constituting the so-called secondary rocks.

Estimate of the value of Petrifactions or Fossil Organic remains in the determination of Rock-formations.—The difficulty of determining the formations of the secondary series from purely geognostical characters or relations, has introduced the necessity of taking petrifactions into consideration, in order to remove it. But the agreement of the relations of the rocks and of superposition, with the phenomena of the fossil remains of organic bodies, has, in many instances, been found to be so slight, and so many important contradictions between the two have been signalised, that conclusions have thence* been deduced respecting the deceptive nature of mineralogical and geognostical characters (not their deficiency and imperfection); and at last, in such determinations, the nature of the rock, and the position of the masses, have been entirely thrown aside, and recourse has been had solely to petrifactions, and the field of geognosy has thus been entirely forsaken. Va-

* Bulletin de la Société Geologique de France.

rious extraordinary results have thus been produced, of which, however, it is not necessary to quote any here. For it requires no examples to shew, that this is not the true use which geognosy should have made of the undoubtedly extremely remarkable phenomena of the occurrence of petrifactions in different mountain-rocks.

The following incontestible propositions, which ought never to be left out of view, will render apparent the uncertainty, perhaps the impossibility, of determining formations by means of remains of organic beings, even supposing that the idea of formations were applicable.

1. The existence or the presence of remains of organic bodies does not belong to the essential characters of mountain-masses; and it is only from *such* that formations or systematic determinations can be deduced, if these are to possess *a scientific value*, and to afford *a safe application*. The determination of a mountain-mass remains the same, so long as that mountain-mass is not altered in respect to the nature of its rock and of its relations of superposition, whether it should contain petrifactions or not.

2. The species of the lower classes of animals are not yet determined in such a satisfactory manner as to enable us, at least in most cases, to be assured, that varieties of one and the same species, which are perceptibly, but not specifically, different, are not regarded as distinct species, perhaps as distinct genera. This remark is more particularly applicable to fossil remains, because, in them, several of the characters are lost which lead to the determination of living beings. In animals provided with shells, of whose remains the most important use is made, there are only separate portions, particularly the coverings, which are found fossil; and it has been observed, that very different beings of this kind inhabit entirely similar shells, just as some plants of different species or genera have perfectly similar leaves. Moreover, some possessors of extensive collections of recent and fossil shells have expected, according to their own confession, that the number of species would be multiplied in proportion as their collections were increased; but they have found that the reverse was the case, for many of the species have turned out to be varieties of one and the same species, and hence have been, as it were, blended together.

At present, we are unable to judge at what point this blending together will cease; and hence it follows, that the *real* species, between which no passages take place, are not yet ascertained. *But when the mode of determination is not yet satisfactorily settled in a science (natural history, to which palæontology belongs), no determinations, if they are to be deemed satisfactory, ought to be founded upon it in another science (that of geognosy which takes natural history for granted, without belonging to it).*

3. The age of the fossils can only be ascertained from the age of the mountain-masses in which they are found; and that, supposing that a proper idea be formed of the subject, can only be determined from the relations of superposition of the mountain-masses.* But when the relations of superposition of a mountain-mass are sufficiently known (the nature of the rock gives rise to fewer difficulties), no other assistance is requisite for the determination of its age, that is of the formation to which it belongs, provided the idea of a formation actually be a reality, which is here supposed and assumed, because the present place is not suitable for entering more deeply into the subject.

The result of these observations is, that, in the search for repositories of useful minerals, we must not trust to determinations of this kind, viz. to determinations of certain formations of secondary rocks by means of their petrifactions. I shall, in another place, speak further of the proper and legitimate use of petrifactions in this occupation.

In order to prosecute successfully the search for valuable mining repositories in secondary rocks, it is not enough to be well acquainted with the different members of the series recognised by geognosts, and to explore them when they are present in a district; but we must investigate *each separate bed*, or each individual rocky mass, to whatever formation we may be inclined to refer it, and must endeavour to discover the sequence

* Another mode of procedure for accomplishing the determination of the relative age of petrifactions, and by means of it, of formations of rocks, that, namely, by observing the relations of the remains of still living species to those of the extinct, although it may possess value in speculative geology, cannot come into consideration in an occupation where we must rely on direct observation alone.

of all in the tract under consideration. We must, at the same time, just as in the case of primitive and transition-rocks, by no means assume the universal, regular, bed-like form, although it occurs more frequently here than in these rocks ; and we must not regard tabular division as actual stratification, although it is the so-termed secondary rocks in which *actual* stratification presents itself, with all its characteristic features. It is well known regarding gypsum, that it frequently occurs in amorphous irregular masses, but limestone and sandstone, likewise, are met with in that manner ; and we must therefore attend particularly to this point, because the contractions or compressions, standing in connection with such widenings or enlargements of size, often go to such an extent, that the one or the other rocky mass disappears, at least for certain distances, and does not make its appearance where it would be expected, in the natural order of succession of strata. It is self-evident how important this feature is, in the preparation of sections of such a district.

On the Bendings and Contortions of Strata.—Another subject worthy of attention is that of the frequent bendings and contortions, partly of the real strata, partly of the mere concretionary portions, from which arise saddles, troughs, and some other similar phenomena, and the position more or less approaching to the perpendicular of the already mentioned tabular divisions, with which it has even been attempted to place in connection, by means of elevations and tiltings, the invertion of the usual order of certain mountain-masses. If it be desired to know in reality the nature of such phenomena, it is necessary for the observer to be without prejudice, and without bias to a particular theory, still not superficial, or inclined to suppose that he sees through the thing at the first glance, but, on the contrary, it is necessary for him to investigate the matter from the foundation, and without regard for the expenditure of a certain amount of time ; for, when one single such relation is quite thoroughly understood (which is not so frequently the case as is believed), it may not only be the cause of an important saving of time, but also the source of important discoveries. Should such phenomena depend merely on distinct concretions, we have already seen that the tabular masses are united with no form and with no position and direction, and their alternation cannot surprise us, how often soever it may occur; the only thing, however, which is to be attended to, is, that we do not lose the thread of the connection. Should they depend on stratification, it is easy to determine from the nature of the rock whether it be of crystalline or merely mechanical formation, and from other circumstances, if the strata are in their original position, or if they are in an altered position, as actually seems to take place occasionally in the secondary rocks under certain circumstances; and we thereby obtain, not unfrequently, an opportunity of accounting naturally and satisfactorily for such phenomena, from the accompanying circumstances. But should there, in fact, be an inversion of the order of the rocky masses, as some writers at least assert to have noticed, then we have a proof, if it be general, of what has been stated above, that we cannot expect to find the rocky masses, in reference to their mineral composition and order, in one of the specified basins, precisely the same as they occur in the other; should it, however, be only partial, then the undefined order and repetition of the alternation, the wedging out of some, and the change of rock of other beds, are important grounds of explanation, which must previously be well applied ere it be resolved to suppose and assume an event which harmonises but little with the peaceful course of nature. This seems to be the proper place to quote a passage from Delius,* in which that sound and experienced miner remarks, "I must here remind my readers that great care must be taken not to draw general rules from one single observation respecting metalliferous rocks, because the contrary is often met with in other countries and other metalliferous rocks. I must also caution them still more to beware of framing speculative ideas, and thus, by this means, restricting the operations of nature; for, on the contrary, it is necessary here to lay a foundation of varied observation, and only then to form an opinion as to how it may be supposed that nature may have operated."

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* Second edition, part 1st, page 56.

Trap Dykes.—Another circumstance which deserves attention is, that rocky masses, which, in other respects lie in regular beds, under no unusual inclination, and in regular sequence over and under one another, are bounded by vertical or more or less inclined planes, so that their strata, or the tabular masses into which they are separated, meet with their heads, as it is generally expressed. This phenomenon is explained, and with greater appearance of probability than in the so-called primitive and transition rocks, by means of great dislocations; and the proof of the correctness of this explanation is found in this, that, at the boundary, a substance is met with different from the rocks of both the mountain-masses, and which, from its form and other characters, has the aspect of having been formed in a fissure which had been produced after the existence of these masses, and which had been opened previous to the appearance of the separating mass. Greenstones and basalts more especially, but likewise rocks which are regarded as of mechanical origin, occupy these fissures. However hasty it would be to reject this explanation, yet, on the other hand, it must likewise be remarked, that there are instances of this kind in which there is no rock present, separating the rocky masses at their meeting, and even no fissure or distinct concretion-surface, but in which the two masses actually are in direct contact, are firmly united with each other, and even so pass into each other, and become mutually confounded, that we are not able to assign a fixed boundary between them; and that appearances present themselves which are less for than against this explanation. In the neighbourhood of Newcastle, it has been observed, that some of the basalt or greenstone veins (whin-dykes), notwithstanding their thickness (amounting to as much as seven fathoms), do not traverse the beds of coal with which they come in contact; and Mr Buddle, one of the most intelligent of the engineers of the coal-mines, remarks, that these and other facts are to be regarded as exceptions from the generally received opinion, "that the whin-dykes are formed by the eruption of basalt from fissures, and that they descend to an unascertained depth." "It is also doubtful," he adds, "if the basaltic dykes which traverse the coal-fields of Newcastle cut through the strata in unbroken and continuous lines."* If we were not more concerned here with the use of these phenomena in the discovery of valuable minerals than with their explanation, we should express the opinion that that explanation which comprehends all these facts, and hence is the most generally applicable, is also the most natural and most acceptable.

Caution in regard to Shifts.—Another phenomenon would seem to have been confounded with what we have been describing. It has been found that different rocky masses, often of very considerable size, meet together in the direction of their strike, without our having an opportunity of ascertaining the exact boundary, and the conclusion has thence been also deduced, that there exists a shift of immense extent. An acquaintance with the boundary or junction would have taught the observer that there is no shift, but, on the contrary, that the usual relations of superposition, accompanied by transitions, or other previously mentioned appearances, present themselves.

As the secondary rocks include districts which are bounded by two, or perhaps by several mountainous tracts, and as their relations of superposition, especially when they are not modified by particular relations of structure, or the latter taken for the former, depend upon their fundamental rock, it hence follows, that, towards the base of the mountains, they possess a more inclined place or position, and their outgoings appear more frequently at the surface than in the interior of the space they occupy, where their position approaches more nearly the horizontal, and where sections are only to be expected in con-

* "As the whin-dyke, previously described, does not pass through the Beaumont coal-seam, Mr Buddle, from whom the particulars are derived, considers that this and other facts shew it to be an exception to the generally received opinion, that whin-dykes have been formed by the basalt in a state of fusion, having always been forced upwards through the fissures into the stratification from below, and that they extend to an indefinite depth. It is also doubtful whether those basaltic fissures which occur in various parts of the Newcastle coal-field run through the strata in uninterrupted and continuous lines." *Vide* the history and description of Fossil Fuel, p. 165. Passage from the Transactions of the Natural History Society of Newcastle. 1830.

sequence of particular circumstances. However, this is not universally the case. But in cases where it is, such places, viz. at the edges of the basin, are the best suited for the investigation of these rocks, and for determining their succession in the basin. At the same time, the eminences and the hollows constituting the valleys are not to be neglected, although both in such tracts are generally less important in throwing light on the subject, than in actual mountainous districts.

Case in which Fossil Organic Remains may be used for assisting in the determination of formations.—It is quite a natural consequence of the constitution of the so-termed secondary series, and of their mode of superposition, that the thread of connection is easily lost, by means of which we can hope to attain a true knowledge and correct apprehension respecting them; for, in respect to their rocks, they contain but little variety, as they chiefly consist of limestones, including delomite, and of sandstones; and they frequently occupy a horizontal position, and one of their rocky masses covers the others for so great an extent that we can see nothing of them, and can ascertain nothing respecting them by direct observation, and are even unable to form a correct opinion of the overlying rock, because, very often, this can only be done by means of the underlying strata. It is in such circumstances that, among other auxiliaries, which cannot often be applied without some hesitation, petrifactions may be of the utmost service; and it is hence indispensably necessary to observe them with all care. But this is the only use which geognosy can or ought to make of these remains of organic beings for the determination, or in fact for the *recognition*, in one and the same basin, of a rocky mass which had been already *determined;* although these remains are likewise of great importance in other scientific respects, which cannot here be taken into consideration. For example, if we observe a certain mass of limestone in its succession below, between, or above other rocky masses, and properly determine its relations of superposition; and if, in the same limestone, we find certain petrifactions which are peculiar to it, and are generally termed *characteristic;* and if, in the same basin, we find a limestone, whose underlying and overlying strata cannot be ascertained, but discover in it the same fossils, or at least the characteristic ones, which had been met with in the other, we may thence conclude, with tolerable certainty, that it is the same limestone, and assume that it occurs in the same order in which it was observed at the first point. This procedure is certainly, in all points of view, of great utility; but, as the accurate Delius says, we must beware, or at least use every precaution,—that is, examine the relations of superposition with ample detail,—ere we assume that a limestone, which has been observed in *one* basin, is identical (in a geological sense) with another which has been found in a place which is remote from it, that is, which is separated by whole groups of mountains; for in this way many determinations have been made which are not only uncertain, but are, in part, plainly incorrect. The method of determining rocky masses by their fossil remains possesses, it is true, two advantages, which recommend it to a preference as compared with the laborious investigation of the phenomena of superposition. The first is its great convenience; for at present many formations are actually determined in the study.* The other is the appearance of learning which it carries along with it, in whose place, however, owing to the recognition and employment of the connection which prevails among the sciences, there is often found an illogical mixture of them. But it is to be expected, that those to whom is assigned and intrusted the important and serious business of searching for valuable repositories of useful minerals, will not sacrifice the success of their task either to that convenience or to that semblance.

Useful Minerals found in Secondary Rocks.—The useful minerals which may be expected in the so-termed secondary rocks are, with the exception of a few which seem to be confined to isolated points, coal, rock-salt, ironstone, lead-glance, calamine, and some copper-ores; and the repositories in which they occur are chiefly in the form of beds.

The true coal-formation is easily recognised by its composition, and other characters. It contains the best and most productive coals; but these are not always present where the coal-

* Bulletin de la Société Géologique de France.

formation exists. On the contrary, coal occurs in limestones, in sandstones, and in some other rocks, and even in beds of clay, sand, and loam, or covered by these, by basalt, and similar mineral matters; and all the coals that are found under these diversified circumstances are not of the same nature and quality. Beds of coal make their presence more readily known than beds of other useful minerals, by their outcrop; but still, it is seldom the case that their true nature is there exhibited; and it cannot, therefore, be ascertained at such points.

Rock-salt occurs in irregular masses, which are surrounded by limestone; but it likewise forms beds, generally of great extent, and irregular masses, in several other rocks, to which different ages are assigned, especially in sandstones and the newer limestones. Owing to its easy solubility, it is seldom seen on the surface of rocks; but, where it does so occur, it presents itself in whole rocks, which, in their interior, possess their natural characters, but on the surface have lost more or less by being dissolved, but have not otherwise sustained any important change. The rock of which the greater part of the mass of the irregular repository included in the limestone consists (*Das Haselgebirge*), is often exposed in the form of clay, mixed with gypsum, &c., but under these circumstances it does not always contain salt, which has either disappeared by solution, or has not originally been present; for, as the coal-formation is not unfrequently found without coal, the saliferous strata often occur without salt. The presence of rock-salt is sometimes made known by the salt springs which originate from it, and these are worthy of attention, not only on that account, but also because they themselves often form a subject of investigation.

The ironstones which occur in the rocks now under consideration are chiefly brown and clay ironstones, including under the latter the lenticular clay iron-ore, and the pea iron-ore, none of which are confined to any particular rock, except the pea iron-ore and the common clay iron-ore (an impure variety of sparry iron, mixed with clay), of which the former is generally found in cavities in limestone, and the latter generally in the coal-strata. Most of these ironstones betray their presence by their colouring properties, and their discovery is not attended with great difficulties.

Lead-glance and calamine are chiefly found in one and the same kind of repositories in limestone; and, of the occurrence of copper-ores in the secondary rocks, the copper-slate of Thuringia and Mansfeld is the most remarkable and the most interesting example.

In this short summary of the secondary rocks in relation to their repositories of useful minerals, I must, in conclusion, add the remark, that these rocks enter, for considerable distances, the valleys of the primitive and transition rocks, which extend to the plains, and that rocks of the same kind not unfrequently occur in places which are not in actual connection with the plains. Such places, as well as the generally wide valleys, are in this respect to be viewed as small plains, or isolated groups of secondary rocks, whose small dimensions facilitate exceedingly the search for repositories of useful minerals; a search which requires no particular directions.

It is not necessary to treat particularly in this place of the alluvial and tertiary formations, because those of them which appear as solid rocks are to be treated and investigated like the secondary series, and those which occur as loose incoherent masses are at the surface, and therefore require no particular preparation for their discovery.

On the Electricity and Explosive Force of Steam, and on the Economy of the Hot-blast Furnace. By Henry Meikle, Esq. Communicated by the Author.

Much curiosity has lately been excited by the electrical phenomenon observed in the condensation of the steam which escaped from a high-pressure boiler at Cramlington Colliery, near Newcastle, and which was briefly this: The engine-man, when happening to be standing where the steam, issuing from a leaky joint, was condensing into a cloud about his legs, put his hand to the lever of the safety-valve, and instantly experienced an electrical shock. Scientific men, on examining this phenomenon, as detailed in the *Philosophical Magazine* for November last, found the electricity so evolved to be positive ; but it is curious to observe how inaccurately in other respects they have expressed themselves regarding the phenomenon, calling it "the rapid production of electricity which appears to accompany the generation of steam," "the evolution of electricity by vaporization," and "the evolution of electricity during the conversion of water into vapour ;" whereas the electricity was evidently evolved by a process just the very reverse, viz. by the sudden change of transparent steam into water ; for a cloud just consists of minute drops of water. The novelty in this case seems, after all, to consist chiefly in the intensity of the electricity ; for the evolution of positive electricity, when transparent aqueous vapour suddenly assumes the form of a cloud, had been so often observed before as to leave little doubt that it will be found to hold universally when once adequate means have been devised for detecting it ; and conversely, that steam, instead of evolving electricity at its formation, must then absorb it as a necessary and constituent ingredient. Dr Faraday thinks the case observed at the Cramlington colliery "brings us much nearer to the electrical phenomena of volcanos, water-spouts, and thunder-storms than before ;" but not nearer, I presume, than Mr Espy's new theory (which, as noticed in the last number of this Journal, I had published near a dozen years ago) does ; because it just refers these phenomena to the evolution of electricity by the sudden condensation of aqueous vapour.

Water requires a higher temperature to make it boil in a glass vessel than in one of metal, and in the former, too, it boils more violently, probably because the glass, being a bad conductor, cannot so well supply the electricity for the formation of steam. When, therefore, the lower part of a steam-boiler becomes incrusted with a bad conductor of electricity, the temperature may be raised till the water boil so violently, especially under the feeble pressure of the imperfectly formed steam, as to reach the upper parts of the boiler, which, having little or no incrustation, may then supply the electricity so suddenly to the water and steam already at a high temperature, as to increase the force of the steam to a degree which occasions a violent explosion before there is time for the safety-valve being acted on, or giving sufficient vent for the steam. But this I only mention as one of the various causes to which explosions may be ascribed.

Since air, whilst it expands by having its temperature raised under a constant pressure, absorbs a considerable portion of latent heat, and still more if the pressure be decreasing, it probably also at same time, like steam, absorbs a corresponding portion of electricity. Now, I presume it is on this principle that we are to seek for an explanation of the economy of the hot-blast furnace. When cold and compressed air is forcibly injected into the fire, the fuel and bricks, being generally bad conductors of electricity, cannot supply it with sufficient promptitude or quantity both for the expansion of the air and for the formation of the gaseous products of the combustion. Hence, the process proceeds more languidly than when the air has been previously heated, and supplied with a corresponding share of electricity.

Address to the Geological Society, delivered at the Anniversary, on the 21*st of February* 1840. By the Rev. Professor Buckland, D. D., F. R. S., Corresponding Member of the Institute of France, President of the Society.*

POSITIVE GEOLOGY.

Devonian System.—In the Home Department of Positive Geology, the most striking circumstance has been an announcement by Professor Sedgwick and Mr Murchison of the conclusion to which they were led by Mr Lonsdale's suggestion in December 1837, founded on the intermediate character of the fossils in the Plymouth and Torbay limestone,—that the greater part of the slate-rocks of the south of Devon and of Cornwall belong to the old red sandstone-formation.

The order of the observations which have led to this important result, is nearly as follows:—

In a paper read at Cambridge, during the winter of 1836–37, Professor Sedgwick considered the fossiliferous slates on both sides of Cornwall to be of the same formation, and coeval, or nearly so, with the calcareous rocks that lie between the slates of South Devon.

In 1836 and 1837 also,† Messrs Sedgwick and Murchison proposed to

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* The biographical part of the Address is left out, owing to the crowded state of the present number of the Journal.—*Edit.*

† In August 1836, at the Meeting of the British Association at Bristol; and in a paper read before the Geological Society, May and June, 1837, now published in the Geological Transactions, Second Series, vol. v. Part iii.

transfer the culmiferous or anthracitic shale and grits (Shillot and Dunstone) of *North* Devon to the carboniferous system; withdrawing them from the greywacke in which they had before been included, and thus assigning a much more recent date than heretofore to the strata which occupy nearly one-third part of the map of Devonshire.

But the relations of the slates and limestones of *South* Devon still remained to be determined; the mineral characters of the former being different from those of the old red sandstone beneath the carboniferous group in many parts of South Wales and in Herefordshire, while the true position of the limestones (*e. g.* those of Plymouth, Torbay, and Newton Bushell) was doubtful. At this period (1837), the fossils of this district were examined by Mr Lonsdale and Mr Sowerby, to whom the organic remains both of the carboniferous and Silurian systems were familiar. It was soon perceived, that, while some of the South Devonshire fossils approached to those of the carboniferous strata, and others to those of Siluria, there were still many species which could not be assigned to either system; the whole, taken together, exhibiting a peculiar and intermediate palæontological character. Mr Lonsdale therefore suggested, that the difficulties which had perplexed this inquiry could be removed by regarding the limestones of South Devon as subordinate to slaty rocks, which represent the old red sandstones of Hereford, Wales, Scotland, and Ireland,—their true place in the series of Devonshire being intermediate between the culmiferous basin of North Devon, and the Silurian strata,—if the latter exist in that county.

The value of this suggestion was not at first appreciated; but after the lapse of more than a year, Mr Lonsdale's views were adopted (March 1839) by Messrs Sedgwick and Murchison,* who soon afterwards applied this new arrangement not only to the groups of Devonshire originally under review, but, with a boldness which does credit to their sagacity, extended it to the whole of the slaty and calciferous strata of *Cornwall*, till then known only as greywacke, clay-slate, or killas; assigning to those strata, likewise, the date of the old red sandstone, and resting this determination entirely on the character of the fossils. This change—the greatest ever made at one time in the classification of our English formations—was announced in a memoir read before the Geological Society in April

* It is to be observed here, that Mr Murchison, having previously shewn that the fossils of the Silurian era are distinct from those of the carboniferous period, had also pointed out "the vast accumulations" (in which few fossils had at that time been discovered) "then known to separate the two systems." He mentions especially, that "the *fishes* of the old red sandstone—entirely distinct as to form and species—are as unlike those of the Silurian system, as they are to those of the overlying carboniferous system;" adding, "that he has no doubt, although at present unprovided with geological links to connect the whole series, that such proofs will be hereafter discovered, and that we shall then see in them as perfect evidence of a transition between the old red sandstone and carboniferous rocks, as we now trace from the Cambrian, through the Silurian, into the old red system."—See *Silurian System*, p. 585, line 22, *et seq.*

1839; the authors then also proposing for the whole series (including both the old red sandstones of Herefordshire, and the fossiliferous slates and limestones of South Devon and Cornwall) the new name of "*the Devonian system*," and expressing their belief that many of the groups hitherto called greywacke, in other parts of the British Islands and on the Continent, would ere long be referred to the same geological epoch.

The proposed alteration, therefore, will terminate the perplexity hitherto arising from the circumstance that the *old* red sandstone of Werner has been frequently confounded with the *new* red sandstone formation of English geologists. It also explains the cause of the English old red sandstone having been rarely recognised on the Continent:—for if the Devonian slates afford the normal type of this formation, whilst the marly sandstones and conglomerates of Herefordshire are abnormal exceptions in it, we see the reason why their slaty Continental equivalents, like the greater part of the South Devon slates, have been referred to the undivided Wernerian formation of greywacke.

Mr Austen, in a communication relating to the structure of the south of Devon, has identified the calcareous slate and limestone of the south of Cornwall with the limestones of this district, and considers that of Torbay among the newest deposits in the latter series.

The Rev. D. Williams also has communicated two papers respecting these disputed rocks, which he refers to the transition or greywacke system, and endeavours to shew that the strata of Devonshire can be distinguished into certain groups by their lithological characters.

Mr De la Beche, in his map of Devon and Cornwall, published in 1839, has adopted divisions of the strata similar to those of Professor Sedgwick and Mr Murchison as to their order of sequence; applying, provisionally, to the culmiferous rocks the name of *Carbonaceous series*, and to the Devonian and Cornish slates the appellation of *Greywacke*.

We know also, on the authority of Mr De la Beche, that tin-mines are worked in carbonaceous rocks at Owlescomb, near Ashburton, on the east side of the Dartmoor granite, and on its west side at Wheal Jewel, near Tavistock. He further informs us that one of the richest tin-mines now worked in Cornwall, namely the Charlestown mine, east of St Austle, is in a fossiliferous rock containing encrinites and corals, and that the same corals occur also near tin-mines at St Just; and in the neighbourhood of Liskeard the Rev. D. Williams has found slates which contain vegetable impressions dipping under other slates which are intersected by lodes of tin and copper.

From these new facts, we learn that the killas and other slate-rocks of Cornwall and the south of Devon do not possess the high antiquity which has till lately been imputed to them; and that tin occurs, as copper, lead, and silver have long been known to do, not only in slate-rocks that contain organic remains, but even in the coal-formation.

Soon after the publication of the views of Messrs Sedgwick and Murchison, a similar change was applied by Mr Griffith to the south-west portion of his geological map of Ireland. In a paper that accompanied the presentation of this map to us on 22d of May last, he states that he has now coloured, as old red sandstone and carboniferous limestone, extensive districts of the counties of Kerry, Cork, and Waterford, previously considered of higher antiquity; imputing his former erroneous opinion to the identity in lithological character of the shales and grits of the old red sandstone and carboniferous systems, with the older rocks in the transition-series.

Mr Griffiths has also demonstrated by sections the unconformable position of the carboniferous and old red sandstone-formations, which overlie older and more highly inclined slates in the counties of Kerry, Cork, Waterford, and Wexford.

Mr Charles William Hamilton has likewise adopted similar changes; and believes that the slates which occupy a large space between the Mourne Mountains and Dublin are equivalent to those near Cork, which he now transfers to the old red sandstone.

Mr Greenough, in the new edition of his map of England, represents nearly the same boundaries and order of succession in Devon and Cornwall as we find in the maps of Mr De la Beche and Messrs Sedgwick and Murchison; but in his memoir connected with the map, adopting the name of *Carbonaceous series* for the culmiferous rocks, he substitutes that of *Upper killas* for the Devonian system of Sedgwick and Murchison (including under that term the old red sandstone of Herefordshire), and *Lower killas* for the slates inferior to the Silurian system, which they have termed Cambrian.

Mr Greenough, in his memoir, also shews by quotations from Dr MacCulloch, that the undisputed old red sandstone of the north of Scotland exhibits, at intervals, the same great changes of mineral character that occur in the strata intermediate between the carbonaceous and Silurian systems in the west of England and on the borders of Wales; and justly infers the inadequacy of any one term to characterize formations which vary so much in lithological composition, that at one place they present the condition of a fine-grained silky slate, at another of sandstone, and at a third that of coarse gravel and conglomerate rock.

Thus, with respect to the slate-rocks of Devon, Cornwall, and Wales, the difficulties are reduced to those of an unsettled nomenclature; whilst nearly all parties are in unison as to the fundamental fact of referring the slates of South Devon and Cornwall to the epoch of the old red sandstone-formation. The term greywacke, however, I rejoice to think, will not be condemned to the extirpation which has been threatened from the nomenclature of geology; it may still retain its place as a generic appellative, comprehending the entire transition-series of the school of Freyberg, and divisible into three great subordinate formations:—the Devonian system of Sedgwick and Murchison being equivalent to the upper greywacke, the Silurian to the middle greywacke, and the Cambrian system to the lower.

In this threefold distribution of the vast series of strata which have hitherto been indiscriminately designated by the common term greywacke, we are, as it were, extending the progressive operations of a general inclosure act over the great common field of geology; we propose a division, founded on measurements, surveys, and the study of organic remains, analogous to that of the secondary strata, from the chalk downwards to the coal-formation, established by William Smith, and to the separations of the once-undivided territory of the great tertiary system, effected by Cuvier and Brongniart, Desnoyers, Lyell, and Deshayes.

To the uninitiated in geology, rectifications in the distribution of strata upon so large a scale may seem calculated to shake confidence in all the conclusions of our science; but a contrary inference will be drawn by those who know that these corrections have never been applied to conclusions established on the sure foundation of organic remains, but to those rocks only of which the arrangement had been founded on the uncertain character of mineral composition.

Coal-Formation.—The Society has received from Professor Ansted a paper on the carboniferous and transition rocks of Bohemia, a country which he visited last summer, directing especial attention to the district between Prague, Luditz, and Pilsen, which he has illustrated by sections made from personal observation. Above the fundamental granite and gneiss he found extensive deposits of greywacke, on which lie, in unconformable superposition, disconnected patches of the coal-formation. The age of this coal is well known, from the fossil flora of Count Sternberg, who resided in the midst of it near Swina, to be identical with that of the great coal-formation of England. Mr Ansted gives information also as to the action of trap-rocks in producing disturbances of the strata in this district; and respecting dislocations, by which the greywacke is several times placed on a level with the coal-measures, whilst in some cases the strata are inverted and the coal-measures laid beneath the greywacke.

We have received an interesting communication from Mr Hawkshaw respecting a remarkable disclosure made in the Bolton railway, six miles north of Manchester, of five fossil trees in a position vertical to the plane of the strata in which they stand. The roots are imbedded in a soft argillaceous shale immediately under a thin bed of coal. Near the base of one tree, and beneath the coal, more than a bushel of hard clay nodules was found, each inclosing a cone of *Lepidostrobus variabilis*. The bark of the trees was converted to coal, from one-quarter to three-quarters of an inch thick; the substance which has replaced the interior of the trees is shale; the circumference of the largest of them is 15½ feet at the base, 7½ at the top, and its height 11 feet. One tree has spreading roots, four feet in circumference, solid and strong. By the care of Mr Hawkshaw these trees have been preserved, and a covering is erected over them. The attendant phenomena seem to shew that they grew upon the strata that lie immediately beneath their roots.

Mr Barber Beaumont, in a communication respecting these same trees, considers that no drifted plants occur in coal-fields, and that all the vegetables which are now converted into coal, grew upon swampy islands covered with luxuriant vegetation, which accumulated in the manner of peat-bogs; that these islands, having sunk beneath the sea, were there covered with sand, clay, and shells, till they again became dry land, and that this operation was repeated in the formation of each bed of coal. In denying altogether the presence of drifted plants, the opinion of the author seems erroneous; universal negative propositions are at all times dangerous, and more especially so in geology: that some of the trees which are found erect in the coal-formation have not been drifted, is, I think, established on sufficient evidence; but there is equal evidence to shew that other trees and leaves innumerable which pervade the strata that alternate with the coal, have been removed by water to considerable distances from the spots on which they grew. Proofs are daily increasing in favour of both opinions: viz. that some of the vegetables which formed our beds of coal grew on the identical banks of sand, and silt, and mud which, being now indurated to stone and shale, form the strata that accompany the coal; whilst other portions of the plants have been drifted to various distances from the swamps, savannahs, and forests that gave them birth, particularly those that are dispersed through the sandstones, or mixed with fishes in the shale-beds.

The cases are very few in which I have ever seen fossil trees, or any smaller vegetables, erect and petrified in their native place. The cycadites and stumps of large coniferous trees on the surface of the oolite in Portland, and the stems of equisetaceous plants described by Mr Murchison in the inferior oolite-formation near Whitby, and erect plants which I have found in sandy strata of the latter formation near Alencon, are examples of stems and roots overlaid by sediment and subsequently petrified without removal from the spots in which they grew. At Balgray, three miles north of Glasgow, I saw, in the year 1824, as there still may be seen, an unequivocal example of the stumps of several stems of large trees standing close together in their native place in a quarry of sandstone of the coal-formation.

In a paper on the sinking of the surface over coal-mines, Mr Buddle has shewn that the depressions produced on the surface by the excavation of beds of coal near Newcastle-on-Tyne are regulated by the depth and thickness of the coal, the nature of the strata above it, and also the partial or total extraction of the beds of coal. The accumulation of water forming ponds in these superficial depressions, and the sinkings of a railway, have afforded accurate measures of the amount of the subsidences in question.

Wealden and Portland Formations.—In the north of Germany, Mr Roemer of Hildesheim has identified beneath the cretaceous system the Purbeck stone and beds of the Wealden formation, with nearly all its characteristic shells, and three minute species of Cypris. He has also found the Portland sand, and the upper and lower green sand and the Gault clay, in the north of Germany. He has, moreover, found the Wealden formation near Bottingen in the High Alps.

Chalk-Formation.—In extension of our knowledge of the chalk-formation, the Rev. J. Gunn has sent us a short communication, accompanied by a lithograph representing the columnar disposition of some Paramoudras, to the height of many feet, one above another, in the chalk of Norfolk. The history of these enormous urn-shaped flints, which were first noticed by Professor Buckland in an early volume of our Transactions, first series, vol. iv., p. 413, pl. 24, is still involved in much obscurity. Their form is most probably due to siliceous matter collected around, and penetrating throughout, the substance of gigantic spongiform bodies; but we have yet to learn the reason why they are occasionally placed in single vertical rows, almost like the joints of a basaltic column, sometimes nearly touching, but not articulating with, one another.

A paper has been read by Mr Henry Hawes Long on the occurrence of numerous *subterraneous chasms* or *swallow-holes* in the chalk on the west of Farnham, with observations on the drainage of the country near the west extremity of the highly-inclined ridge of chalk called the Hog's Back, between Guildford to Farnham. The land-springs immediately on the north of Farnham descend southwards in open gulleys over tertiary strata, until they arrive at the narrow band of chalk which passes under Farnham Park, where they are suddenly engulfed in transverse fissures or swallow-holes, through which they pass under ground to a considerable distance, and again break forth on the southern side of the chalk. Seven of these swallow-holes occur near Farnham, from some of which the water emerges in sufficient force to turn a mill. They are probably connected with subterranean faults and transverse fractures, the origin of which was coeval with the elevation of the narrow band of chalk which forms the Hog's Back, and which, near Farnham, is inclined at a high angle to the north. The water that now passes through the Farnham swallow-holes may tend to enlarge the chasms through which it takes its subterraneous course, by dissolving slowly the chalk of their sides in the small quantities of carbonic acid which rain-water usually contains.

Similar transverse fractures, on a greater scale, have given origin to the chasms, which, being enlarged by denudation into transverse valleys, afford outlets through the high escarpment of the chalk to the rivers that, rising within the Weald, flow through the escarpment of the North Downs into the valley of the Thames, and through the escarpment of the South Downs into the sea, viz. to the Wey, the Mole, the Darent, the Medway, and the Stour, through chasms in the North Downs; and to the Arun, the Aduz, the Ouse, and the Cuckmere, through chasms in the South Downs.

Dr Mitchell has communicated a paper on Artesian and other wells, in the gravel and London clay in Essex, shewing that water occurs under the London clay at various depths, the deepest at Foulness Island being 460 feet. He attributes this inequality in part to unevenness in the surface of the subjacent chalk. On reaching the chalk a large volume of water usually rushes up: Artesian wells are now general in Essex, where they are of the greatest utility in districts that have no natural springs. He also gives an interesting list of localities, both of constant and intermitting springs, some of them very powerful, that burst out from the chalk.

Dr Mitchell has also communicated an account of deleterious gases that occur in wells in the chalk and strata above it near London. The most abundant of these, namely, carbonic acid gas, issues very partially and only from certain strata, and produces sometimes effects fatal to persons employed in digging wells. Sulphuretted hydrogen is occasionally met with in chalk; and both sulphuretted hydrogen and carburetted hydrogen occur in beds immediately above the chalk.

Supercretaceous Formations.—In illustration of the history of the eocene division of the tertiary strata, Mr Bowerbank has concluded, from his personal observations at White Cliff Bay in the Isle of Wight, that there are no well-defined zoological distinctions between the London and plastic clays, but that in the cliffs of this bay the same shells are common to alternations of these clays with one another. At Alum Bay also he found many London clay fossils in beds of greenish-grey sand and clay below the variegated sands and clays referred by Mr Webster to the plastic clay. A similar rectification was some time ago proposed by Professor Sedgwick.

We have also witnessed, during the past year, the commencement of a valuable publication by Mr Bowerbank on the fossil fruits and seeds of the London clay, illustrated with very numerous and accurate engravings by Mr James Sowerby.

The great attention the author has long paid to the remains of fruits and seeds which occur in such vast abundance in the Isle of Sheppey, whence he has collected not less than 25,000 specimens, place him in a position peculiarly advantageous for the object before him. In this work drawings will be given of the anatomical structure of many of these fossils, as seen under the microscope. The simple expedient Mr Bowerbank has adopted of preserving these fruits in jars of water has kept him in the entire possession of every specimen ever placed in his collection; whilst the thousands of similar fossils that have been deposited in other collections, including that at the British Museum, nearly all have perished from the decomposition of the iron-pyrites with which they are always penetrated.

Mr Lyell has communicated to us a paper full of elaborate detail of facts, and of ingenious speculations respecting the boulder formation, or drift, associated with fresh-water deposits, in the mud cliffs of Eastern Norfolk. These cliffs are in some places 400 feet high, and consist of chalk, crag, fresh-water deposits, drift mud and sand, stratified and unstratified; —with superficial accumulations of flint gravel. The centre of his observations is the town of Cromer; he considers the boulder formation to have been accumulated on land permanently submerged, and not, by one or many transient advances of water over dry land, and therefore proposes, as Mr Murchison and others have already done, to substitute the term of Drift for that of Diluvium, which many other writers have assigned to it. The drift, or diluvium, is of two kinds: one composed of sand, loam, clay, and gravel, all regularly stratified; the other consisting of clay, not divided into beds, and containing boulders of granite, trap, and other rocks.

This clay is known on the east and north-east coast of Scotland by the name of Till. He considers the stratified drift and till to be contemporaneous formations, and compares the latter to moraines formed at the termination of glaciers. He imagines that drifted masses of ice, charged with earthy matter and fragments of rock, may have deposited the till as they melted in still water, and the occasional intercalation or juxtaposition of stratified materials is ascribed to the action of currents on materials also falling from melting icebergs.

Mr Lyell refers the complicated bendings and tortuous foldings of many beds of this formation near Mundesley and Cromer to lateral pressure from drifting ice, especially where extremely contorted beds repose upon undisturbed and horizontal strata. But he admits that some of them may be due to landslips of ancient date, and which had no connection with the present line of cliffs. At the bottom of the boulder-formation, and immediately above the chalk, extensive remains of a buried forest occur, the stools of the trees being imbedded in black vegetable earth. From the position of this forest, a vertical subsidence of several hundred feet and a subsequent rise of the land to the same amount is inferred. This forest and a bed of lignite are connected with fluviatile or lacustrine deposits, which occur about the level of low water below the drift; but at Mundesley they are partly above it, and the fresh-water shells which they inclose, being nearly all of British species, shew that they, as well as the contemporaneous drift, all belong to the newer pliocene period.

In an address formerly delivered from this chair, in 1836, and in a subsequent edition of his "Principles of Geology," as well as in his "Elements," Mr Lyell has called our attention to some differences of opinion which had been expressed by several eminent conchologists as to the number of fossil shells of the crag of Norfolk and Suffolk which could be identified with living species. So great was the discordance of the results at which M. Deshayes, Dr Beck, and others seemed to have arrived, that their announcement was calculated materially to impair our confidence in the applicability of the chronological test so much relied on by Mr Lyell, for the classification of the tertiary formations; namely, that derived from the proportional number of recent and extinct species discoverable in each deposit. In the hope of arriving at some definite conclusion on this important point, Mr Lyell visited Norfolk and Suffolk during the last year, and having obtained a considerable collection from the crag near Norwich and Southwold, he instituted, with the assistance of Mr Searles Wood and Mr George Sowerby, a thorough comparison between them and recent species. The fossil shells of this formation, which the author calls the Norwich Crag, are partly marine, and partly fresh-water, and indicate a fluvio-marine origin, and the proportion of living species was found to be between 50 and 60 per cent. This deposit, therefore, the author refers to the older pliocene period. A similar examination was then made of 230 species of shells from the red crag in Mr Wood's museum, and it was found that 69 agreed with living species, being in the proportion of about 30 per cent. This group, therefore, Mr Lyell ascribes to the miocene era. A collection of 345 species of coralline crag shells in Mr Wood's cabinet was then compared in like manner, and 67 were determined to be identical with recent species, being about 19 per cent. Mr Lyell, therefore, considers that the coralline crag is also miocene, although belonging to a more remote part of that period than the red crag. Having obtained from M. Dujardin a collection of 240 shells from the Faluns of Touraine, he found, with Mr George Sowerby's assistance, that the recent shells were in the proportion of 26 per cent., so that he has now come round to the opinion long ago announced by M. Desnoyers, that upon the whole the crag of Suffolk corresponds in age with the Faluns of Touraine, both being miocene, although the species in the two countries are almost entirely distinct, those of England having a northern, and those of France a sub-tropical character. I am also informed by Mr Lyell, that out of 400 marine and fresh-water species, from the eocene strata of the London and Hampshire basins, Mr George Sowerby was scarcely able to identify two per cent. with living shells. It is satisfactory, therefore, to observe that the test of age, derived from the relative approach to the recent fauna, is in perfect accordance with the independent evidence drawn from superposition. We ascertain, for example, by superposition, that the fresh-water strata of the mud cliffs of East Norfolk rest on Norwich Crag, and are the newest formation of all. They are then followed in the descending series by 1st, the Norwich; 2dly, the red; and, 3dly, the coralline crag, beneath which is the London clay. The same order of sequence is indicated by the organic remains considered independently, and simply with reference to the degree of their correspondence with the existing fauna.

It has been known for many years, that near Bridlington, in Yorkshire, sand and clay containing marine tertiary shells had been exposed on the coast. From an examination of the shells collected there by Mr Bean, Mr Lyell finds the deposit to agree in age with Norwich Crag.

I cannot conclude these remarks without observing, that some part of the confusion and apparent inconsistency of the opinions of different conchologists, respecting the age of the crag, must have arisen from the intermixture of fossils derived equally from the Norfolk and Suffolk beds, or from strata, some of which now turn out to be referable to the older pliocene, others to the miocene period.

From an examination of some fossil shells, identical with recent species collected by Capt. Bayfield from the most modern deposit near the Gulf of St Lawrence, and near Quebec, Mr Lyell infers that the climate of Canada was colder than now during the era immediately antecedent to our own times. The shells, which were determined by Dr Beck, differ in great part from those now living in the Gulf of St Lawrence, agree more nearly with arctic genera and species, and resemble those which Mr Lyell collected at Uddevalla, in Sweden; whereas, if the living shells most abundant in the Swedish and Canadian seas are contrasted, they differ almost entirely. From notes sent by Capt. Bayfield, it appears that, at different depths in the stratified sand and clay containing the fossil shells, near Quebec, insulated boulders are numerous, which, it is presumed, have been brought down at distant intervals by drift ice, and have dropped to the bottom of the sea as the ice melted.

While Mr Lyell, by the aid of Dr Beck's determination of fossils, had adopted these views respecting the climate of Canada, Mr James Smith of Jordan Hill had been led by independent observations to a similar conclusion respecting the climate of Scotland during the newer pliocene era, arguing from the arctic character of the testacea found in the raised beds of the valley of the Clyde, and other localities. In the first of two papers communicated by this author, he regarded all the deposits abounding in recent shells in Scotland and Ireland as belonging to one group; but in his second memoir he contends that there are two distinct formations on the Clyde, in the older of which there are from ten to fifteen per cent. of extinct or unknown species of shells, which he refers to the newer pliocene system of Lyell; whereas all the species found in the newer, which he calls post-tertiary, exist also in the present seas. During this post-tertiary period, which is considered to have been anterior to the human epoch, an elevation of at least forty feet took place on the shores of the Clyde. Mr Smith affirms that the till, or unstratified accumulation of clay and boulders, belongs not to the post-tertiary, but to the older pliocene division.

Igneous Rocks.—The principal communication we have received on rocks of igneous origin has been from our secretary Mr W. I. Hamilton, who has read an interesting paper on the north-west part of Asia Minor, from the Peninsula of Cyzicus to Koola, with a description of the Katakekaumene. Between Cyzicus and Koola the principal stratified rocks are schist, with saccharine marble, compact limestone resembling the scaglia of Italy and Greece, tertiary sandstones, and tertiary limestones. The igneous rocks are granite, peperite, trachyte, and basalt. The tertiary limestones are referred to the great lacustrine formation which occupies so large a part of Asia Minor. Hot springs burst forth near Singerli from a porphyritic trap-rock. The Katakekaumene is a volcanic region, extending about seven miles from north to south, and from eighteen to nineteen east and west. It presents two systems of volcanic craters and coulées: the older of them are placed on parallel ridges of gneiss and mica slate, and the newer in the intervening valleys; hence he argues, that, when the latter eruptions took place, the lines of least resistance to subterraneous expansion were in the valleys. The streams of lava from the more recent cones are bare and rugged, like the coulées in central France. Three periods of eruption are traced: the first having produced basalt, which caps the plains of white limestone, and was ejected before the formation of the valleys; the second marked by currents of lava from the more ancient system of volcanos in action since the formation of the valleys; the third resembling the coulées of Etna and Vesuvius, and mentioned by Strabo, but of which there is no historical tradition as to the period when they were in activity.

We have a notice by the Rev. W. B. Clarke of a shower of ashes that fell on board the Roxburgh off the Cape de Verd islands in February 1839, the cause of which was not apparent. The sails were covered with a fine powder, resembling the ashes of Vesuvius, which was probably derived from an eruption in the Cape de Verd group.

Palæontology.—In the department of *Palæontology*, Prof. Owen has, during the past year, contributed many papers, with his usual zeal and ability, to the elucidation of this most essential, and perhaps most generally interesting branch of our subject. At the head of these we must place his determination of a tooth and part of the jaw of a fossil monkey, of the genus *macacus*, with part of the jaw of an opossum, and the tooth of a bat, in eocene strata of the English tertiary formation. These remains were found at Kingston, near Woodbridge in Suffolk, by Mr Colchester, in strata which Mr Lyell has referred to the London clay; thus proving the existence of quadrumanous, marsupial, and cheiropterous animals in this country during the eocene period. We have now evidence of fossil quadrumana in the tertiary formations, not only of India and Brazil, but also of France and England; respecting which Mr Owen has observed, that they appear under four of the existing modifications of the quadrumanous type: viz. the tailless ape (*Hylobates*), found fossil in the South of France; the gentle vegetable-feeding *Semnopithecus*, found fossil in India; the more petulant and omnivorous *Macacus*, found in Norfolk; and the platyrrhine *Callithrix*, found in Brazil. This genus is peculiar to America, and its extinct species is of more than double the stature of any that exists at the present day. This geographical distribution of quadrumana adds further weight to the arguments derived from the tropical aspect of vegetable remains that abound in the London clay at Sheppey, shewing that great heat prevailed in the European part of the world, as well as in India and South America, during the eocene period.

The probability of high temperature is further corroborated by Mr Owen's recent recognition of four petrified portions of a large serpent (*Palæophis Toliapicus*), eleven feet long, and in several points resembling a boa, or python; and also of a bird allied to the vultures (*Lithornis vulturinus*), all from the London clay of the isle of Sheppey; wherein the occurrence of fossil Crocodilians and Testudinata, and of fossil fruits, having a tropical aspect allied to cocoa-nuts and many other fruits of palms, has been long known. Can we account for these curious facts without supposing that, at the eocene period of the tertiary epoch, the very clay on which London now stands was in the condition of a nascent spice-island, its shores covered with basking reptiles, and the adjacent lands waving with cardamoms and palms, and thuias and cypresses, with monkeys vaulting and gamboling upon their branches, and gigantic serpents entwined around their trunks; the seas also swarming with sting-rays and saw-fishes, with chimæras and enormous sharks? for all these, together with countless shells of pearly nautili, occur among the fossil remains of the numerous extinct species of fishes, which, during the early ages of the tertiary period, crowded the tepid seas of our now humid and chilling climate.

Mr Owen has also determined the character of a new genus of pachydermatous animal (*Hyotherium*) intermediate between the Hyrax, hog, and Chæropotamus, found in the London clay at Herne Bay, near Margate, by Mr Richardson.

Mr Lyell having submitted to Mr Owen some fossil teeth from the red crag of Newbourne in Suffolk, they proved to be referable to the leopard, bear, hog, and a large kind of deer, and afford the first example of mammalian remains being found in England in any of those divisions of the crag which Mr Lyell, in a paper already alluded to, has ascribed to the miocene period: these genera are known to occur in the miocene formations of France and Germany. The numerous mammalia in the fluvio-marine crag of Norwich are decidedly of a later date; among these Mr Lyell enumerates the teeth and jaw of *Mastodon longirostris*, a tusk of an elephant with serpulæ attached, and bones of a horse, hog, and field-mouse; there occur bones of birds, many fishes, and numerous shells, partly marine, and partly fresh-water and terrestrial.

The recent discoveries in Brazil by Dr Lund of extinct mammalia, that probably lived in some late portion of the tertiary epochs, form a new and important chapter in palæontology. The largest of these are referable to more gigantic forms than at present exist of families now peculiar to South America—*e. g.* to sloths and armadillos; just as most of the fossil mammalia of New Holland belong to families and genera which are still peculiar to that country. In a paper on one of these animals from Buenos Ayres, Mr Owen has shewn that the bony armour, which several authors have referred to the megatherium, belongs to the glyptodon, an animal allied to the armadillo, and of which a head containing teeth, and attached to a tessellated bony covering of the body and tail, resembling those of an armadillo, has been lately found near Buenos Ayres, and is figured by Sir Woodbine Parish in his interesting work on that country, 1838.

The Glyptodon differed from the megatherium in the structure and number of the teeth, and from all known armadillos in the form of the lower jaw, and the presence of a long process descending from the zygoma; and approached in both these respects to the megatherium. The teeth differ from those of armadillos, in having two deep grooves both on the outer and inner surface, are more complex than those of any known edentate, and indicate a passage from that family into the toxodon. The ungual phalanges are wholly unlike those of the Megatherium, and most nearly resemble those of Dasypus, but are short, broad, and flat, and seem to have been covered with hoof-like claws. The form of the foot most nearly resembled that of the fore-foot of the mole. Having appropriated to the Glyptodon the armour supposed to belong to the megatherium, Mr Owen next proves that the latter animal was unprovided with any such bony covering, arguing from a comparison of its vertebral column and pelvis with that of the armadillo; and from the absence of the oblique processes, which in the loricated edentata resemble, as to form and use, the *tie-bearers* in carpentry, that support the weight of a roof. The vertebral conditions of the Megatherium are nearer to those of the sloths and ant-eaters. We have accounts of twelve skeletons of Megatherium, not one of which was found to be accompanied by bony armour. Cuvier considered the Megatherium more nearly allied to the ant-eaters and sloths than to the armadillos.

Captain Martin has found that many parts of the bottom of the English Channel and German Ocean contain in deep water the bones and tusks of elephants. They have been dredged up between Boulogne and Dungeness, in the mid-sea between Dover and Calais, and at the back of the Goodwin Sands; also midway between Yarmouth and the coast of Holland. In 1837, a fisherman inclosed in his net a vast mass of bones between the two shoals called Varn and Ridge, that form a line of submarine chalk-hills between Dover and Calais. Captain Martin says these bones do not occur on the top of banks or shoals, but in deep hollows or marine valleys. Sir John Trevelyan possesses the molars of a large elephant from gravel in the bed of the Severn, near Watchet, and we have long known that the bones of elephants occur in great abundance in the oyster grounds off Yarmouth.

In subterranean ornithology three important discoveries have been made during the past year; the first in the eocene formation by Professor Owen, who has recognised the fossil vulture before alluded to in the London clay of Sheppey; the second by Lord Cole and Sir P. Egerton, who have acquired from the chalk of Kent the humerus of a bird most like that of an albatross, but of larger and longer dimensions; the third by Professor Agassiz, who has found in Switzerland a nearly entire skeleton of a small bird (not unlike a swallow), at Glaris, in the indurated blue slate-beds of the lower region of the chalk-formation. We know that the bones of a wader, larger than a heron, have been found by Mr Mantell in the Wealden formation of Tilgate Forest; and that the Ornithichnites in the new red sandstone of Connecticut have been referred to seven species of birds.

We have an interesting accession to our knowledge of the anatomy of the Ichthyosaurus in Mr Owen's description of the hinder fin of an *Ichthyosaurus communis,* discovered at Barrow-on-Soar by Sir Philip Egerton;

this fin distinctly exhibits on its posterior margin the remains of cartilaginous rays that bifurcate as they approach the edge of the fin, shewing in this respect a new approximation to the fin of a fish, and more fully justifying the propriety of the name Ichthyosaurus. Traces are also preserved of scutiform compartments on the integument of the fin. It is singular that this structure should never have been observed in any of the numerous specimens from Dorset and Somerset that have come under our notice; whilst at Barrow-on-Soar, from whence the paddle in question was derived, even the fibres of the skin and folds of the epidermis are sometimes accurately retained.*

Mr Owen's first part of his report on fossil Saurians, read at the British Association at Birmingham in August last, forms the commencement of a most important addition to the history of extinct reptiles. His recent investigations in odontography have also supplied to the geologist a new and most efficient instrument of investigation, enabling him to distinguish genera of extinct animals by the microscopic structure of their teeth; and as, of all fossil remains, the teeth are the parts most perfectly preserved, and in the case of cartilaginous fishes the teeth and spines are generally the only parts that have escaped decomposition, this method assumes an especial importance in fossil ichthyology, as affording exact characteristics of animals long swept from the surface of the earth, and whose very bones have been obliterated from among the fossil witnesses of the early conditions of life upon our planet. By this microscopic test applied to the family of sharks, Mr Owen has confirmed the views of Agassiz, respecting the affinities between the living cestracion and the extinct genera Acrodus, Ptychodus, Psammodus, Hybodus, Cochliodus; in the case of animals also of the higher orders, he has settled the much-disputed places of several extinct gigantic mammalia by the same unerring test. Thus he has shewn the supposed reptile Basilosaurus to be a cetaceous mammifer, allied to the dugong; the Megatherium to be, as Cuvier had considered it, more nearly allied to the sloth than to the armadillo; and the Saurocephalus to be, as Agassiz had supposed it, an osseous fish.

Dr Malcolmson, in a memoir on the old red sandstone of the north of Scotland, has done important service in shewing that the rocks composing that group are divided into three formations, the two lower of which are clearly distinguished from each other by their fossil fishes. The cornstone or central formation is charged with numerous remains of ichthyolites, including *Holoptychus nobilissimus*, a new species of Cephalaspis, and other forms not yet described. The lower division, consisting in this region of conglomerates, shales, and sandstone, is characterized by the genera Dipterus, Diplopterus, Cheiracanthus, &c. of Agassiz, as well as by the occurrence of a singular ichthyolite, which seems to offer close analogies to certain forms of crustacea. By help of these ichthyolites, the

* See Buckland's Bridgewater Treatise, Pl. 10.

author has been enabled to connect certain strata of Orkney and Caithness, and determine their relations to the beds of old red sandstone containing fossil fishes in the basin of the Tay, and in the border counties of England and Wales, where they had been described by Mr Murchison.

Mr Williamson, in a notice on the fossil fishes of the coal-fields of York and Lancaster, says that these coal-measures are very rich in ichthyolites, which abound so much at Middleton colliery, near Leeds, that the workmen have given to one bed the name of Fish Coal. They are usually in fine bituminous shale above and below the coal, and most frequent in the roof immediately above it, where, as at Burdiehouse near Edinburgh, there is a thin seam of coprolitic matter; they are rarely mixed with any great quantity of vegetable remains. In the lower measures of Lancashire, they are associated with Goniatites and Pectens; and in the higher measures of Lancashire and Yorkshire, with fresh-water shells allied to Unio, and with Entomostraca. Exact observations as to facts of this kind are of inestimable importance, for it is only by careful induction from a sufficient number of such like phenomena, and from similar details as to the local distribution and condition of animal and vegetable remains in the marine and fluvio-marine and lacustrine deposits, which compose the carboniferous series, that we shall arrive at a solution of the grand problem of the formation of coal.

Crustaceans.—The Rev. T. B. Brodie has discovered in the Wealden formation near Dinton, in the vale of Wardour, the remains of coleopterous and hymenopterous insects, and a new genus of *isopodous crustacea* in the family of Cymothoidæ. The Isopods are clustered densely together; the lenses in their eyes are sometimes preserved; there are also traces of legs, but of no antennæ. With them he has found a large species of Cypris. The insects are chiefly small coleoptera; there are several species of dipterous, and one homopterous insect, and the wing of a Libellula. Mr Brodie's discovery is the first yet made of insects in the Wealden formation, and also the first example in a secondary formation of isopods that approximate in form to the trilobites of the transition series.

Worms.—An addition has been made to fossil helmintology by Mr Atkinson of Newcastle-on-Tyne, who has found in slabs of micaceous slaty sandstone, from the carbonaceous series near Haltwhistle, tortuous casts of vermiform bodies of various sizes, some almost an inch in diameter, and several feet in length; the surface of many of these is thickly marked by transverse rings and a longitudinal groove, similar to those in the largest recent marine sand-worms, *e. g.* the *Leodice gigantea.* The integument of some of these worms containing *chitine,* like the covering of insects, seems to have endured long enough to fix impressions of the transverse rings upon the sand; and the habit of swallowing large quantities of earth and sand, which we observe in many recent worms, may explain the presence of the large portion of sand, now indurated to stone, which occupies the interior of the impression of the skin. Since many casts are found upon the same slab, these worms must have been very numerous at the bottom of the sea, when the sandstone was in process of formation. Similar impressions of annelids on the Cambrian rocks are figured by Mr Murchison in pl. 27 of his great work on the Silurian System.

Ichnology.—About twelve years ago we witnessed the creation of a new department in geological investigations, viz. the science of ichnology, founded on the evidence of footsteps made by the feet of animals upon the ancient strata of the earth; this new method commenced with the recognition of the footmarks of reptiles on the new red sandstone near Dumfries, and not long after (1834) was followed by most curious and unexpected discoveries in Saxony and America. The Chirotherium of Hessberg and Ornithichnites of Connecticut were among its early results. Our own country has, during the last two years, been abundantly productive of similar appearances in many localities.

In recent excavations for making a dock at Pembray, near Llanelly, in Pembrokeshire, tracks of deer and of large oxen have been found on clay subjacent to a bed of peat, the lower peat being moulded into the footsteps; similar impressions were also found upon the upper surface of the peat beneath a bed of silt, and bones both of deer and oxen in the peat itself. Footmarks of deer have been also noticed in Mr Talbot's excavations for a harbour near Margam burrows, on the east of Neath.

Near Liverpool Mr Cunningham has successfully continued his researches, begun in 1838, respecting the footsteps of chirotherium and other animals in the new red sandstone at Storeton Hill, on the west side of the Mersey. These footsteps occur on five consecutive beds of clay in the same quarry, the clay-beds are very thin, and having received the impressions of the feet, afforded a series of moulds in which casts were taken by the succeeding deposits of sand, now converted into sandstone. The casts of the feet are salient in high relief on the lower surfaces of the beds of sandstone, giving exact models of the feet and toes and claws of these mysterious animals, of which scarcely a single bone or tooth has yet been found, although we are assured by the evidence before us of the certainty of their existence at the time when the new red sandstone was in process of deposition.

Further discoveries of the footsteps of Chirotherium and five or six smaller reptiles in the new red sandstone of Cheshire, Warwickshire, and Salop, have been brought before us by Sir P. Egerton, Mr I. Taylor jun., Mr Strickland, and Dr Ward.

Mr Cunningham, in a sequel to his paper on the footmarks at Storeton, has described impressions on the same slabs with them, derived from drops of rain that fell upon thin laminæ of clay interposed between the beds of sand. The clay impressed with these prints of rain drops acted as a mould, which transferred the form of every drop to the lower surface of the next bed of sand deposited upon it, so that entire surfaces of several strata in the same quarry are respectively covered with moulds and casts of drops of rain that fell whilst these strata were in process of formation.

On the surface of one stratum at Storeton, impressed with large footmarks of a chirotherium, the depth of the holes formed by the rain drops on different parts of the same footstep has varied with the unequal amount of pressure on the clay and sand, by the salient cushions and retiring hollows of the creature's foot; and, from the constancy of this phenomenon upon an entire series of footmarks in a long continuous track, we know that this rain fell after the animal had passed. The equable size of the casts of large drops that cover the entire surface of the slab, except in the parts impressed by the cushions of the feet, record the falling of a shower of heavy drops on the day in which this huge animal had marched along the ancient strand; hemispherical impressions of small drops, upon another stratum, shew it to have been exposed to only a sprinkling of gentle rain that fell at a moment of calm.

In one small slab of new red sandstone found by Dr Ward near Shrewsbury, we have a combination of proofs as to meteoric, hydrostatic, and locomotive phenomena, which occurred at a time incalculably remote, in the atmosphere, the water, and the movements of animals, and from which we infer with the certainty of cumulative circumstantial evidence, the direction of the wind, the depth and course of the water, and the quarter towards which the animals were passing; the latter is indicated by the direction of the footsteps which form their tracks; the size and curvatures of the ripple-marks on the sand, now converted to sandstone, shew the depth and direction of the current; the oblique impressions of the rain drops register the point from which the wind was blowing, at or about the time when the animals were passing.

Demonstrations founded solely upon this kind of circumstantial evidence were duly appreciated, and are well exemplified, by the acute author of the story of Zadig; who, from marks he had noticed on the sand, of its long ears, and teats, and tail, and from irregular impressions of the feet, declared the size and sex, recent parturition and lameness, of a bitch he had never seen; and who, from the sweeping of the sand, and marks of horse-shoe nails, and a streak of silver on a pebble that lay at the bottom of a single footstep, and of gold upon a rock against which the animal had struck its bridle, inferred that a horse, of whose existence he had no other evidence, had recently passed along the shore, having a long switch tail, and shod with silver, with one nail wanting upon one shoe, and having a bridle studded with gold of twenty carats' value.

In addition to the commencement of Mr Bowerbank's publication on the fossil fruits and seeds of the London clay, before alluded to, we have hailed with satisfaction the announcement by Professor Henslow and Mr Hutton, of their intended continuation of the fossil flora of Great Britain, conducted for some years by Dr Lindley and Mr Hutton, and lately suspended.

A dictionary of the terms and language of geology has long been a desideratum to young students, to whose early progress the technical terms of the science have hitherto presented formidable impediments. This want has been recently supplied by two publications of this kind, one by Mr George Roberts, author of the History of Lyme Regis; the other by Dr Humble.

During the last year the society has received no communication on mineralogy; and almost the only volume that has been published in England on this much-neglected subject, has been a small but highly elaborate treatise on crystallography by Professor Miller, of the University of Cambridge. In this treatise the author has adopted the crystallographic notation proposed by Professor Whewell in his paper on a general method of calculating the angles of crystals, and the laws according to which they are formed, published in the Transactions of the Royal Society of London, 1825; and Professor Neuman's method of indicating the positions of the faces of a crystal by the points in which radii, drawn perpendicular to the faces, meet the surface of a sphere. The expressions which have been thus obtained are remarkable for their symmetry and simplicity, and are all adapted to logarithmic computation, and for the most part new.

Geological Notes made during a Journey from the Coast into the Interior of the Province of Ceará, in the North of Brazil, embracing an Account of a Deposit of Fossil Fishes. By George Gardner, Esq., of Glasgow. Communicated by J. E. Bowman, Esq., of Manchester.

The knowledge of the fact that but little, if any thing, of the geology of this province has hitherto been made known, induces me to offer the following sketch of those parts of it which I have visited, drawn up from the daily notes of my journal. However imperfect it may be, I trust that it will not be altogether useless, as it gives at least a general idea of the structure of a part of this great empire, which, so far as I can learn, has never before been visited by any of the many naturalists who, for the last twenty years, have at various times traversed other portions of it.

The province of Ceará, which is situated between the third and eighth degrees of south latitude, and the thirty-seventh and forty-first of west longitude, is bounded by the sea to the north; by the provinces of Rio Grande do Norte, and Parahiba to the east; by that of Pernambuco to the south; and to the west by a low mountain range which divides it from the great inland province of Piauhy. I landed at the north-east corner of the province at the town of Aracatý, which is situated on the east bank of the Rio Jaguaribe, at three leagues from the coast. The first thing that struck me on my arrival was the flatness of the country around it, reminding one of the descriptions which are given of the pampas of Buenos Ayres. With the exception of a few low sand-hills towards the sea, and a round isolated one about 800 feet high, situated two and a half leagues to the south-west of the town, called the Serra de Areré, these is nothing to interrupt the uniform level. The soil for many leagues around is of a sandy nature, and the characteristic vegetation is a beautiful species of palm called Carnahuba by the Brazilians. It is the *Corypha cerifera* of Martius, and is so abundant that, on my journey south to the Villa do Icó, I rode for about two days through a forest of almost nothing else. Two and a half leagues to the south of Aracatý, I first met with rocks. This was on crossing the river at a place called the Passagem das Pedras. I found them to consist of thin strata of gneiss, almost in a vertical position. The little inclination which they had was towards the north-west, in the direction of the above-mentioned Serra de Areré. From this place to the villa do Sañ Bernardo, a distance of a little less than eight leagues, the country continues perfectly flat, but the ground among the Carnahuba palms, and in several large open spaces almost destitute of vegetation, called vargims, is covered with abundance of gravel; and this, which extends over large tracts, gives it the appearance of the dried-up bed of an immense river. Intermingled with this gravel, there are numerous boulders of various sizes, the largest I saw not being more than four feet high. They are all more or less rounded, and consist of granite, gneiss, and quartz. For the next ten leagues the country continues nearly of the same character, with the exception of a low range of gravelly hills, running from east to west, and wooded with shrubs and small trees, the most common of which is a species of Mimosa. During the next ten leagues, a slight but perceptible rise of the country takes place. The soil is generally a yellow-coloured clay, in many places thickly covered with gravel and boulders, while in others gneiss-rocks are seen cropping out, and forming often long slightly elevated ridges covered with a species of Cactus, and a large Bromelia. Their strata, like those farther down, are almost vertical. In this tract Carnahuba palms become less numerous, and small dicotyledonous trees and shrubs more abundant, but all of them in the dry season, at which time I passed, destitute of leaves. These deciduous woods, which often cover large tracts of country, are called Catingas by the Brazilians. These tracts are still farther characterized by three large species of Cacti belonging to the genus Cereus. During the next twenty leagues, which brought me to the Villa do Icó, the nature of the country differs in again becoming more level, consisting of large open campos or vargims, the vegetation of which, during the dry season, is quite burnt up; but they are said to yield abundance of grass during the rains; and the Catingas or deciduous woods are much larger than they are farther down. The rocks are gneiss and quartz, and in several places large tracts are covered with fragments of the latter more or less rounded. At about ten leagues below Icó, the monotonous level of the country is varied by a mountain range, which makes its appearance to the eastward. This is the Serra de Pereira. It runs from the south-west to the north-east. It is sixteen leagues in length, but its greatest height is not more than 1000 feet above the level of the plains in which it is situated. The structure of its south-west extremity, at least, is entirely primitive, but near its base I observed a coarse red conglomerate containing rounded fragments of both primitive and secondary rocks.

The Villa do Icó, which is one of the finest in the interior of the north of Brazil, is situated on the east bank of the Rio Jaguaribe, in the middle of one of the large open campos which I have already described, and during the dry season is one of the most miserable places imaginable to live in. The country around it is then so much dried up that not a green leaf is to be seen; and the river, which during the rains is of considerable size, becomes quite dry. The houses are all built of brick, which are made from a very good kind of clay found in the neighbourhood, and are all whitewashed on the outside with a white limestone, which is found about ten leagues to the west of the villa.

From Icó I went to the Villa do Crato, which is about thirty-four leagues to the south-west of the former place. Between these two places the country is of a more hilly undulating character, more abundantly wooded, the trees larger, and many of them evergreen. Owing to these circumstances but few of the large campos, which exist below Icó, are met with. The carriage of goods between Aracatý and Icó is effected in large waggons, generally drawn by twelve oxen; but the hilly nature of the country between Icó and Crato does not admit of this mode of conveyance, the backs of horses, and even of oxen, being made use of instead. Shortly after leaving Icó I passed over the south-west end of the Serra de Pereira, at a place where it has but a slight elevation, and consists entirely of gneiss. From this place to the Villa das Lavras da Mangabeira, a distance of about ten leagues, the country is of a gently undulating nature, and in many places well wooded. This villa, which is situated close to the Rio Jaguaribe, takes its name from a number of small gold workings (Lavras) which, from time to time, for many years past, have been wrought in its neighbourhood. Nothing, however, was done to any extent till about two years ago, when two English miners were sent for by a company in the city of Ceará, the capital of the province. They continued their labours till about two months before I passed through the place, having been recalled by their employers. I could not learn what amount of gold they had obtained, but the persons at whom I made inquiries remarked, with apparently much truth, that they did not believe it was sufficient to repay the expense, or the work would not have been abandoned. The gold is here found in small particles, in a dark-coloured diluvial soil at a considerable depth; but the place being shut up I had not an opportunity of examining it.

At about eighteen leagues below Crato, I lost sight of the gneiss rocks, and for the next four found them replaced by a grey-coloured primitive clay-slate. At the termination of this the secondary stratified series begins, the few rocks which I met with from thence to Crato consisting of a white coarse-grained sandstone.

The small Villa do Crato stands in the middle of a large undulating valley, which is bounded to the south, to the west, and to the north, by mountains which, in their highest parts, do not rise more than from 1200 to 1500 feet above the level of the town. The country around is very fertile, producing abundance of cane, from which an impure sugar, in the form of small square cakes, is made, mandiocca, Indian corn, rice, cotton, and tobacco, besides all the varieties of fruit which are to be met with on the coast. The great cause of this fertility is the numerous springs which exist along the foot of the mountains. The small streams which proceed from these are divaricated in a thousand directions, for the purpose of irrigating the plantations. The mountains are branches of the long range which separates the provinces of the coast from that of Pianhy to the west, which here receives the name of Serra de Araripe. Their tops are perfectly level, and extend so for many leagues to the westward and southward, forming what the Brazilians call Taboleiras. I have ascended this range in all directions, and have universally found it to consist of a generally white-coloured sandstone, but in many places it is of a reddish tinge. In the bed of one of the largest streams which proceed from it, where a section of the rocks to a considerable depth is formed, I found a stratum of limestone, about three feet thick, immediately below the sandstone, and below it another of an impure coal, two feet thick, resting on another stratum of limestone. Nothing seems to have disturbed the strata, as they all lie in a perfectly horizontal position, and the level nature of the Serra proves that this is general. In the limestone I could meet with no fossil remains. The temperature of two of the springs, which rise at the base of the Serra, I found, on examination, to be 75° Fahrenheit.

That part of the Serra which lies to the south of Crato is a branch which runs about ten leagues to the eastward. On the south side of it there is another small villa called Barra do Jardim, distant from Crato about fourteen leagues. I went to this place, partly for the purpose of botanizing, and partly to make a collection of fossil fishes, which, I was informed, were found in great plenty in its neighbourhood. The road skirts along the base of the Serra in a south-east direction for about five leagues, at the termination of which it is necessary to ascend it for the purpose of crossing to the other side. The ascent is far from being good, it being left entirely in the hands of nature. The only rock I observed was sandstone, similar to that which exists at Crato. The breadth of the Serra here is nearly eight leagues, and during the whole of this distance the road is as level as a bowling-green ; and, as no water is to be found on it, travellers are obliged to supply themselves with it before ascending. For small parties it is carried in calabashes, but when many pass together a horse is provided to carry two large leather bagfuls. These Taboleiras are generally thinly wooded, with small trees, the principal of which are a species of Caryocar called Piké, a small tree belonging to the natural order Apocyniaceæ, which produces a delicious fruit called Mangaba, a fine species of Brysonema, the Cashew (*Anacardium occidentale*), a purple-flowered Qualea, and several small leguminous trees belonging to the division Rectembriæ.

The Villa do Barra do Jardim stands in a small valley, upwards of a league in length, and in its broadest part about half a league in breadth. It is bounded to the north and east by the branch of the Serra which I crossed over, and to the west by another, but neither so broad nor so long. Having made inquiries for the place where the fossil fishes were to be found, I was directed to a rising ground which extends along the foot of the Serra. On my arrival at an open place of this gently sloping ridge to the north of the villa, I found the ground covered with great abundance of stones of various sizes, and I was informed that almost every one of them, on being broken, presented some part or other of a fish. These fragments I soon found to consist of compact fawn-coloured limestone. They are of all sizes, from pieces not larger than an egg to blocks of several feet in circumference, and are almost all rounded and smoothly polished, having apparently been for a long time under the influence of a current of water.* They in general

* This is hardly borne out by the appearance of the specimens, which are certainly not rolled pebbles, but nodules of impure limestone, nearly of the form of the imbedded fish, and apparently aggregated round it by chemical attraction from the sandstone while in a soft state. The fact stated by Mr Gardner, that he nowhere found limestone *in situ* in the neighbourhood of Barra do Jardim, and that the fossiliferous nodules are not mixed with,

split very readily, and almost all of them present parts of a fish in a more or less perfect state. But by far the greater number of them are so much broken that it is with considerable difficulty tolerably perfect specimens can be obtained. The spot which these stones occupy is not above an hundred yards square, and almost no other stone is mixed with them; but on every side of this deposit the ground is covered with little rounded sandstones, similar to the rock of which the Serra is composed. Besides this, I afterwards visited other deposits; one half a league to the south of it; one at a place called Macapé, five leagues to the east of Jardim; and another at Mundo Novo, three leagues to the west; all perfectly similar to the one I have described, being all situated on the declivity of the low hills which stand between the valley and the Serra, and all occupying places which are almost altogether free from other kinds of stone. From these places I have obtained a suite of specimens, embracing upwards of a dozen species of fossil fish.* They vary in size from those of a few inches in length, to others which must have been several feet; and all of them, so far as my limited knowledge of the subject allows me to judge, except two species, belong to the order *Cycloideæ* of M. Agassiz. The most abundant species is one of those which do not belong to this order. Of it I possess a nearly perfect specimen, about a foot and a half long, but, judging from other fragments of the same species, it must have attained a much larger size.† It has the head very much elongated, and the scales of the back and abdomen are angular, while those of the sides consist of but one row of long narrow ones arranged vertically. Of the other species I only possess the tail and a very small part of the body. It differs from the last, in appearing to be entirely covered with small angular scales. Both of them, I have no doubt, belong to the order *Ganoideæ* of M. Agassiz.

though surrounded by others of sandstone, seems to make it probable that the former occur in a bed or layer of detached concretions in the sandstone rock of the Serra.—J. E. Bowman.

* Agassiz makes them but seven species, and refers three of them to the Ctenoid group.—J. E. B.

† The fish here described is the Aspidorhynchus Comptoni. Agass. J. E. B

On breaking these stones, some of them exhibit abundance of a minute bivalve shell; and at Mundo Novo I met with a very perfect specimen of what I believe will prove to be a species of Turrilites, about an inch and a half long, and a single valve of a Venus, about half an inch in length, and in very excellent preservation. Both of them were found in the same fragment of limestone.* I was informed by a person in Jardim, that a few years ago he found a small serpent coiled up in a stone which had been split, but this, no doubt, was a species of Ammonites. In the several hundred stones, however, which I broke in search of fish, I met with nothing of this description. During my excursions in the neighbourhood of Barra do Jardim, I nowhere met with limestone *in situ*.

In conclusion, I may mention that the map of Brazil which I possess is that published by the Society for the Diffusion of Useful Knowledge, and that the towns of this province are there very inaccurately laid down. Thus the Villa das Lavras da Mangabeira, in place of being to the north of Ico, ought to be ten leagues to the south of it; and the Villa do Crato, which occupies about the place of Lavras, ought to be twenty-four leagues further to the south-west, which will bring it up to its proper station at the foot of the Serra de Araripe; while Barra (not *Bom*) Jardim should be fourteen leagues to the south of Crato. Such being the case with these, I have no doubt that most of the other inland towns are equally erroneously laid down. The southern boundary also of the province ought to run in a line with that which separates Parahiba from Pernambuco.

On the Fossil Fishes found by Mr Gardner in the Province of Ceará, in the North of Brazil. By Professor Agassiz. Communicated by the Author.

Among the fossil fishes collected in the north of Brazil by Mr Gardner, and which have been submitted to my examina-

* These shells I have not seen, but Mons. Agassiz considers the minute bivalves to consist of two kinds, both new and peculiar; and I shall be careful to have them examined by competent conchologists.—J. E. B.

tion by Mr Bowman, I have recognised seven distinct species, all new, but which appear to me to identify not the less the deposit in which they are found as belonging to the chalk-formation, and that because of the simultaneous occurrence of Ganoid, Ctenoid, and Cycloid fishes.

The two Ganoids which I have recognised, belong to two genera already characterized in my *Recherches sur les Poissons Fossiles.* They are

1. An *Aspidorhynchus*, which I have named *A. Comptoni;* allied to the *A. cinctus* of the Kentish chalk, but which differs from it by its scales being more rugose. I have dedicated it to the Marquis of Northampton, who communicated the first specimens to me.

2. A *Lepidotus*, which I shall name *L. temnurus*, characterized by the numerous articulations of the rays and of its fins. The species of lepidotus met with in the chalk of Kent has more elongated scales than those of the *L. temnurus.*

The Ctenoids are three in number, but they belong to one genus, of which I am not acquainted with a living species. Its characters are very remarkable; and it is a combination of features borrowed from several types, afterwards distinct. One dorsal scaly fin, without spinous rays, combined with ventral fins occupying the middle of the abdomen, and pectinated scales, form, certainly, a very peculiar assemblage of characters. I have named this genus *Phacolepis*, and have distinguished three species.

1. *Phacolepis Brama.* Broad: second posterior suborbitary narrower than the superior.

2. *Phacolepis buccalis.* Elongated; the two posterior suborbitaries of equal size, and elongated.

3. *Phacolepis latus.* Short, broad; the two posterior suborbitaries equally short, and of the same breadth.

The Cycloids belong to two extinct genera.

1. *Cladocyclus*, covered by very large scales, higher than they are long, marked with pores and lobed furrows, diverging towards the posterior edge. Small conical teeth. There is only one species, *Cl. Gardneri*, of very great size, which I have dedicated to the zealous traveller to whom the discovery is due. The scale from the Kentish chalk, which I have figured in my work

on Fossil Fishes, and which I supposed might be referred to my genus Hypsodon, is a scale of Cladocyclus. The teeth of the Brazilian Cladocyclus, which I have observed on a specimen furnished with scales, leave no doubt as to their generic difference.

2. *Calamopleurus.* Scales circular; tube of the lateral line straight and very short; trunk cylindrical, which induces me to term the species *Cal. cylindricus.*

I shall publish detailed figures and descriptions of these fishes, as complete as the materials I have examined admit of, in one of the supplementary parts of my work on Fossil Fishes.

On Hydrostatical Pressure as a Cause of Earthquakes. By the Rev. John Toplis, B. D., South Walsham, Norfolk. Communicated by the Author.*

Amongst the various causes brought forward to account for the phenomena of earthquakes, the writer of this article is not aware that they have ever been attributed to that of hydrostatical pressure. Perhaps this action may account in some instances for the circumstances which attend the convulsions of the earth's surface. Those which are most violent generally occur near the sea in the neighbourhood of the highest mountains, which would afford the greatest pressure, upon the supposition of their containing columns of water communicating with that under the surface where the eruption takes place, of sufficient altitude to produce such an effect. The vertical columns may be supplied by the water condensed from the aqueous vapour passing over their cold or snow-covered summits.

In order to remove any doubts with respect to the power arising from hydrostatical pressure being able to produce earthquakes, it may be observed, that the weight of a cubic foot of water is about 1000 ounces avoirdupois; consequently, that of a column of water of an equal base, 500 feet in height,

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* Read before the Wernerian Society, November 14. 1840.

would be 31,250 pounds. As it is the property of fluids to press equally in all directions, a square foot of the water, which communicates with a column of it of this height, would act against an upper stratum with a force equivalent to the same weight. From the above it may be easily inferred, that when the height of the column of water amounts to several thousand feet, and there is a pressure upwards to the extent of a number of square miles, it may be sufficiently great to produce, by sudden elevations and ruptures of the strata, the violent concussions and upheavings which occasion the most calamitous earthquakes.

That the interiors of many mountains do contain water, is well ascertained and proved by numerous facts given in various treatises upon geology. During volcanic eruptions from some of the Andes in South America, there are many instances, according to Humboldt, of very large quantities of water bursting from their sides. Those of Cotopaxi, Tunguragua, and Sangay, always, from openings at the elevations of 2500 or 2600 toises above the level of the sea, eject a prodigious quantity of fish along with torrents of water. At the time of the last eruption of Mount Idienne, a volcano in the east of the island of Java, so great a body of water was forced out, that the country extending from the mountain to the sea, a distance of twenty leagues, was inundated, and it gave rise to two large rivers.

The existence of extensive levels of water beneath the surface of the soil in those districts which are liable to earthquakes is rendered probable, from this circumstance being known to occur in many instances in different regions. It is evident in the case of Artesian wells. The springs of fresh water, some of which are very powerful, that rise from the bottom of the sea, and also, as Humboldt observes, those in the centres of very flat islands, for instance, the Caymans near Cuba, and some of the Bermudas, must be forced upwards by hydrostatical pressure from higher lands, although they are at a very considerable distance. Streams of fresh water are frequently found near the shore at the depth of several hundred feet below the level of the sea, many of which most probably pass under its bed. The numerous cases of rivers being wholly engulfed, and of fountains rising sufficiently large to form considerable rivers, are a proof that very great quantities of water sometimes exist below the surface of the ground.

Except in the cases of very slight earthquakes, or when they are connected with volcanic action, an eruption of water generally takes place through the fissures which are made at the time. It was the case with the earthquake at Jamaica, in the year 1692, as appears from the Philosophical Transactions*, where it is stated, that at several places very large quantities of water were forced up from the openings of the earth to a considerable height. At Varga, a few miles from Lisbon, during the time of the earthquake there in November 1755, many springs burst forth, and water was spouted to the altitude of nineteen feet. According to Humboldt, when an earthquake destroyed the city of Cumana on October 21. 1766, the earth opened at several places in the province and vomited sulphureous water. Also, during the violent earthquake which in one minute overthrew the city of Caraccas on March 26. 1813, so much water was thrown up through the cracks that a new stream was formed. When the earthquake occurred at Riobamba in 1797, the earth was fissured in innumerable places, and immense gulfs were likewise formed ; quantities of water rose, filling up valleys 1000 feet in breadth and 600 feet in depth. Wide rents were also opened during violent earthquakes on the north coasts of South America, in order to give exit to streams of water which then gushed out. It was often observed that, during these convulsions, water with sand, mud, &c. was thrown up from wells, sometimes to a height of thirty feet. Von Humboldt also relates, that this phenomenon is generally observed during the shocks at Cumana. From the statements of the earthquakes in Calabria, which took place in the years 1783–1786, large columns of water frequently spouted to a great height above the surface of the earth.

During the convulsions of the valley of the Mississippi in the year 1811, the earth rose in great waves, and when they reached a certain elevation, the surface burst, and volumes of water, sand, and coals, the materials of the soil, were discharged to the altitude of 100 feet or more. When an earthquake took place at Amarapoo in the Birmah territory on

April 23. 1839, water issued up to a considerable height through the openings of the earth. It is not requisite to enumerate more instances of the effusion of water which generally takes place at the time of earthquakes which are not connected with volcanic action, as they may be found in the works of Lyell and other geologists. The above are sufficient to shew that hydrostatical pressure, producing sudden fractures and eruptions of the under strata, if not the sole cause, is, in general, acting forcibly at the time of their shocks.

It is, perhaps, difficult to assign any other cause than that of hydrostatical pressure to the circumstances of earthquakes frequently occurring longitudinally in parallel directions to the chains of mountains in the neighbourhood. This was the case in the plains of the Mississippi in 1811, where the chains were all parallel and in the same direction from SW. to NE. as the Alleghany range. Professor Bischof, in his memoirs upon volcanos and earthquakes (published in this Journal), states this to have occurred in the Pyrenees, on December 28. 1779, July 10. 1784, July 8. 1791, May 22. 1814, &c.; also earthquakes in South America seem to follow the direction of the mountains. Thus, that at Caraccas in 1812 followed the direction of the littoral Cordilleras from E.N.E. to W.S.W. That of Cumana in 1797 presented an instance of the same fact. The predominant direction of the frequent earthquakes on the coast of Chili and Peru is also that of the large chain of the Andes, which is parallel to the coast. All the older reports likewise state that in these countries their direction is from S. to N., or *vice versa*; and Mrs Graham remarked that she felt, during the violent earthquake in Chili in 1822, as if the whole ground from north to south were suddenly raised and then sunk again.

If it be supposed that there are hollows under the mountains in consequence of their original elevations, and that they are filled with water, the earthquake might arise from the action of water under the plains, which was rendered sufficiently powerful from an additional quantity, owing to rain or the melting of snow, having added to the heights of the communicating vertical columns. That cavities do often exist under great elevations is evident from the frequent instances of their falling in which are given in various geological works, particularly that of Lyell, vol. ii., p. 249, &c., 5th ed. It is observed by Ulloa and Humboldt, that the subterraneous roarings which accompany the earthquakes and the amazingly great quantities of water that issue from the earth in the dryest places during their shocks, also the eruptions from volcanos, and numerous other phenomena, indicate that all the soil of the plains in the vicinity of the Andes is undermined. This may have been occasioned by the constant percolation of water forced gradually forwards, where there is the least opposition, by the weight of the high columns contained in the mountains. It is stated by Raynal, in his Histoire Philosophique, that numbers of rich mines in Peru are ruined by the water which descends in a slope from the Cordilleras to the South Sea.

The waters of seas, rivers, lakes, wells, and springs are generally affected by earthquakes, in some cases to a very considerable distance ; for instance, on the day of that at Lisbon, November 1. 1755, within a short interval, the waters of the sea in the West Indies, on the shores of Madeira, of lakes in Scotland, &c. &c., were unusually agitated. This may have been occasioned by the oscillations of the earth being communicated laterally, causing it to heave and subside under the waters. The strata were observed to be shaken in many of the Derbyshire mines from the effects of that earthquake. According to Bischof, in his Memoirs on Volcanos and Earthquakes, the commissioners who were employed to make observations on the earthquakes in the district of Pignerol near Turin, relate that the very day, April 2. 1808, when one of the most violent shocks was felt, the masting engine at Toulon was elevated more than an inch.

Boussingault asserts, Annal. de Chim. et de Phys. t. lviii., p. 83, that the most memorable earthquakes in the New World, which ravaged the towns of Latacunga, Riobamba, Honda, Caraccas, Laguayra, Merida, Barquisimeto, &c., do not coincide with any well-established volcanic eruption. The oscillation of the surface, owing to an eruption, is as it were local ; whilst an earthquake which is not subject (at least apparently) to any volcanic explosion, extends to incredible distances, in which case it has also been remarked that the shocks most commonly followed the direction of chains of mountains. The same observation has been made with respect to the West Indies by Jonnes in his Hist. Phys. des Antilles, p. 104. This circumstance shews that earthquakes, except in the vicinity of volcanos, are not owing to the expansive power of gases or vapours, which most probably do not extend to any considerable distances, as they would soon be condensed or absorbed by water, or else so far expanded as to lose their power of acting with sufficient force.

It is frequently observed during violent earthquakes, for instance those of the Caraccas in March 1812, and of the plains of the Mississippi in 1811, that the surface of the ground has been in a continual undulating movement and heaving up like a boiling liquid. This agitation may be explained with far greater probability by the supposition that the action upwards arises from an under body of water, than from the expansion of gases or vapours. The reiteration of the shock of an earthquake after certain intervals, may be owing to the subsidence of the elevated strata having closed the principal openings, which caused another oscillation after a sufficient influx of water had taken place.

Earthquakes very seldom, if ever, occur, except where there are volcanos, in very high latitudes that have the surface of the ground covered almost perpetually with ice or snow. In these countries the soil probably remains frozen to a very great depth, consequently there can be no action of water capable of upheaving it.

If hydrostatical pressure be the cause of earthquakes, they would not occur, as is the case, on very elevated surfaces or steppes, except in a very slight degree, from the motion communicated by distant convulsions of the earth.

Brief Observations on the State of the Arts in Italy, w'' a short Account of Cameo-cutting, Mosaic work, Pietra ... *and also of some of the Domestic Arts and Mechanical Contrivances of the Italians.* By Charles H. Wilson, Esq. Architect, Edinburgh, A.R.S.A., and M.S.A.* Communicated by the Society of Arts for Scotland.†

I feel that I ought to apologise to the Society for bringing before it a paper of this nature, which contains no description of any new art or discovery, but which may be described as being little more than a *catalogue* of arts and practices, most of which are of great antiquity. I hope that such a paper may be deemed admissible. As far as my individual opinion goes, I would say that it would be very desirable if several papers were read every session containing as distinct accounts as could be obtained of the state of the arts and sciences, with reviews of the progress made in them in different Continental countries every year. That such papers would be useful in various points of view appears to me sufficiently obvious; those who have neither leisure nor opportunity to inquire for themselves would by this means obtain a great deal of valuable and interesting information; our efforts to excel in the arts and sciences would be stimulated; and, above all, I think that, whilst our national vanity would be advantageously chastened, feelings of respect and esteem, founded on a knowledge and just appreciation of the merits of other nations, would beyond all other influences lead to international amity. Feelings like these have already been happily nourished by the amicable intercourse of literati of different nations: the course which I advocate would tend to the further diffusion of such sentiments amongst all classes.

I cannot, without presumption, imagine for a moment that the paper which I now bring before you can deserve to be con-

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* Read before the Society of Arts for Scotland, 23d November 1840.

† Having been requested by the Society of Arts to publish this paper on a popular topic, we give it a place, although it is not strictly connected with the subjects usually discussed in this Journal.—Editor.

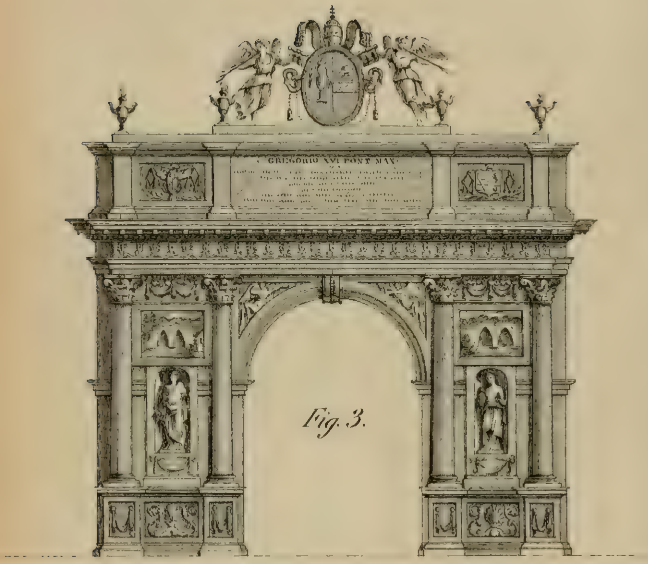
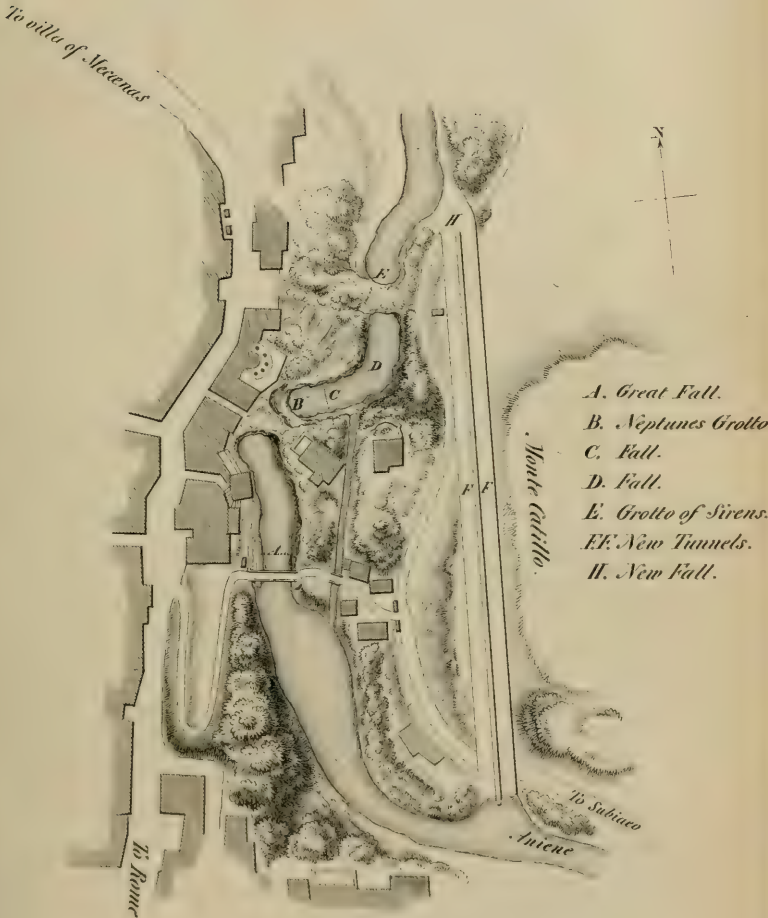


Fig. 3.

Triumphal Arch
of wood, canvas, and stucco erected at Tivoli on the occasion of a visit from the Sovereign Pontiff.

Illustrations of Mr. Wils

S. Leith's



Plan of Tivoli.

...'s paper on the Arts of Italy.

Fig. 5.

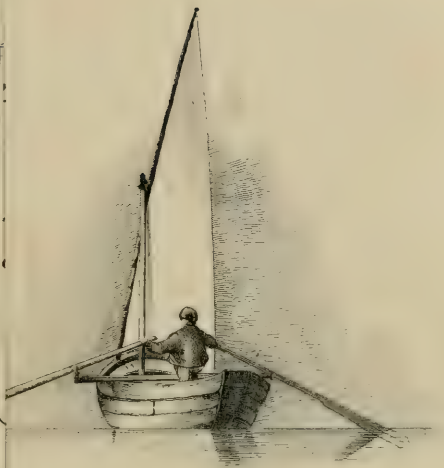


Fig. 4.

ishing boats used on the lake of Orbetello.

Italian Copper Bath Heater

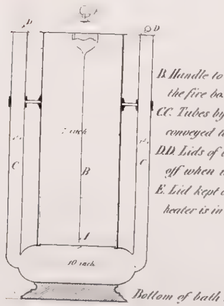
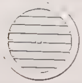
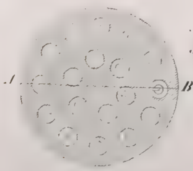


Fig. 5

B. Handle to lift out the fire box.
CC. Tubes by which air is conveyed to the fire.
DD. Lids of brafs taken off when in use.
E. Lid kept on when the heater is in the water.



Plan of cast iron grating for fire at A



Sketch of the perforated copper plate by means of which Maccaroni is made into tubes.

Section on AB.

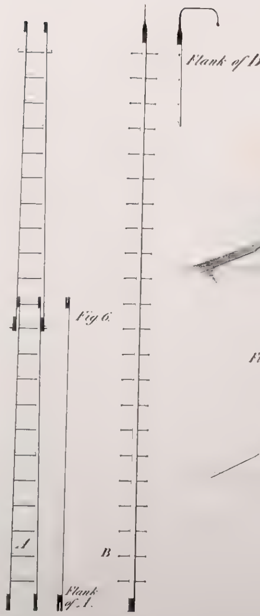


Fig 6.

Fire ladders used in Rome

Fig. 4

Fishing boats used on the lake of Orbetello.

sidered one of such a series. I went abroad at a very early age, and my time was entirely given up to the study of the art to which I had devoted myself, and which every thing around me tended to increase my love of. The collateral studies of the youthful artist are naturally those connected with his art, and are greatly more extensive in Italy, from many favourable circumstances, than in Scotland, and the brief allusion which I have made to them and to the time of life when I lived abroad, is meant as an apology for the meagreness of the details which I humbly bring under your notice.

Any comment on the political condition of Italy would be out of place in a paper to be read here, although a distinct apprehension of it would be necessary previously to any inquiry into the state of her arts and sciences, and also to enable us justly to appreciate the great merits of those Italian philosophers and literati, who, despite of adverse circumstances, so greatly distinguish themselves. To so slight a sketch of the *arts* of Italy as that I am about to offer, any lengthened observations are not so necessary. Whatever may be our opinion of Austrian principles of government, and of Austrian influence in Italy, all who have visited the Italian territories of that power, must, I think, acknowledge that Lombardy is greatly in advance of the independent states, and in no part of Europe, Scotland excepted, are there more numerous schools for the instruction of all classes of the people. As the traveller advances southward, with nominal independence political degradation increases, and the general character of the people is lowered. We can feel no other emotions than those of regret for the prostration of Italy; but if we examine into the customs of the Italians, we shall everywhere find expressive indications of ancient power and refinement, and pleasing proof that, where civilization and its attendant sciences and arts has once held extensive sway, advantages are secured of which it is almost impossible, or at any rate very difficult, to deprive a people.

I shall commence with a brief notice of the art of painting in Italy: this fine art has gradually declined, and there seems to be no indication at present of its recovery. It is trammelled by academic system. The Roman school is distinguished by a cold affectation of classic purity, and a want of energy and nature in all its productions; but, whilst we avoid the errors into which it has fallen, we should not allow these, and the difference of its practice from our own, to blind us to its good qualities; many Roman artists draw exceedingly well, and they evince this power in the large and fine cartoons which they are in the habit of executing before commencing a picture. But if the student in this country does not draw long enough, which I think is the case, the Italian student, in acquiring his mastery of the crayon, seems to forget that he is ever to use the brush; and the Italian artists rarely prove even tolerable colourists, whilst their prejudices as to the adoption of many necessary processes in painting, and which were unquestionably in use amongst their great predecessors, are invincible. This was illustrated in an amusing manner one day in the Florence gallery. An Italian artist was busy copying a Venetian picture, and my late friend Mr James Irvine, happening to look at his work, remarked to him that he never could hope to imitate the brilliancy of the original without glazing. "I know that," said the Italian, "but I won't glaze."

At Florence, painting is in much the same state as at Rome; of late some artists have endeavoured to add richness in colour to the correctness of their drawing, but they have only succeeded in arranging on their pictures in brilliant juxtaposition rainbow colours, without attaining that harmonious effect which marks the works of their great predecessors. At Naples, painting is at a low ebb; at Genoa, lower still; at Venice, it is little better; but at Milan it reckons amongst its professors clever men in some departments of the art.

Fresco painting is still pursued in Italy, but with most success by the Germans. I wish to avail myself of this occasion to do homage to the extraordinary merits of the masters of this distinguished school; in looking on their works, we cannot but regret that greater encouragement is not given to the highest department of painting in this country; in those which are encouraged, our artists excel; and we may, I think, therefore, justly conclude that ability would soon be found to execute works of the noblest description.

Engraving may appropriately be considered after painting.

You are all, doubtless, well acquainted with the great names which have lately marked the progress of this art in Italy; most of these distinguished artists are now dead. Several of Raphael Morghen's pupils are much esteemed, the best of whom are established at Milan; many very fine and important works have been lately finished or are now in progress. Messrs Ludwig Gruner and Rusweigh, both Italianized Germans, promise to revive the style of Marc Antonio with success.

The Italian engravers are most successful in their works from historical pictures; but a practice which they follow is, in my opinion, calculated to prevent their imitating with fidelity the style and feeling of the artist whose production they copy. They engrave from highly finished chalk drawings copied from pictures by artists who devote themselves to this branch: however faithfully these may apparently copy, it is certain that their drawings will, to a certain extent, exhibit their peculiarities of mind and feeling, and, as the engraving must likewise so far be marked by the style of its author, the process is not favourable to the production of engravings of a faithful character.

It is but fair to mention that this practice is forced upon the Italian engraver, as he can neither transport gallery pictures nor frescoes to his study.

The landscape engravers of Italy are not successful. Frigid imitaters of Woollet in general, their works are far inferior to those of that admirable master.

Sculpture is certainly the art which stands highest in Italy. Canova rescued it from the infamy into which it had sunk, and his genius at once raised it to excellence. If I say that that immortal artist has worthy successors amongst his countrymen, I express, as strongly as possible, a favourable opinion of the state of the art. If we are to term that the Roman school of sculpture which reckons amongst its professors all the great sculptors of various nations who make the Eternal City their fixed place of residence, then we must, I think, hold that it is the first school existing. England is worthily represented in that united school. I shall not venture upon any comparison between it and our present British school; but it is an important fact, and to its honour, that, before Canova resuscitated sculpture in Italy, England could boast a succession of very eminent sculptors. I may mention the estimation in which our great Flaxman is held in Italy. "Flaxman," said a distinguished artist to me on one occasion, "was the greatest sculptor the world has known since the time of the Greeks;" and this opinion is very general in Italy. I touched shortly on the state of painting in the different Italian capitals. I shall pursue the same course with sculpture, but more briefly still, merely remarking that, with one or two exceptions, there are no Italian sculptors of eminence out of Rome.

In connection with the arts of painting and sculpture, we may now consider mosaic work and cameo-cutting as practised in Rome. The art of mosaic work has been known in Rome since the days of the republic. The severe rulers of that period forbade the introduction of foreign marbles, and the republican mosaics are all in black and white. Under the empire the art was greatly improved, and not merely by the introduction of marbles of various colours, but by the invention of artificial stones, termed by the Italians *smalti*, which can be made of every variety of tint.

This art was never entirely lost. On the introduction of pictures into Christian temples, they were first made of mosaic; remaining specimens of these are rude, but profoundly interesting in a historical point of view. When art was restored in Italy, mosaic also was improved, but it attained its greatest perfection in the last and present century. Roman mosaic, as now practised, may be described as being the production of pictures by connecting together numerous minute pieces of coloured marble or artificial stones; these are attached to a ground of copper by means of a strong cement of gum mastic, and other materials, and are afterwards ground and polished as a stone would be to a perfectly level surface; by this art not only are ornaments made on a small scale, but pictures of the largest size are copied. In former times the largest cupolas of churches, and not unfrequently the entire walls, were encrusted with mosaic. The most remarkable modern works are the copies which have been executed of some of the most important works of the great masters for the altars in St Peter's. These are in every respect perfect imitations of the originals;

and when the originals, in spite of every care, must change and perish, these mosaics will still convey to distant ages a perfect idea of the triumphs of art achieved in the fifteenth century. The government manufactory in Rome occupies the apartments in the Vatican which were used as offices of the Inquisition. No copies are now made, but cases of *smalti* are shewn, containing, it is said, 18,000 different tints. Twenty years were employed in making one of the copies I have mentioned. The pieces of mosaic vary in size from an eighth to a sixteenth of an inch, and eleven men were employed for that time on each picture.

A great improvement was introduced into the art in 1775 by the Signor Raffaelli, who thought of preparing the *smalti* in what may be termed fine threads. The pastes or *smalti* are manufactured at Venice in the shape of crayons, or like sticks of sealing-wax, and are afterwards drawn out by the workman at a blow-pipe into the thickness he requires, often almost to a hair, and now seldom thicker than the finest grass stalk. For tables and large articles, of course, the pieces are thicker; but the beauty of the workmanship, the soft gradation of the tints, and the cost, depend upon the minuteness of the pieces, and the skill displayed by the artist. A ruin, a group of flowers or figures, will employ a good artist about two months when only two inches square, and a specimen of such a description costs from L.5 to L.20, according to the execution; a landscape, six inches by four, would require eighteen months, and would cost from forty to fifty pounds. This will strike you as no adequate remuneration for the time bestowed. The finest ornaments for a lady, consisting of necklace, ear-rings, and brooch, cost L.40. For a picture of Paestum, eight feet long, and twenty inches broad, on which four men were occupied for three years, L.1000 Sterling was asked.

I shall now notice the mosaic work of Florence, before touching on cameo-cutting. It differs entirely from Roman mosaic, being composed of stones inserted in comparatively large masses; it is called work in *pietra dura*. The stones used are all more or less of a rare and precious nature. In old specimens the most beautiful works are those in which the designs are of an arabesque character. The most remarkable specimen of this description of pietra dura is an octagonal table in the *Gabinetto di Baroccio*, in the Florence Gallery. It is valued at L.20,000 Sterling, and was commenced in 1623 by Jacopo Datelli, from designs by Ligozzi. Twenty-two artists worked upon it without interruption till it was terminated in the year 1649. Attempts at landscapes, and the imitation of natural objects, were usually failures in former times,—mere works of labour, which did not attain their object; but of late works have been produced in this art, in which are represented groups of flowers and fruit, vases, musical instruments, and other compatible objects, with a truth and beauty which excite the utmost admiration and surprise. These pictures in stone are, however, enormously expensive, and can only be seen in the palaces of the great. Two tables in the Palazzo Pitti are valued át L.7000, and this price is by no means excessive. These are of modern design, on a ground of porphyry, and ten men were employed for four years on one of them, and a spot is pointed out, not more than three inches square, on which a man had worked for ten months. But Florentine mosaic, like that of Rome, is not merely used for cabinet tables or other ornamental articles; the walls of the spacious chapel which is used as the burial-place of the reigning family at Florence are lined with pietra dura, realizing the gem-encrusted halls of the Arabian tales. Roman mosaic, as we have seen, is of great value as an ally to art; but Florentine mosaic can have no such pretensions, and time and money might be better bestowed. The effect is far from pleasing in the chapel I have alluded to, and I think that the art might be advantageously confined to the production of small ornaments, for which it is eminently adapted.

An imitation of the pietra dura is now made to a great extent in Derbyshire, where the Duke of Devonshire's black marble, said to be quite equal to the famous Nero Antico, is inlaid with malachite, Derbyshire spars, and other stones; but the inlaying is only by veneers, and not done in the solid as at Florence. This, with the softness of the materials, makes the Derbyshire work much cheaper, and yet for a table, twenty to twenty-four inches in diameter, thirty guineas is asked. Were a little more taste in design and skill in execution shewn, the

Derbyshire work might deserve to be more valued, as the materials, especially the black marble, are beautiful.

I shall now return to cameo-cutting. This art is also of great antiquity, and is pursued with most success in Rome, where there are several very eminent artists now living. Cameos are of two descriptions, those cut in stone, or *pietra dura*, and those cut in shell. Of the first, the value depends on the stone, as well as in the excellence of the work. The stones most prized now are the oriental onyx and the sardonyx, the former black and white in parallel layers, the latter cornelian, brown and white; and when stones of four or five layers of distinct shades or colours can be procured, the value is proportionably raised, provided always that the layers be so thin as to be manageable in cutting the cameo so as to make the various parts harmonize. For example, in a head of Minerva, if well wrought out of a stone of four shades, the ground should be dark grey, the face light, the bust and helmet black, and the crest over the helmet brownish or grey. Next to such varieties of shades and layers, those stones are valuable in which two layers occur of black and white of regular breadth. Except on such oriental stones no good artist will now bestow his time; but, till the beginning of this century, less attention was bestowed on materials, so that beautiful middle-age and modern cameos may be found on German agates, whose colours are generally only two shades of grey, or a cream and a milk-white, and these not unfrequently cloudy. The best artist in Rome in *pietra dura* is the Signor Girometti, who has executed eight cameos of various sizes, from 1½ to 3½ inches in diameter, on picked stones of several layers, the subjects being from the antique. These form a set of specimens, for which he asks L.3000 Sterling. A single cameo of good brooch size, and of two colours, costs L.22. Portraits in stone by those excellent artists Diez and Saulini may be had for L.10. These cameos are all wrought by a lathe with pointed instruments of steel, and by means of diamond dust.

Shell cameos are cut from large shells found on the African and Brazilian coasts, and generally shew only two layers, the ground being either a pale coffee-colour or a deep reddish-orange; the latter is most prized. The subject is cut with little steel chisels out of the white portion of the shell. A fine shell is worth a guinea in Rome. Copies from the antique, original designs, and portraits, are executed in the most exquisite style of finish, and perfect in contour and taste, and it may be said that the Roman artists have attained perfection in this beautiful art. Good shell cameos may be had at from L.1 to L.5 for heads, L.3 to L.4 for the finest large brooches, a comb costs L.10, and a complete set of necklace, ear-rings, and brooch, cost L.21. A portrait can be executed for L.4 or L.5, according to workmanship.

Having now touched upon those minor arts which have an intimate connection with painting and sculpture, I shall make a few observations on architecture, and the constructive and decorative arts which are connected with that science; but this I must do very briefly indeed, as otherwise I should occupy too much of the time of the Society.

The architects of Italy have but little scope for a display of ability, as the population is not on the increase, but, on the contrary, except in parts of the Austrian States, has shrunk away from the number required to occupy the palaces, villas, and houses which already exist both in town and country; and this is painfully proved by the number of empty and dilapidated edifices. The various buildings which belong to Government, the churches, colleges, and hospitals, have generally been built on a scale of magnificence which has never been excelled, in some instances never equalled, in other countries, but all betoken more or less the same melancholy decline. By this observation I do not mean to convey the idea that the buildings themselves are ruined or neglected; I allude to their emptiness, and to the absence of that state which once filled them with its splendour. To her honour, the hospitals of Italy have long been known for their number, extent, and order, and these are still models in many respects. Although not *many* works, yet some of great magnitude are going on in Italy, and in these taste in design, magnificence in material, and solidity of construction, are displayed. The restoration of the Basilica of St Paul's at Rome is an immense undertaking; to effect it, contributions have been obtained from all countries, whether in money or materials. It is said that

George the Fourth subscribed; and I may mention that the façade of another church in the Eternal City has been built at that sovereign's expense, in a way which he must little have anticipated. When the celebrated Gonsalvi visited England, his Majesty presented him with a magnificent snuff-box, which the cardinal in his will directed to be sold, and the proceeds applied to put a front on a church which had for a long time been unfinished in that respect.

The passion which all pontiffs have displayed for building still animates the less potent holders of St Peter's chair of our day; and although inhabiting a palace which contains twenty-two court-yards, twelve halls of entrance, twenty-two grand stair-cases, and thirteen hundred of various descriptions; two large chapels, and eleven thousand rooms and galleries, in which miles may be walked without returning on the steps, yet each succeeding pope adds or alters, or marks repairs with his sculptured coat of arms.

Although there is not much employment for architects in Italy, there can be no question of the skill displayed in erecting their designs. The masonry is excellent, and the ancient Roman brick-work is rivalled by that of the present generation; houses are built of brick, in which all the exterior decorations are moulded in that material as perfectly as if executed in stone. The skill with which the Italian workmen build in brick may be exemplified by a notice of the Florentine practice of arching over rooms without centering of any description. Two thin moulds of board, the shape of the intended arch, alone are used; these are placed at each end of the apartment which it is intended to cover in, and pieces of string are stretched from the one to the other, guiding the workman as he advances in the formation of his arch, which he builds, uniting the bricks by their thin edges (greatly thinner than in those we use), and trusting entirely to the tenacity and quick setting of the cement.

Plastering is carried to a perfection in Italy of which we have, I believe, no idea in this country; rooms are so exquisitely finished, that no additional work in the shape of house-painting is required, the polish of the plaster and its evenness of tint rivalling fine porcelain. At times the surface of the plaster is fluted, or various designs are executed in *intaglio* upon it, in the most beautiful manner. Scagliola, a very fine preparation from gypsum, is the material chiefly used.

As an instance of the cheap rate at which this work is done, I may mention the new ball-room in the Palazzo Pitti, grand-ducal residence at Florence, which, including mouldings, figures, bas-reliefs, and ornaments, was executed at a cost of two crowns for every four feet square.

Work in scagliola naturally follows in my notice of the arts of architectural decoration ; but this I need not describe, as the art is now practised in England with great success, and an artist has lately settled in Edinburgh, whom I earnestly hope may meet with encouragement. A most beautiful art may be mentioned here, in connection with the last, I mean that of making what are termed Venetian pavements, which might advantageously be introduced into this country. The floors of rooms are finished with this pavement, as it is somewhat incongruously termed, and I shall briefly describe the mode of operation in making these, but must first observe that they are usually formed over vaults. In the first place, a foundation is laid of lime mixed with pozzolana and small pieces of broken stone; this is in fact a sort of concrete, which must be well beaten and levelled. When this is perfectly dry, a fine paste, as it is termed by the Italians, must be made of lime, pozzolana, and sand; a yellow sand is used which tinges the mixture; this is carefully spread to a depth of one or two inches, according to circumstances. Over this is laid a layer of irregularly broken minute pieces of marble of different colours, and if it is wished, these can be arranged in patterns. After the paste is completely covered with pieces of marble, men proceed to beat the floor with large and heavy tools made for the purpose; when the whole has been beaten into a compact mass, the paste appearing above the pieces of marble, it is left to harden. It is then rubbed smooth with fine grained stones, and is finally brought to a high polish with emery powder, marble-dust, and, lastly, boiled oil rubbed on with flannel.

This makes a durable and very beautiful floor, which in this country would be well adapted for halls, conservatories, and other buildings. In connection with the arts which the architect summons to his aid, I shall now notice that of ornamental sculpture; and here again we must acknowledge the superior skill of the Italians. The chief encouragement to artists of this description, is that given by foreigners, especially by English travellers in Italy. Copies of ancient sculptures, vases, chimney-pieces, and other ornamental articles, are executed in the most perfect manner, and at a very cheap rate. Such is the skill of the Italian workmen, that a native of Carrara actually cut a bird-cage in marble, which he presented to his sovereign the Duke of Modena, who, by the return he made, rather shewed his sense of the folly of the sculptor, than of his patient perseverance in the production of so useless a specimen of his skill.

But whilst the sculptor displays his skill in these comparatively trifling departments, he is equally successful in the execution of architectural details on the most gigantic scale, whether in solid marble or in veneer. By this latter art he produces magnificent columns plain and fluted, the core of which is of coarse stone, but the joining of the marble-coating is so perfect, that the finished pillar seems a mass of solid marble. The marble is attached in a rough state to the core by means of a cement composed of resin and marble dust, which is so tenacious, that it admits of the hammering, chiselling, and polishing necessary in finishing the work. By means of this system of veneering, the interior walls of churches and other buildings are encrusted with rich and varied marbles, and tables and other articles of furniture are manufactured at a very cheap rate. The art which I have just described is, in fact, that of pietra dura on a gigantic scale.

With the sculpture of the Italians in alabaster, you must be all acquainted. This art is chiefly practised at Pisa, Florence, and Leghorn. The material, besides being used in sculpture, is ingeniously applied in Rome to the manufacture of false pearls. The pieces of alabaster, after being turned and filed into the proper shape, are enveloped in a brilliant paste, made with the scales of a very small fish found near the shores of the Mediterranean.

To return to the subsidiary arts of architecture, I may remark, that the carpentry of the Italians, as observable in ordinary houses, displays little skill and indifferent workmanship; but in the roofs and floors of important buildings, they satisfactorily prove their knowledge of scientific principles, and several of their designs are well known to British architects.

With regard to the working of iron, in comparison with our system the Italian is primitive indeed; yet at times they can and do produce very good specimens of workmanship, but at a heavy cost; consequently they are generally content with very ordinary productions. A manufactory of wire, and of driving and screw nails, by means of machinery, *now occupies the villa of Mecœnas at Tivoli;* the articles produced are very well made. Copper is extensively used in Italy, and there are productive mines in the *Maremma Toscana.* The workmanship of articles made of this metal is respectable; various utensils are made of brass in a very neat and satisfactory manner, but in the interior finishing of houses, if much nicety is required, articles of foreign manufacture are used.

House-painters may be mentioned in the last place, and these display much taste and skill; and there is a class of them who greatly excel those in this country, having more the feeling and taste of artists. Surrounded by the finest models in this art, the Italian decorator enjoys every advantage in its study, and he inherits besides from the best periods of art, or rather from all antiquity, taste and a good system of workmanship. He is not a mere machine like the workman in this country, who has little use for an intellect beyond enabling him to use his moulds, stamps, and the various mechanical contrivances which confine all our decorative arts within such commonplace limits.

In all old architectural drawings and engravings, we find a vigorous artist-like style, which is reflected in the works done from them. In the architectural engravings of the present day, every thing is sacrificed to a display of dexterity in the use of the burin; the spirit of the original ornaments is never represented. How strongly this is illustrated, for example, in our engravings from Etruscan vases! Works executed from such engravings, or from drawings like them, are naturally stiff and lifeless like the models. People who possess a feeling of taste, dissatisfied with such productions, seek to replace them with older specimens, and amongst other things very inconvenient carved chairs and tables, in the workmanship of which they find a pleasure in tracing the influence of mind. But the cleverness in the workmanship of these specimens has greatly misled the taste of the day; and the abominations of Elizabethan architecture, lately dignified with the name of the *Renaissance* style, of which however it is a mere caricature, the extravagances of the Louis XIV. and XV. eras, or the debonnaire barbarisms of Watteau, have contributed to the banishment of a healthy taste in style. To restore a feeling for better art, the purer styles of classic or Gothic art must again be executed in the spirit of better times, and to grace of form must be added feeling in execution.

I shall now turn to the engineering works of Italy, a subject worthy of much attention, but on which I regret to say I am able to say very little indeed. The greatest works I saw going on were those at Tivoli, and from the Ombrone to the Lake of Castiglione in the Tuscan Maremma. I shall merely offer a very brief description of these works, necessarily very imperfect, as I write entirely from memory. The Tiber or Aniene, on reaching Tivoli, was dammed up by the architect Bernini; precipitating itself over the lofty barrier he raised, it disappeared under the rocks on which the town is built, and was seen again in the celebrated grotto of Neptune; rushing out of this remarkable cavern it fell into another abyss, and again vanished into the grotto of the Sirens, from whence it issued in the deep valley under Tivoli, several hundred feet below its original level. The pencils of the painters of every nation have been employed for centuries with this, I may say, terrible scenery, this *orrido bello*, of the falls of Tivoli. They may now depict the rocks, but the waters are gone for ever. Some years ago, Bernini's dam was carried away in a flood; it was rebuilt by the Pope's engineers, but if I remember aright the river got the better of them and threw down their work; at last they dammed up old Tiber, and made the very ugliest waterfall that ever unfortunate artist contemplated. It was discovered that the river, in passing through Neptune's grotto, had worn away the rocks in such a manner that the town and its temple depended on a rugged pillar, the duration of which could not be calculated upon. To prevent the town paying a visit to the Sirens beneath, it was resolved to turn the river, and it will be acknowledged that this was a bold undertaking; walled in by mountains, it sought a passage under them; and to a certain extent imitating the operations of nature, the engineers have carried the river through two parallel tunnels, and tumbled it into the valley beyond the Sirens' grotto over a bank twice or perhaps three times as high as the Calton hill. I have made a plan from memory (fig. 1.) of this operation, as the best mode of explaining it. The engineers have saved Tivoli, but its romantic beauty, as far as the river is concerned, is gone for ever.

The other engineering work which I mentioned, namely the canal from the Ombrone to the Lake of Castiglione, has excited much interest. The Lake of Castiglione, anciently the Lacus Prilis, falling very low in summer, left much marshy ground uncovered, in which were numerous stagnant pools, and quantities of putrid herbage, making the air poisonous in hot weather, and breeding myriads of noxious insects. To remedy these evils, Leopold the First ordered his architect Ximenes to make a canal from the river Ombrone to the lake; by this means it was intended to keep the latter constantly at the same level. This work was finally executed by the present Grand Duke in the year 1830, and by means of a canal seven miles long and twenty-five feet broad, a sufficiency of water is supplied to keep the lake at a proper level; so sufficient indeed was the supply that the whole surrounding country was overflowed the first year, but this has been remedied. The air it is said has been improved; but, when I visited Castiglione in 1832, I found that all who could left it in the summer months, and all who remained had the fever. Some notice may be expected from one of the engineering works in the Pontine marshes; but like other British travellers, I have only galloped through them, and have merely to state that the attempts to drain them cost a million of money.

The roads in the north of Italy are excellent, and indeed generally throughout the Peninsula. Although a small portion comparatively of the country is intersected by roads, I have travelled many miles over turf, or by small tracks on the coast or on the mountains. Towns are almost universally built on eminences; consequently the roads are hilly, but I think less so than would be supposed from the nature of the country, and both in direction and in smoothness, they greatly excel those of France.

The system of road-making followed is nearly the same as that adopted by the late Mr Telford, that is to say, a pavement of stones is first formed upon which the metal is laid; but I do not think that the principles advocated by our great engineer are followed out in the formation of the pavement. Excellent roads, however, are the result of the system, even although gravel is used instead of broken metal.*

Various principles of paving are now exciting much attention in London; it is to be regretted that something like a sensible principle is not followed in Edinburgh. In Italy various modes are adopted, in Genoa and at Naples large flat parallelograms of lava are used, at Florence large irregular polygons carefully jointed, and at Rome a pavement resembling our own, except that the stones are of regular forms, of one size, and grouted in with lime and pozzolana.

I shall now touch very briefly on a few arts of Italy which remain to be described, and shall then take the liberty of bringing before you one or two contrivances which struck me as ingenious and of which I have prepared drawings.

The goldsmiths of Italy produce ornaments which are both remarkable for taste and workmanship, especially those of Genoa and Venice. I am enabled to shew you some trifling specimens which our workmen cannot equal.

After the goldsmiths I may mention the makers of bronze

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* I have not seen the railroad which has been lately made from Naples to Castellamare, but am well acquainted with the line; a novel question in engineering must arise in considering how it is to be protected from the lava of Vesuvius. This I believe will not be very difficult, but it has a more insidious enemy in the earthquake, and a more overwhelming one in the showers of scoriæ and ashes which accompany an eruption.

Railways may be useful in Italy to promote her commercial prosperity, but I pity the man who could think of travelling in such a manner through any part of that country.

ornaments and figures; this is an art in which the Italians shew much taste and dexterity, so much dexterity indeed that they sell numbers of antique bronzes of modern fabric yearly to *soi-disant* antiquaries, who, however, neither possess that extensive learning nor profound experience and correct taste necessary to constitute such a character. It is much the practice in Rome to take moulds from real lizards and to cast them in bronze; these make very pretty ornaments for the table. I regret that I am unable to give you an idea of the value set upon these works.

The manufacture of glass is pursued with great success in Venice: the numerous glass ornaments for ladies which come thence are well known, and the endless varieties of form and combinations of colour given to glass beads for rosaries and embroidery, or vessels for domestic use, are very ingenious and beautiful. The ruby glass of the 1500 and 1600 can now be imitated so as to make imposition a famous trade, the false being only distinguishable by weight. Glasses are also made in which white threadlike lines of arsenic are incorporated. The process by which they make sheet-glass differs from ours. Instead of being formed into immense circular sheets, the Venetian workman blows cylinders of considerable length and diameter; he then cuts off the two ends of his cylinder, dexterously slits it down one side, and spreads it flat on a table in an oven. By this process sheets of a sufficient size are made, and there is no loss as in those fabricated in this country.

I think that I have lately observed that the process which I have thus briefly described is practised at some manufactory in England.

The velvets of Genoa, and the exquisitely turned ware of the same place, the straw hats of Tuscany, the silks of Florence, the embroideries of Rome, the musical instruments and musical strings, and, although last not least, the maccaroni, of Naples, are all samples of skill creditable to the Italians. As I never met any one who could guess by what process maccaroni is made into pipes, I have made a drawing of the copper plate (fig. 2.) through which it is squeezed, in case any member of the society should wish to understand it.

I shall now request your attention to this lithograph of a triumphal arch (fig. 3). This is a specimen of an art in which the Italians display both taste and great ingenuity, and which seems to me deserving of notice, for although it may be deemed useless by some, yet it contributes largely to their happiness. I allude to their preparations for festivals and pageants. Without entering into any description of these, I shall content myself with exhibiting a print of a triumphal arch erected at Tivoli on the occasion of a visit from his Holiness the Pope. Erections of this description are put up in a day or two, being formed of a frame-work of wood, covered with coarse canvass painted in imitation of stone. The bas-reliefs are of stucco, and the statues are formed of straw, arranged round wooden supports; casts of heads, hands, and feet are easily procured and attached. This *anima* (soul), as it is termed, is skilfully enveloped in drapery of cotton cloth, which is tastefully arranged by an artist, and is then lightly brushed over with white-wash, which stiffens it. That a knowledge of the art displayed in erecting this arch may be useful, may I think be proved, by an allusion to the gallows-like erection under which his Majesty George IV. passed when he entered Edinburgh.

I now beg your attention for a few minutes, whilst I describe the next drawing (fig. 4). In the summer of 1833 I made a journey from Leghorn to Rome along the coast, a *terra incognita* to most travellers, my object being to trace the Via Aurelia. At Orbetello, the last town in the Tuscan States, besides making some interesting antiquarian discoveries, I observed the boats which I am about to describe. Orbetello stands upon a peninsula, projecting into a shallow lagoon of some extent; the boats which are used upon it, are flat-bottomed, rise considerably at the bow and stern, being lowest at midships, across which part of the vessel a beam is fastened, about four inches thick each way, and which projects about two feet six inches over each side. On each of the ends of this beam an oblong piece of plank is nailed, the longest sides being horizontal, and a stout pin rises from each of these. The oars are of considerable length in proportion to the boat, and of great breadth in the blade, which is of the form shewn in the drawing. These oars rest upon the pieces of board at the ends of the cross-beam, being attached to the pin by means of a piece of cord, in this last respect resembling a mode adopted in boats on our own coasts. The blade of the oar slightly overbalances the portion within the fulcrum on which it rests, the handles nearly touch each other, meeting a-midship. By this contrivance, one man can manage a pair of very powerful oars, and can drive a boat, which is apparently but ill adapted from its form for speed, with surprising rapidity through the water; he can arrest its progress, or turn it with equal rapidity and certainty, and with very little exertion.

My knowledge of boats and ships is indeed very trifling, but I could not help seeing how easily the fisher of Orbetello manœuvred his rude boat; and therefore I have been induced to bring forward this notice of a vessel and mode of rowing which I am not aware has been described. Besides, it suggests ideas as to the probable mode in which the ancients managed their triremes, well worthy the attention of the antiquary, especially if he will combine the hint thus obtained with the modes of rowing followed in the Bay of Naples on board the Sorrentine boats, which, I have been led to imagine from an examination of pictures in Pompeii, are much the same in every respect as the galleys which in old times navigated the same sea.

My next drawing represents (fig. 5), by means of a section, an apparatus used in Italy for warming baths. I need not describe it, but shall merely observe generally, that it is made of copper; the live charcoal is put upon the grating A, which is put into the stove by means of the handle B, the fire is kept alive by air supplied through the tubes C C, and when immersed in the water of a slipper-bath, this light and portable apparatus will heat it in a quarter of an hour. I think it might be useful in this country.

In the (fig. 6) same drawing I have introduced the ladders used by architects in Rome in measuring the antiquities, and by the fire-brigade. Probably these are known here,* yet we

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* I am informed that the scaling ladders used in our army are of this description (fig. 6 A). The ladder (fig. 6 B) is used by the Roman fire-brigade, being very light, and is hooked over a window sill. I have often seen men ascend into houses by means of this ladder.

see immense and heavy ladders carried about which these Roman ones would better supply the place of; they can be used in one, two, or half a dozen lengths, as may be required, are very easily carried, put together in a minute or two, and would be very useful in cases of fire. With regard to fire-escapes, a Roman apparatus has been brought to my recollection by several London fires lately described. On these lamentable occasions six or seven lives were saved by the simple expedient of individuals beneath holding a blanket, into which the sufferers jumped and thus reached terra firma in safety, even from top stories; on one occasion lately, I observed a man, his wife and child, saved by the use of a watchman's great coat to break their fall. At Rome the fire-brigade is furnished with a large sheet of sail-cloth bound with rope, in which loops are left at convenient distances. At every review of the men, by way of setting an example to the Roman citizens, and shewing them what to do if burned out, every fireman jumps from the second story of some chosen house into this sheet; shouts of applause greet the skilful jumpers, and roars of laughter those who precipitate themselves down in a less graceful manner. I think that we might take the hint, especially as late fires in London have proved the use of this cheap apparatus.

I now close this paper with many apologies for having detained you so long. The engineering works I have briefly described may seem trifling as compared with those extrordinary and gigantic operations you are accustomed to in this country; but I would ask you to consider the relative extent, power, and resources of the states, and you must then allow that they are very creditable to the Italian Governments.

The Italians, we have seen, are still remarkable for their taste and skill in many beautiful arts, and for nearly 3000 years they have been thus distinguished. Various arts were successfully practised by the Etruscans, and when they were subdued by the ruder Romans, they did not lose their skill, but enlightened their masters.

The conquest of Greece filled Italy with artists and works of art; and when northern hordes overwhelmed the empire, these ruthless barbarians were gradually softened by the fine arts of the people they had conquered. A new power arose in Italy, and by its influence again she became pre-eminent in Europe, and we know to what illustrious perfection the fine arts again attained.

In our sale-rooms we see sold every winter many cracked and dingy daubs, and with these before him, the auctioneer rings the changes on some half-dozen names, as if the Italian school could boast no more; but a host of artists attest the fertility of Italy in the production of men of talent; and in Lanzi's dictionary, 1000 names will be found before the reader reaches the middle of the letter D in the index.

I have imperfectly described to you some of the arts which the Italian has inherited. I shall close this paper by observing that, whatever public work is undertaken in Italy—whereever improvement is contemplated, even although it should not be extensive, it is justly thought that the assistance and advice of the artist, whose taste and judgment have been cultivated, ought to be secured, and there is no practice in *its full extent* more worthy of our imitation.*

On the Frozen Soil of North America. By John Richardson, M.D., F.R.S., &c. Inspector of Naval Hospitals. Communicated by the Author.†

At the meeting of the British Association held at Edinburgh in the year 1834, M. Arago addressed the Committee of Recommendations *on the necessity of more extensive and systematic observations on the temperature of the earth.* In the course of the discussion which ensued, I mentioned the advantages of *correct measurements of the depth of permanently frozen soil* in the northern parts of America, and at the suggestion of the members then present, undertook to apply personally to the Governor and Committee of the Hudson's Bay Company, requesting that they would give directions for mak-

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* Mr Wilson exhibited numerous specimens of mosaic, pietra dura, cameos of different ages in pietra dura, and specimens of shell cameos; also of Genoese and Venetian jewellery, Venetian glass, and ruby glass, together with numerous prints and drawings.

† Read before the Wernerian Society, Dec. 12. 1840.

ing the necessary observations as fully as the means available at their several posts would allow. I wrote, accordingly, to the Governor, now Sir John H. Pelly, Bart., stating fully the object in view, and the mode of making the observations ; and that gentleman and the other members of the Committee of the Company, with the zeal for the advancement of science for which they have long been distinguished, early in 1835 transmitted copies of my letter to the several chief factors in charge of districts in the fur countries, with instructions for them to comply with the directions therein expressed.

Pits were dug at upwards of fourteen different posts in the autumn and winter following, but the reports of the results did not reach me until the beginning of the present month (November 1840). In the mean time, the inquiry had been rendered more interesting to scientific men in England by Professor Baer's papers on "*The Ground Ice or Frozen Soil of Siberia*," published in the Journal of the Geographical Society for 1838 ;* and not being aware that my former letter had been acted upon, I again drew up a paper having similar objects in view, which was printed in the Geographical Journal for 1839, together with "Some notes on the best points in British North America for making observations on the Temperature of the Air." Copies of both were transmitted that same season to Hudson's Bay, together with twenty-six thermometers, carefully constructed by Newman. These thermometers were ordered by the council of the Royal Geographical Society, but the governor and committee of the Hudson's Bay Company liberally determined to defray the expense themselves. Unfortunately the greater part of the thermometers were destroyed by accident in the overland journey, and some of the remaining ones were lost in the winter by their *ivory* scales curving and breaking the tubes ; a mischance which has not happened to thermometers with metal scales in that country. By these accidents we have been deprived for a time of a knowledge of the mean temperature of the atmosphere in the northern zones of America, and consequently of the means of calculating theoretically the depth of the frozen soil in the different latitudes.

* See also this Journal, vol. xxiv., p. 435, and vol. xxv., p. 417.—*Edit.*

The "Extracts of letters" printed in the following pages contain all that each observer reports on the state of the pits dug in 1835 and 1836; but although the zeal with which the trials were made cannot be too highly commended, yet it is to be regretted that the reports are in some instances too concisely drawn up, and a few minute but important particulars have been omitted or overlooked.

Professor Baer remarks that, "If we examine ground which contains only very little moisture in a frozen state, it is very difficult to detect the ice, as it forms an extremely thin partition between the single particles of earth. Should the moisture be more considerable before the freezing comes on, we perceive in its frozen state small pieces of ice, wherever the spaces between the particles are large enough to admit of them. These pieces of ice, which look like small crystals, I have particularly noticed between the upper layer of soil which is thawed, and the lower layer in a frozen state." Attention to these remarks will be very useful in examining a pit dug in sandy soil. A thermometer (as in the case of the observations made in the York Factory) plunged into the soil at the bottom of the pit, will preclude mistake, and should it stand above $+32°$ F., will shew that the earth in which it is placed is not frozen. The effect of warming at the fire pieces of hard earth chipped up by the pickaxe, may also be tried. On one occasion, when exploring the banks of the Mackenzie, I broke off a piece of a solid stratum by a smart blow of my hammer, and on examining its grain, concluded that it was a very hard sandstone. Having labelled the specimen and deposited it in my pocket, it was shortly thawed by the heat of my body, and I discovered that it was merely sand containing much frozen water in its interstices. The thawing of this stratum causes a constant crumbling of the cliff at the mouth of Bear Lake River.

"The farther we go east" (in Europe and Siberia), says Professor Baer, "the more southerly do we find the limit of perpetual ground ice. It has not been observed in Lapland out of the mountainous districts, nor did I ever hear of it at Archangel, though Herr Schrenk assured me that, on the Petchora, the ground at a certain depth is never free from ice. Humboldt found in the district of Boguslowsk, in Lat. 59¾° N., Long. 60° E., at the foot of the Ural Mountains, small pieces of ice at the depth of six feet in summer; but Boguslowsk lies very high. No permanent ice has been found in Tobolsk in 58° N. At Berezov, in 64° N., where Erman found the temperature of the ground, at the depth of twenty-three feet, to exceed ($+1°$ R.) $+34.22°$ F., a dead body was found in 1821, which had been buried ninety-two years in a bed of ground ice, shewing no signs of decay; and we learn from Belawski, that the lower parts of the district are never without ice. So that Berezov is probably very near the limit of perpetual ground ice; for it is clear that *peculiarities of soil must have considerable influence in countries which lie near this limit.* Farther east this frozen soil extends much more to the southward. Georgi, in the last century, related that ice remained in the ground throughout the whole year, upon an island in Lat. 52° N., Long. 106° E., in the environs of Lake Baikal." "In the district of Nertchinsk, Lat. 52°, the ground thawed in summer from one to nine feet, according to its exposure to the sun's rays, but beneath this the frost extended to the depth of forty-two feet from the surface, when the intervention of solid rock prevented farther search. On one occasion, Captain Frehse having dug down six feet through the frozen ground, came to pure ice 2¼ feet thick, enclosing boulders of different rocks. Still farther east again, the perpetual ground ice is found at a less depth southwards, probably because the neighbourhood of the sea raises the temperature of the soil. Erman, at least, found no ice at Okhotsk." (BAER, Geogr. Journ. vol. viii.)

The observations on the temperature of the atmosphere in North America hitherto recorded, and the course of the line of termination of the forests towards its arctic extremity, indicate that the isothermal lines dip, as in Siberia, to the southward in their course from west to east through the continent, and the reports of the frozen soil detailed below tend to the same conclusion. Indeed, it has long been said that the climate of the north-west coast of America is milder and opener than that of the eastern coast, and the difference of mean temperature in the 57th parallel is stated by Baer to be 18° F.* The observations of frozen soil made on opposite sides of James's Bay are too few to found much upon, especially as they are not supported by reports on the mean temperature of the air, but they seem to denote a milder climate on the east side of that bay than on the west. Were this proved to be actually the case, it might be considered merely as another example of the fact, that the western coasts of continents have milder climates than the eastern ones, but it might also be adduced in support of an explanation which I have elsewhere† endeavoured to give of the cause of the low temperature of the coast of Hudson's Bay, namely, the detention of

* *Vide* Richardson, *Therm. Obs. Geogr. Journ.* vol. ix. p. 380.

† Appendix to Franklin's Journey, and to Back's Journey in 1833-35.

fields of ice in its neighbourhood for nearly the whole summer, resulting from the form of the land, and the direction of the prevailing winds and currents of the sea.

As to the local influence of soil to which Professor Baer directs our attention, I have been informed by a gentleman who has had forty years' experience as a practical farmer in England, that when he used *sand* as a covering for potatoes, carrots, or other vegetables, in the winter, the frost penetrated farther than when loam or other earth was used, and that the looser the latter was thrown over the vegetables, the better was the protection it afforded. He remarked, also, that he always found the earth frozen to a greater depth after severe frosts under a beaten footpath in a field, or compact gravel on the highway, than in loose soil.

Fort-Simpson, on the Mackenzie, being nearly in same latitude with Yakutzk (62° 11′ N., and 62° 1½′ N.), is a desirable locality for ascertaining the thickness of the permanently frozen stratum. At the latter place a well has been sunk into the frozen soil to the depth of 382 feet, and the temperature of the earth, which was +18.5° F. at some feet below the surface, gradually rose to +31.9° F. at the bottom of the well, where the soil was so loose as to require timbering, which it had not done higher up. This gives a rise of temperature equal to one degree of Fahrenheit's scale for every 28⅓ feet of descent at a place where the mean heat of the year is about +14° F.* In Phillips's Guide to Geology, the mean increment of heat is stated to be 1° F. to every forty-five feet of descent from the surface of the earth, which is one-third less quick than the above. At Fort-Simpson the mean temperature of the air is about +25° F., and the frozen soil was found to extend at least seventeen feet from the surface. A thermometer was kept at the bottom of the pit for some time, but the register of its indications has not reached me. Mr McPherson not having been made aware of the

* M. Erman (Geograph. Journ. viii. p. 213) states the mean heat at Yakutsk, as deduced from observations made thrice a-day in the year 1827, to be +18.72° F. The mean stated in the text is also from M. Erman's observations, but continued for several years, and was found by drawing the daily curve for the year indicated by the means at the specified hours.—(Geogr. Journ. ix. p. 380.)

importance of searching for spiculæ of ice in the looser and dryer subsoil, or of ascertaining and registering its temperature, the depth of the frost cannot be said to be yet perfectly ascertained at that place. I am indebted to him for a register of the temperature of the atmosphere, carefully kept at Fort-Simpson for three years, and for much useful verbal information respecting the Mackenzie, where he has wintered for nearly eighteen years. He is now in this country, and should he return to his old quarters, we trust that he will speedily set the matter at rest as respects that locality.

Pits were commenced at Fort-Chepewyan and Great Slave Lake, but discontinued, I believe, on coming to the rocky strata which lie near the surface, and no reports were made.

Table containing the Geographical Position of the Posts named in the subjoined Report, and the principal facts ascertained.

Places.	District.	Date.	Remarks.
Woswonaby, Lat. 49° 20′ N. Long. 76° 25′ E.	Rupert's River district. Interior, elevated?	Nov. 3. 1835,	A pit dug in the potato-garden to the depth of 7 feet without discovering any frozen soil.
......	North of Abitibbe Lake,	Jan. 18. 1836,	In an open potato field, 63 paces from the borders of the lake, snow lying 1 foot deep, soil frozen to the depth of 2 feet 3 inches from the surface.
......	......	...	In a thick wood 3 furlongs from the lake, snow 2 feet 5 inches. Soil sandy. Soil frozen to the depth of 1 foot 7 in.
......	......	...	Potato field half a mile in the woods. Snow 2 feet. Loose sandy soil frozen 1 foot deep.
......	......	...	Ten paces from the lake. Soil coarse gravel, frozen to the depth of 2 ft. 8 in.
Michiskam, Lat. 50° 30′ N. Long. 76° 40′ W.	Rupert's River district. Interior, elevated?		Soil never permanently frozen; but in the potato field the frost penetrates 3½ feet in the winter, and in the woods only 6 inches. Soil sandy.
Moose Factory, Lat. 51° 15′ N. Long. 80° 55′ W.	Mouth of Moose River, at bottom of James' Bay,	Sept. 13. 1835,	No frozen soil in a pit dug 12 feet deep, when the influx of water through the gravel put an end to the operation.
Rupert's House, Lat. 51° 26′ N. Long. 78° 40′ W.	East side of James' Bay, on the coast,	Between Oct. 16 & Nov. 17. 1835,	Several pits dug from 6 to 8 feet deep in banks of sand, shingle, and clay, from 50 to 60 feet high. Much water flowed in.

Places.	District.	Date.	Remarks.
Rupert's House, Lat. 51° 26′ N. Long. 78° 40′ W.	East side of James' Bay, on the coast,	April 28 and 29. 1836,	A pit dug in open ground, exposed to the NW. winds from Rupert's Bay, and 50 feet from the face of the bank. The frozen soil extended 7 feet from the surface. Under a snow-drift 8 feet thick the soil was frozen only 13 inches deep.
Eastmain Lat. 52° 19′ N. Long. 78° 40′ W.	East side of James' Bay, Rupert River District, on the coast,	Sept. 1. 1835,	Situation exposed. Dug 7 feet 8 inches in unfrozen soil, and drove a stake through the soft earth 5 feet farther, in all 12 ft. 8 inches. Stopped by the influx of water. (Compare this with the observations at Albany, in same latitude.)
......	......	Sept. 18. 1835,	Dug 8 feet 3 inches, until stopped by water, when a stake was driven 7 feet lower. In all 15 feet 3 in. of soft soil.
......	......	Sept. 29. 1835,	Stake driven with ease into a swampy piece of ground 12 feet deep.
......	......	Feb. 27. 1836,	Pit dug in the burying-ground, inclosed by stockades, when the frost was found to have penetrated 8 or 10 inches into the soil. The snow-drift lay 8 feet deep over the spot.
Albany, Lat. 52° 15′ N. Long. 81° 53′ W.	West side of James' Bay near the coast,	Sept. 2. and 3. 1835,	Thawed surface soil 6 inches; 3 feet 7 inches frozen earth; beneath which a brown clay, so hard that it required to be cut with an ice-chisel, cut into 11 inches; in all 5 feet. Query, *Was not the clay Mr Corrigal mentions frozen?* It would have been desirable to have placed it near the fire to see if it became more plastic.
Green Lake, Lat. 54° 17′ N. Lon. 117° 30′ W.	Interior, on a small lake, at least 350 yards above the sea,	Oct. 15. 1835,	A pit dug 15 feet deep, 40 yards from the lake; 2 feet of gravel on the surface; rest compact clay, so hard at bottom that it was impracticable to dig it deeper. Query, *Was it frozen? Would it have softened by heat?*
Isle à la Crosse, Lat. 55° 25′ N. Lon. 107° 53′ W.	Interior, on the shore of a lake. Perhaps 300 yards above the sea,	Oct. 15. 1835,	2 feet gravel and sand and 8 of hard clay, when water flowed into it. Query, *Did the water come in at the bottom?*
Severn Outpost, Lat. 56° 00′ N. Long. 86° 15′ W.	Severn River, west side of Hudson's Bay,	Autumn,	A frozen stratum reached at the depth of 11 feet, in a loose sandy soil, when the surface-water filled the pit.
......	......	...	A pit dug 7 feet in another place was filled with water before the frozen soil was reached.

Places.	District.	Date.	Remarks.
Severn Outpost, Lat. 56° 00′ N. Long. 86° 15′ W.	Severn River, west side of Hudson's Bay,	Autumn,	Frozen earth at the depth of $5\frac{3}{4}$ feet in compact blue mud. The frozen stratum was cut into $7\frac{1}{4}$ feet, when a snow storm put an end to the operation. Pit 13 feet deep.
......	...	...	The river bank is 30 feet high, and on cutting into it, to make a quay, the frost was found extending downwards till it reached high-water mark, at which point the earth was soft, as far as the experimenter could penetrate horizontally.
York Factory, Lat. 57° 00′ N. Long. 92° 26′ W.	West side of Hudson's Bay, 5 miles from the mouth of Hayes' River,	Oct. 1835,	Surface water solid. New frost to the depth of 8 inches. Soil, blue tenacious mud, with boulders. Pit 30 yards from the river. Thaw 3 feet from surface. The digging into the permanently frozen stratum continued for fourteen days, when the thawed earth was again reached at the depth of $20\frac{1}{2}$ feet from the surface, leaving $17\frac{1}{2}$ feet of permanent frost, beneath which the temperature of thawed mud was found by repeated trials to be +33° F.
Fort Simpson, Lat. 62° 11′ N. Lon. 121° 32′ W.	Influx of the south branch of the Mackenzie into the main stream, about 80 yards above the level of the Arctic Sea,	Oct. 19. 1836,	Pit 80 yards from the banks of the river. Soil heavy, sand and clay mixed. Thawed surface soil 10 feet 7 inches. Frozen substratum 6 feet 3 inches. 8 feet more through a loose sandy soil, and a rod thrust 4 feet deeper into the bottom. In all 29 feet. A thermometer kept at the bottom of the pit shewed a higher temperature than one exposed to the air. The mean temperature of October here is about +23° F. Mean annual heat +25° F.*

* These facts ascertained partly by verbal communications with the experimenter, Mr Murdoch McPherson.

Our previous knowledge of the depth to which the summer thaw penetrates, or the extent of the frozen substratum in North America, was very limited.

At Fort Franklin on Great Bear Lake, the summer thaw, on the 5th of October 1825, had penetrated 21 inches into the sandy platform on which the house was built. The frozen substratum was dug into about 6 feet, but its total depth was not ascertained. Fort Franklin is supposed to stand about 230 feet above the sea-level. The mean heat of the year 1825–6 was + 17.61° F., and of the first ten days of October + 19.4° F. On the 9th of the same month the small lakes were frozen over, and on the 11th, the snow lying on the ground put an end to the further action of the sun on the soil for that season.

July 5. 1826.—On the east bank of the Mackenzie, in Lat. 68° 37′ N., a hole was dug in the sandy soil, at the foot of the Rein-deer Hills, to the depth of 3 feet, without reaching the frozen soil.

July 17. 1826.—On an island of the Arctic Sea, Lat. 70° 10′ N.; Long. 129° 15′ W., composed of sand and slaty clay, the thaw had penetrated scarcely a foot.

July 28. 1837.—In Lat. 70° 50′ N., on the coast to the westward of the Mackenzie, Mr Dease found the soil thawed only to the depth of 4 inches.

Royal Naval Hospital at Haslar,
20th November 1840.

No. 1.—*Extract of a Letter from George Keith, Esq. chief factor, dated Moose Factory, August* 8. 1836.

"Early in the morning of September 13. 1835, two labourers commenced digging a pit on the island of Moose-factory, in an open, dry situation, 170 paces from the bank of the river. The pit was seven feet long by six wide, and the operators penetrated to the depth of ten feet perpendicular without discovering anything remarkable, the soil being a fine, light, sandy-coloured marl throughout. At this stage, however, water began to make its appearance at the bottom of the pit, and, on digging two feet lower, it increased so much as to put a stop to further excavation. During the progress of digging the last two feet, the soil became rapidly mixed with sand and gravel, and, latterly, almost pure gravel without a sign of frost anywhere. In fact, it has been clearly ascertained that the winter frosts, in dry and exposed situations, do not penetrate beyond five and a half feet; and about the skirts of the forest, which is a wet and marshy soil, the depth of the frost does not exceed three feet. Last winter, which was esteemed severe, the thermometer in the shade fell only once to 35° below zero, namely, on the 30th November, the coldest weather we experienced, which was rather singular, as the colds of January, February, or March, are generally felt to be the most intense."*

No. 2.—*Extract of a Letter from chief trader Robert Miles, Esq., dated Ruperts' House, East Maine, June* 25. 1836.

"From the 16th of October to the 7th of November last, several pits were dug from six to eight feet deep, in all of which much water was found. Indeed, in every part of the vicinity of this establishment, which is situated on banks from fifty to sixty feet high, the earth, being of a sandy nature, over beds of gravel, shingle, and clay, is full of water springs.

"On the 28th and 29th of April last, I caused a pit to be dug in a clear piece of ground in the most exposed situation here, being open to the north-west winds from Rupert's Bay, and in this we did not get through the frost until we penetrated exactly seven feet. This pit was distant fifty feet from the face of the bank.

"One hundred and forty-five yards from the above-mentioned pit, I caused a snow-drift within the stockades of the establishment, eight feet deep, to be cut through, and the earth beneath was found to be frozen to the depth of thirteen inches only."

No. 3.—*Extract of a Letter from Mr Thomas Corcoran, clerk, dated East Maine-post, Rupert's River District, September* 30. 1835.

"On the 19th September 1835, a pit was dug in the plantation in a spot not sheltered with wood, willows, or anything else, and where the snow does not lodge at any time to a greater depth than eight or ten

* "Halley's comet seen from the 12th to the 18th of October 1835, after which it was lost sight of during a succession of snowy weather."—G. K.

inches, and consequently as much exposed to the influence of frost as a place can be.

	Feet.	Inches.
Dug with the spade to the depth of	7	8
Pierced with a wooden stake from the point where the use of the spade was left off, owing to water, to the depth of	5	0
No frost or ice discovered to this depth, . .	12	8

Quality of the Soil.

Feet.	Inches.	
0	3	Loose mould.
0	5	Mixture of whitish loam and sand.
1	3	Pure sand.
2	0	Loam and sand.
2	0	Loam of a lead colour.
0	8	Lighter coloured loam.
1	1	Lead-coloured loam.
7	8	

From the appearance of the earth adhering to the stake, the next five feet was judged to be lead-coloured loam.

"On the 28th September 1835, a pit was dug about a mile to the eastward of the house, in a green bluff of pines, and consequently sheltered from drift.

	Feet.	Inches.
Dug with the spade to the depth of	8	3
Pierced with a wooden stake after the water flowed in, .	7	0
No frost or ice discovered to this depth, . . .	15	3

Quality of the Soil.

Feet.	Inches.	
0	4	Light mould.
1	1	Sand.
1	10	Sand and lime.
1	2	Whitish-coloured loam.
3	10	Lead-coloured ditto.
8	3	

The quality of the remaining depth supposed to be loam.

"On 29th September 1835, a stake of twelve feet in length was driven with ease into a small swamp that is tolerably well sheltered, being surrounded with pine trees and within a mile of the house. No ice was discovered at that depth. In this situation the spade could not be used owing to water.

"On the 27th February last, a grave was dug here in the burying ground, which is enclosed by stockades six feet seven inches high, for interring the remains of a poor old woman who unfortunately was frozen to death on the 25th of the same month. In digging this grave it was discovered that the frost had not penetrated into the soil above eight or ten inches, but then the drifted snow lay as high at least as the stockades that surrounded it."

"From the foregoing observations, it may be inferred that the soil in this neighbourhood, to the distance of a mile in any direction from the house, is not permanently frozen."

No. 4.—*Extract of a letter from Mr James Kellock sen., dated Michiskam Post, 1st January* 1836.

"I beg leave to inform you that the earth is never permanently frozen in this section of the Honourable Company's territories, but I found that the frost had penetrated to the depth of three and a half feet in the potato-fields during the course of last winter, whereas, in the woods, six inches was as far as it had gone down. The soil in both places is sandy, with a few small stones."

No. 5.—*Extracts of a letter from Mr John Spencer, postmaster, dated Woswonaby, Rupert's River District, 28th January* 1836.

"I have caused four different places to be dug, 1*st*, In an open potato-field about sixty-three paces from the border of the lake, the depth of snow measured one foot; the soil is light at the top, and rather inclining to small gravel for nearly a foot, after which it is of a somewhat clayey nature. The wood surrounding it is principally dwarfish pine, with a few silver pines and birches interspersed, and the depth of frozen ground was two feet three inches.

"2*d*, In a thick wood, about three furlongs from the lake, where the snow lay about two feet five inches deep, and the soil is of a more sandy nature throughout, except a small surface-layer of vegetable matter, the ground was frozen to the extent of one foot seven inches.

"3*d*, In a potato-field, about half a mile in the woods, the depth of snow was one foot, and the loose and sandy soil was frozen to the depth of one foot.

"4*th*, On the borders of the lake, about ten paces from the water's edge, the snow was but trifling, and there were bare patches here and there. I chose one of them, and found the ground frozen to the depth of two feet eight inches before we penetrated to the unfrozen soil. The place was very stony and troublesome to dig, and the poor pick-axe had much employment before we discharged it; but, after we got down a little, clay resembling blue marl began to make its appearance. It happened to be a bitter cold day when the men and myself executed the above on the 27th instant, but, for want of a thermometer, I was unable to ascertain the exact temperature, but judge that it might be 25° below zero.

"On the 3d of November last, I dug a pit in the potato-garden to the depth of seven feet, and not finding any symptoms of frost, considered that further pursuit was unnecessary. The weather at that time was uncommonly beautiful, and unlike the rugged weather which we generally experience at this season.

"It may not be unworthy of remark, that there is a strip of open water to be seen on the lake all the winter round, even in the severest weather, for the distance of about three miles in the circuitous course it takes to Net Point. This is supposed to be the leading channel to the main river, but no current is perceptible; the open water must therefore be caused by the nature of that part of the bed of the lake, for the cold at times is sufficiently severe to freeze every thing but an actual cascade."

No. 6.—*Extract of a Letter from chief trader Jacob Corrigal, Esq., dated Albany, 3d September* 1836.

"I beg to inform you, that, in order to ascertain the temperature of the earth at this place, we dug a pit in the course of yesterday and this morning, and found the soil thawed at the depth of five feet, and frozen earth three feet seven inches. The soil at the surface six inches deep of black earth, from that and nearly to where the frozen earth terminates, there was a brownish-coloured clay, so hard that it was not perceived to be thawed earth, till after it was cut about two inches deep with a nice chisel. A few small pieces of shells were found amongst the blue clay."

No. 7.—*Extract of a Letter from Mr Chief Factor R. M'Kenzie sen., dated Isle à la Crosse, 1st June* 1836.

"Two pits were dug 15th October last. One at Isle à la Crosse, thirty yards from the lake, to the depth of ten feet, when water began to come up, two feet of gravel and sand on the top, and eight of hard clay ground. And one at Green Lake, to the depth of fifteen feet forty yards from the lake, two feet of pure sand, and thirteen of clay ground, but so hard at the bottom that it was impracticable to dig it deeper, and no frozen earth to be found. I have heard it frequently said by old servants who have resided here for upwards of thirty years, and who have had occasion to dig pits and graves at all seasons of the year, that they never knew the frost to penetrate into the earth more than from two to three feet deep."

No. 8.—*Extract of a Letter from Mr Chief Trader James Hargrave, dated York Factory, 25th August* 1836.

"At this place the digging of a pit to ascertain the principal facts, commenced in the beginning of October last. The surface water being then completely solid, and any addition to the depth of last summer's thaw being despaired of, as the new frost had already penetrated eight inches into the ground. The spot selected was the driest in the vicinity of this factory, perfectly exposed to all changes of atmosphere throughout the season, and upwards of thirty yards from the bank of the river. The soil is a deep blue tenacious mud. The periodical thaw was soon dug through, and when accurately measured, was found to be exactly three feet from the surface-level to the beginning of the permanent frost. From this point the operation continued for about fourteen days, at the end of which time the thawed earth was again reached, the distance from the surface of the ground being twenty and a half feet, thus leaving a stratum of seventeen and a half feet of perpetual frost. It may, however, be material to remark, that, on close examination, the total depth of the pit was found to approximate very nearly to the height of its mouth above high-water mark in its vicinity, which may lead to a suspicion that the layer of permanent frost might have been thicker had the river bank been higher at the place where the pit was dug. The "blue mud" above noticed was found to extend to the lowest point reached, varied at points from three to five feet asunder by layers of small gravel mixed with solid ice. Each layer about from three to five inches thick. Shells, such as are still found about the shores of the stream, were discovered imbedded in the mud about twelve feet from the surface in a state of perfect preservation; pieces of drift-wood (willow) were dug out about nine feet deep, some of them quite fresh. The temperature of the thawed mud immediately below the permanent frost was ascertained by repeated trials to be + 33° on Fahrenheit's scale.

"A similar attempt was made last autumn to ascertain the like facts at Severn outpost; but, from various adverse circumstances, the result was not so satisfactory. The first pit was dug through a loose sandy soil, eleven feet deep, at which point the permanent frost was reached. The surface-water, however, filled the pit, and the party being unprovided with adequate means to keep it dry, another attempt was made elsewhere.

"In this second pit, the labours were again interrupted by water, after having dug through seven feet of thawed ground without reaching the frost. A third attempt was made in a close compact soil of blue mud, about twenty yards from the bank of Severn river, in which they reached the permanent frost at the depth of 5¾ feet from the surface. The labourers continued cutting the solid frost to an additional depth of 7¼ feet when the surface-water, together with a snow-storm, put a stop to further proceedings that season.

"The postmaster, who superintended the undertaking at Severn, mentions that, having occasion to cut into the river-bank (which at that place was above thirty feet in perpendicular height), for the purpose of making a summer quay, he found the frost extending downwards till it reached the mark of high water, below which the earth appeared quite soft and thawed, as far, in a horizontal direction below the frost, as he could penetrate. This circumstance appears to coincide with what was observed at York Factory, and to lead to the conclusion that permanent frost descends to the point where it first meets with the level of water."

No. 9.—*Extract from a Letter from Mr Chief-Trader Murdoch McPherson, dated Portage la Loche, 4th August* 1836.

"A copy of a letter from Dr Richardson to Governor Pelly, dated Melville Hospital, 25th February 1835, was transmitted to me, together with instructions from Governor Simpson to furnish the information required; but I am sorry to inform you that those instructions did not reach me till late in December, when the ground was so deeply frozen that experiments of this kind became altogether impracticable for the season; and the period at which I travelled on the Mackenzie River this spring was too early for the occurrence of recent land-slips or rents in the earth, by which the question might have been resolved. I shall, however, avail myself of the close of the summer to obtain the required information."

No. 10.—*Extract of a Letter from Mr Chief-Trader Murdoch McPherson, dated Fort Simpson, Mackenzie River,* 1837.

"I beg leave to send you the following report of a pit dug at this place on the 19th of last October (1836). The pit was made at the distance of eighty yards from the banks of the river, in a heavy soil of sand mixed with clay, and in a situation free to the action of the sun during the summer. The result was 10 feet 7 inches deep of thawed soil, from the surface of the ground to that which is permanently frozen, and 6 feet 3 inches deep of frozen soil (permanently frozen, as I believe) between that which thaws during the summer and that which never freezes."

On the Cultivation of the Cerealea in the High Latitudes of North America. Comprised in the extract of a Letter from Peter Warren Dease, Esq., Chief Factor of the Hudson's Bay Company to Dr Richardson. Communicated by the latter.

The following extract of a letter from Mr Dease (to whose intrepidity, skill, and intelligence, conjoined with the able assistance of his lamented colleague Mr Thomas Simpson, we owe the completion of the survey of the northern coast of America from Back's River to Behring's Straits) furnishes a further illustration of the nature of the North American climate, and is therefore closely connected with the preceding and following articles.

"In regard to the cultivation of grain on Mackenzie's River, I can only say, that although we had *wheat* in perfection in 1828 from Fort aux Liards (Lat. 60° 5′ N.; Long. 122° 31′ W.), that was the only season out of several in which the grain ripened; and the culture of wheat is reported to be equally precarious at Dunvegan, Peace River (Lat. 56° 6′ N.; Long. 117° 45′ W.). Most places lying near the Rocky Mountains are liable to summer frosts, and I observed the same thing during my residence at Fort St James (Lat. 54° 30′ N.; Long. 124° W.) on Stuart's Lake in New Caledonia; though I understand that at Fort George (Lat. 54° 35′ N.; Long. 125° W.) on Frazer's Lake, and at Alexandria (Lat. 52° 58′ N.; Long. 123° W.) on Frazer's River, good crops of wheat are raised. The whole average about four months in coming to maturity. *Barley* has been cultivated to advantage at Fort aux Liards, Fort Simpson (Lat. 62° 11′ N.; Long. 121° 32′ W.), and Fort Norman (Lat. 64° 41′ N.; Long. 124° 45′ W.), and generally takes three months to ripen. *Oats* have come to perfection at Fort aux Liards, and at Fort Simpson, but have not been sown at Fort Norman yet, though no doubt they might thrive as well there as at the two former places, and ripen about a fortnight sooner. I know of no vegetable but the turnip which has been successfully tried at Fort Good Hope (Lat. 67° 28′ N.; Long. 130° 52′ W.), but that root was far better on the island of the old fort below the ramparts (Lat. 66° 18′ N.; Long. 128° 32′ W.) than on the more elevated mainbank opposite, where the soil is also more dry. Fort Norman had a tolerably good crop of potatoes, barley, cabbages, &c."

25th Nov. 1840.

Register of the Temperature of the Atmosphere, kept at Fort Simpson, North America, in the years 1837, 1838, 1839, *and* 1840. By Murdoch McPherson, Esq., Chief Trader of the Hudson's Bay Company; abstracted by John Richardson, M.D., F.R.S., Inspector of Naval Hospitals, Haslar.

Fort Simpson, a post belonging to the Hudson's Bay Company, is situated on the Mackenzie, at the confluence of its south branch, named the River of the Mountains, and 670 miles from the Arctic Sea, in Lat. 62° 11′ N.; Long. 121° 32′ W. The fort stands on the west bank of the river, about 40 feet above its channel, and, if we allow 3 inches a mile for the fall of the river from thence to its mouth, the position of the thermometer may be estimated to be about 250 feet above the level of the sea. The temperature was registered in the morning and evening at 8 o'clock, and for two years at 2 P.M. Mr McPherson informs me, that the times of observation were not always exactly adhered to, though, in general, they were very nearly so. With the exception of the time of Mr McPherson's annual absence from the Fort in September, the register is complete for the autumn, winter, and spring months of three years, but the temperatures of the summer months were obtained in the year 1838 alone. In a paper which I published in the Journal of the Geographical Society (ix.) on Sir Edward Parry's Thermometrical Observations in the Arctic Regions, it is shewn that the mean of the temperatures at the hours 8 A.M. and 8 P.M. combined, does not differ more than three-tenths of a degree from the annual mean of the whole twenty-four hours at any of the places of observation, and is always in defect. I have, therefore, assumed the means of Mr McPherson's observations at these two hours as giving very nearly the true annual mean. Having found, also, by consulting a considerable number of North American registers of temperature, that the hourly curve for September falls always between that of May and August, and generally somewhat nearer to the former than to the latter, I have supplied the defect of observations for that month by assuming + 48° F. as its mean heat.

The thermometer employed was a spirit one made by Carey, which, when compared with several constructed by Newman, was found to keep nearest the mean of the whole. No correction has been made for the diminishing contraction of the spirit at low temperatures.

Abstract of the Thermometrical Register kept at Fort Simpson for one year 1837–38. *(The temperature for September interpolated.) (Spirit Thermometer by Carey. Fahr. Scale.)*

Months and Seasons.	Mean Heat in the Shade (Fahr.)			
	At 8 A. M.	At 2 P. M.	At 8 P. M.	8 A. M. 8 P. M.
1837.				
September, . .	+ 46.000	+ 60.000	+ 50.000	+ 48.000
October, . . .	+ 20.294	+ 28.470	+ 25.882	+ 23.088
November, . .	+ 6.700	+ 11.300	+ 12.133	+ 9.417
December, . .	— 10.387	— 9.613	— 9.935	— 10.161
1838.				
January, . .	— 21.258	— 18.419	— 18.419	— 19.839
February, . .	— 9.321	— 5.000	— 5.214	— 7.268
March, . . .	— 8.064	+ 2.484	+ 1.452	— 3.306
April, . . .	+ 17.933	+ 32.000	+ 29.367	+ 23.650
May, . . .	+ 44.903	+ 54.806	+ 49.516	+ 47.210
June, . . .	+ 62.067	+ 73.400	+ 64.933	+ 63.500
July, . . .	+ 55.871	+ 70.613	+ 65.419	+ 60.806
August, . . .	+ 48.501	+ 65.871	+ 57.742	+ 53.161
Whole Year, . .	+ 21.228	+ 30.636	+ 27.064	+ 24.146
Autumn. *September—November,*	+ 24.287	+ 33.204	+ 29.303	+ 26.795
Winter. *December—February,*	— 13.800	— 11.211	— 11.389	— 12.594
Spring. *March—May,* . .	+ 18.261	+ 29.763	+ 26.750	+ 22.502
Summer. *June—August,* . .	+ 55.436	+ 69.924	+ 62.783	+ 59.109

By adding a quarter of a degree to the mean heat at 8 A. M. and 8 P. M. combined, the mean heat of the year 1837–38 is estimated to be nearly + $24\frac{1}{2}$° F.

Abstract of the Thermometrical Register kept at Fort Simpson in the years 1837–38 *and* 1838–39. *(Means of two successive years.) (Temperature for September interpolated in calculating the Mean Temperature of Autumn.)*

Months and Seasons.	Mean Heat in the Shade.			
	At 8 A. M.	At 2 P. M.	At 8 P. M.	At 8 A. M. & 8 P. M.
1837–38.				
October,	+ 20.977	+ 30.080	+ 26.479	+ 23.728
November,	+ 8.034	+ 12.517	+ 12.034	+ 10.034
December,	— 8.516	— 7.984	— 8.339	— 8.428
1838–39.				
January,	— 20.855	— 17.548	— 18.129	— 19.493
February,	— 6.857	— 1.679	— 2.089	— 4.473
March,	— 4.403	+ 12.677	+ 7.065	+ 1.331
April,	+ 23.583	+ 47.216	+ 34.900	+ 29.242
May,	+ 41.993	+ 56.985	+ 50.612	+ 46.303
Autumn. *September—November,*	+ 24.959	+ 34.153	+ 29.471	+ 27.215
Winter. *December—February,*	— 12.250	— 9.317	— 9.747	— 11.000
Spring. *March—May,*	+ 20.356	+ 38.892	+ 30.815	+ 25 586

The mean heat of these two years, including the single summer of 1838, obtained by making a small addition to the mean of 8 A. M. and 8 P. M. combined, is a little above + 25½° F.

Abstracts of the Thermometrical Register kept at Fort Simpson in 1837–8, 1838–9, *and* 1839–40. *(Means of three successive years.) (September interpolated in calculating the Mean Autumn Heat.)*

Months and Seasons.	Mean Heat in the Shade.		
	8 A. M.	8 P. M.	8 A. M.—8 P. M.
1837–38–39.			
October,	+ 20.726	+ 25.674	+ 23.200
November,	+ 5.866	+ 9.166	+ 7.517
December,	— 9.409	— 9.032	— 9.221
1838–39–40.			
January,	— 14.409	— 12.505	— 13.457
February,	— 12.306	— 8.553	— 10.429
March,	+ 0.204	+ 8.742	+ 4.473
April,	+ 22.055	+ 29.833	+ 25.944
May,	+ 44.605	+ 51.168	+ 47.886
Autumn. *September—November,*	+ 24.160	+ 28.251	+ 26.206
Winter. *December—February,*	— 12.033	— 10.074	— 11.054
Spring. *March—May,*	+ 22.291	+ 29.915	+ 26.103

The mean temperature of these three years, calculated as in the preceding tables by using the summer heat of 1838, and adding a quarter of a degree for the defect of the mean of the pair of hours 8 and 8 is rather under + 25½° F.

Physical and Chemical Examination of three Inflammable Gases which are evolved in Coal-Mines. By Dr Gustav Bischof, Professor of Chemistry in the University of Bonn. Communicated by the Author. (Concluded from Vol. xxix. p. 333.)

§ IX. *Analysis of Pit-gas in the Detonation-tube.*

The exact analysis of an inflammable gas in the detonation-tube is, as is well known, somewhat difficult, because only small quantities of gas can be employed. If the analysis be repeated several times, considerable differences take place. The cause of this is principally, that the temperature of the gas is not particularly determined, and that gases, even when standing over mercury, become gradually mixed with atmospheric air. In order as much as possible to remove these two inconveniences, I mixed, in a bottle over water, a quantity of pit-gas sufficient for several experiments, with the requisite quantity of oxygen. I did not fill the bottle entirely with the mixture, but left a very small quantity of water behind. After the bottle had been closed under water by a cork provided with a very narrow long bent (S) tube, it was reversed, and the opening of the tube cut off by mercury. By heating the bottle very slightly, a portion of the gaseous mixture was expelled, which was allowed to enter the detonation-tube filled with mercury, after the first bubbles which contained the atmospheric air of the tube, together with some drops of water, had been permitted to pass over. As, after the cooling of the bottle, the mercury rose in the narrow tube, the inclosed gaseous mixture remained of itself cut off, and the unchanged position of the metal in the tube was at the same time a sign of the whole being air-tight. In this manner several experiments could be performed with the same gaseous mixture, one after another, without any admixture of atmospheric air.*

* I found afterwards that the gaseous mixture, when it remains standing in the bottle for several days, becomes gradually mixed with atmospheric air, without the mercury sinking in the tube. Thus, on the succeeding days, when the detonations were repeated, there was a constant diminution of the products of the detonation, which could only arise from the atmosphe-

The errors of measurement which occur when the inflammable gas and the oxygen are separately measured in each individual experiment were thus likewise avoided.

The detonation-tubes were graduated by myself with the greatest care. The portions were so long, that each given

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ric air having replaced some of the gas in the bottle. This disadvantageous action may, however, be counteracted by immersing the whole bottle under water, and so removing the cork from contact with the atmospheric air; but I preferred repeating the detonations in different tubes rapidly after one another. This mode of proceeding affords the most exact results. From four to six detonation-tubes are kept ready in the mercurial apparatus, and they are filled immediately after one another with the explosive gas, which is driven out of the bottle by gentle heat. In this way a great deal of time is saved; and the analyses are performed under the same circumstances, thus affording results which harmonize as much as can be desired. There are just two things that must be most particularly attended to, viz. first, that, both before and after the detonation, the gas be not measured until no further changes in its volume can be remarked; and, second, that the potash be left long enough in contact with the gas. The neglect of these two precautions is the source of most of the errors. An hour at least is required ere the gas in the detonation-tube acquires the temperature of the atmospheric air, especially after the detonation, which heats the gas considerably. As to the absorption of the carbonic acid, it is to be remarked that the last portions of that gas are not fully absorbed for some time. This is particularly the case when the gas is in a rarefied state, as, for example, when the gas amounts to but a small quantity, and the detonation-tube is long. Hence it is advisable to incline the tubes as much as possible during the absorption of the carbonic acid. If the tube be perpendicular, and should the mercury within be about 14 inches above its level without, four or five days may elapse before the absorption can be accomplished by means of hydrate of potash. Hence I prefer effecting the absorption by solution of potash, which is allowed to ascend through the mercury. If the detonation-tube is then taken out of the mercurial apparatus, and placed in a vessel filled with water, the absorption takes place very rapidly by means of a little agitation; and the gas left behind can be immediately measured.

It is, as I have already remarked, very difficult to prepare detonation-tubes which remain air-tight for several days. Hence we must not neglect to prove these tubes before using them by filling them with mercury, and, as in the case of a barometer, placing them in a mercurial apparatus with their melted ends uppermost. If the mercury sinks after six or eight days, the tubes can be improved by applying a small grain of sealing-wax, and melting it on the platinum wire by a little heat. We must, however, again prove the repaired tube before employing it, for the imperfect place is not always closed the first time.

The difficulty of inclosing a platinum wire quite tightly in a glass tube quantity of gas could always be brought to a particular graduated line by sinking the detonation-tube in the mercury of the pneumatic trough, or by raising it up. In this manner the errors of a geometrical subdivision, owing to the unequal width of the glass tube, were entirely removed. The detonation-tubes were from 12 to 18 inches long. As much of the mixture of gases was always employed for detonation as the tube could bear without fear of being broken. The internal level of the mercury generally stood at from 10 to 14 inches above the external, and the gas was therefore about half the density of the atmospheric air. This was advantageous, as the violence of the detonation was thus diminished, and the measurement of the gas could thus be made with the same exactness as the observation of the height of the barometer, which could not be effected in the same degree if the interior and exterior mercurial level stood at the same height. Of course

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cannot appear surprising, when we consider that, in cooling, platinum and glass do not contract equally. However intimately united, therefore, the two may be during the melting of the glass, yet an opening must be formed after cooling, which, it is true, will for a short time afford no access to the air, but not for a longer period. According to my experiments, the air-tight inclosure of the wire succeeds more easily in proportion to the slowness of the cooling of the glass tube. Hence, immediately after the platinum wire has been included in the tube by fusion, I am in the habit of placing the tube in a large quantity of red-hot sand, and allowing the two to cool together. In order, moreover, to diminish the difficulties, I insert only *one* platinum wire in the end of the tube, where it succeeds better than at the side. The transmission of the electric spark takes place by an iron-wire being pushed up into the detonation-tube. The mercurial trough has a hole at the bottom, which is closed by a cork, through which the wire is pushed. If the cork be good, the wire quite smooth and a little greased, it can be easily pushed up and down, and can yet so close the aperture as to render it impervious to the mercury. It is of course understood that, previous to the introduction of the gas into the tubes, every trace of potash and moisture must be removed. For this purpose I pour some drops of acetic acid into the tube, then rinse it out with water, and afterwards dry it very carefully with blotting-paper.

If these circumstances are neglected, we cannot be surprised by the general complaint among chemists that the analyses of inflammable gas in the detonation-tube afford results which are so little in unison with one another. But by attending to them on the other hand, we obtain results as accurate as can be wished.

the height of the internal mercurial level above the external was subtracted from the height of the barometer.

The gaseous mixture driven out of the bottle by heat was naturally at a maximum of moisture. The residue of gas was in a similar condition after the detonation. According to the well-known formula of Dalton, this watery vapour was subtracted from the volume of the mixture of gases. The residue of gas, after the absorption of the carbonic acid by solid potash, was of course no longer moist, as the potash had absorbed the watery vapour as well as the carbonic acid.

The pit-gas, which, in order to separate the carbonic acid, had been washed with solution of potash, was detonated with three times its volume of oxygen. The following results of my experiments give the measured volumes, after subtracting the watery vapour and after reducing to 32° F., and 28 inches B. (German).

	Exper. 1.	Exper. 2.	Exper. 3.	Mean.
Mixture of gases, ...	4	4	4	4
Absorption after detonation, .	1.9515	1.9632	1.9457	1.9535
Absorption by means of potash, . .	1.0575	1.0284	1.0339	1.0399*

I shewed, seventeen years ago,† that the analysis by the detonation-tube, of a mixture of gases consisting of one or two or three inflammable gases, affords the true composition if the mixture is composed of hydrogen and carbonic oxide gases, or of hydrogen and carburetted hydrogen gases, or of carbonic oxide and olefiant gases, or of carburetted hydrogen and olefiant gases; that, on the other hand, if the mixture consists of any other two or three inflammable gases, the above-mentioned absorption by detonation, and that by potash, are obtained in such relations that it remains undetermined whether the gaseous mixture consists of this or that combination. In order, in these last cases, to come to a decision, it must be ascertained whether or not olefiant gas is present. By means

* When I performed these experiments I was not aware of all the circumstances mentioned above, by attention to which the most accurate results are obtained. Hence, in the results given, there is not the same near agreement which was afforded by my later experiments.

† Contributions to the analysis of gaseous mixtures of hydrogen, carbonic oxide, carburetted hydrogen, and olefiant gases, in the *Journal für Chemie und Physik*, t. xxxvii. p. 133.

of a comparison of the volume of the gaseous mixture employed for analysis, with the volumes which disappear by detonation, and by the use of potash, I have deduced equations for the six possible binary combinations of hydrogen, carbonic oxide, carburetted hydrogen, and olefiant gases, in order, with their assistance, to determine what combinations can occur in a given case. In like manner, equations were deduced for the four possible ternary combinations of these inflammable gases, and for this purpose three were deduced for each single combination, by which means it can likewise be determined what combinations can occur in a given case.

The experiments in § 3 have proved that in pit-gas olefiant gas is present. According to the equations of the binary combinations which contain olefiant gas, only that one consisting of carburetted hydrogen and olefiant gas can occur. As in this combination the volume of the gas which has disappeared by detonation amounts to double the volume of the gaseous mixture employed, the latter must amount to $\frac{1.9535}{2} = 0.97675$. Hence, the residue of 0.02325 vol. must have been a foreign non-inflammable gas. According to § 1 no oxygen can be contained in this residue. If we consider, however, that the pit-gas employed in the analysis came in contact with water in various ways during the washing with potash and the filling of the bottle, it may well be assumed that the pit-gas has acquired some atmospheric air by exchange. As, however, the pit-gas from *Gerhard's Stollen*, as we shall soon see, contains a large quantity of nitrogen, it is probable that the residue consisted chiefly of that gas. This nitrogen was thus, therefore, an essential component part of the pit-gas. The small quantity of the residue did not, however, admit of farther analytical investigations. If now, we take 0.97675 vol. as the quantity of the inflammable gas in the analyzed pit-gas, we find that it consists of

Carburetted Hydrogen Gas, ...	0.9136 vol.
Olefiant Gas, ...	0.0632 ...
Foreign Gas, ...	0.0232 ..
	1.0000

Upon the supposition that the pit-gas is a ternary combination, the equations coincide only with the combination of hydrogen, carburetted hydrogen, and olefiant gas. They take for granted, however, that double the portion of the gas employed amounts to more than the volume of the absorption. We obtain the following combination :—

Hydrogen, ...	0.0930 vol.
Carburetted Hydrogen, ...	0.7741 ...
Olefiant Gas, ...	0.1329 ...
	1.0000

This combination, however, cannot exist, as it supposes a much larger quantity of olefiant gas than is afforded by direct experiment with chlorine gas (§ 3).

According to the results of the analysis in the detonation-tube, the first combination alone can be supposed possible. In regard to the olefiant gas, whose quantity, as ascertained by indirect analysis, was found to be somewhat greater than when determined in the direct way, I would refer to what I shall have to say in § 11.

It still remains to inquire whether the pit-gas under consideration could consist of the four inflammable gases. The following general consideration proves, that, from the results of the analysis of such a gaseous mixture in the detonation-tube, the individual gases can be quantitatively determined, when its specific gravity is known.

Let

w be the volume of the hydrogen,	. . . . .	(α)
x	carbonic oxide gas . . .	(β)
y	carburetted hydrogen gas, .	(γ)
z	olefiant gas, ...	(δ)

Let a denote the absorption by detonation; b the carbonic acid produced by detonation; c the specific gravity of the gaseous mixture; α, β, γ, δ, the specific gravities of the gases standing opposite them; and m the quantity of the inflammable gases burnt in the detonation-tube; and we obtain the four following equations:

$$w + x + y + z = m$$

$$\frac{3}{2}w + \frac{1}{2}x + 2y + 2z = a$$

$$x + y + 2z = b$$

$$\alpha w + \beta x + \gamma y + \delta z = cm$$

from which the four unknown quantities may be determined by elimination. We thus have:

$$w = \frac{(4\beta + 2\gamma - 3\delta)\,m + (3\delta - 3\gamma)\,b - 3\,c\,m + (2\gamma - 2\beta)\,a}{\beta - 3\alpha + 5\gamma - 3\delta}$$

$$x = \frac{(4\alpha - 6\gamma + 3\delta)\,m + (\delta - \gamma)\,b - c\,m + (4\gamma - 2\alpha - 2\delta)\,a}{3\alpha - \beta - 5\gamma + 3\delta}$$

$$y = \frac{(2\alpha + 6\beta - 3\delta)\,m + (\beta - 3\alpha + 2\delta)\,b - 5\,c\,m + (2\alpha - 4\beta + 2\delta)\,a}{3\alpha - \beta - 5\gamma + 3\delta}$$

$$z = \frac{(3\gamma - 3\alpha - 3\beta)\,m + (3\alpha - \beta - 2\gamma)\,b + 3\,c\,m + (2\beta - 2\gamma)\,a}{3\alpha - \beta - 5\gamma + 3\delta}$$

If we now substitute for the specific gravities of the four inflammable gases, the values determined with so much accuracy by Dulong and Berzelius, we have the following numerical co-efficients in the preceding equations:

$$w = \frac{2.0674\,m + 1.2645\,b - 3\,c\,m - 0.8276\,a}{0.6196}$$

$$x = \frac{0.137\,m - 0.4215\,b + c\,m - 0.1372\,a}{0.6196}$$

$$y = \frac{-3.0326\,m - 2.7271\,b + 5\,c\,m + 1.7924\,a}{0.6196}$$

$$z = \frac{1.4478\,m + 1.8841 - 3\,c\,m - 0.8276\,a}{0.6196}$$

If we compare the two equations for m and a with one another, it is easy to see that a combination of the four inflammable gases is only possible when $2\,m > a$; for only in that case is $\frac{1}{2}w + \frac{3}{2}x > 0$. If, on the other hand, should $2\,m \overline{\gtrless}\, a$, there will be $\frac{1}{2}w + \frac{3}{2}x \overline{\gtrless}\, 0$, and this combination is then not possible.

If we apply this to the results of our analysis in the detonation-tube, we have $2.1 > 1.9535$, and the combination would consequently be possible. But it is here that the detonated gas consisted entirely of inflammable gases, or at least that the quantity of the latter was between 0.97675 and 1. As in all such cases only small differences are under consideration, let us make $m = 1$, and we have $a = 1.9535$, $b = 1.0399$; and c has been found $= 0.5769$. If we employ these values in the above equations, we obtain:

Hydrogen,	0.0564
Carbonic oxide gas,	0.0122
Carburetted hydrogen gas,	0.8351
Olefiant gas,	0.0963
	1.0000

We see, however, that this combination cannot exist, because it supposes a much larger quantity of olefiant gas than has been found by direct determination by means of chlorine. There therefore remains no other possible combination but the one found above of carburetted hydrogen and olefiant gases, and in that case there must likewise be present a foreign non-inflammable gas,—probably nitrogen.

§ 10. *Analysis of Pit-Gas by Oxide of Copper.*

The pit-gas freed from carbonic acid, was secured in a gasometer by water. It was conducted through a tube 18 inches long, filled with chloride of calcium, and then through a glass tube 16 inches long, which was filled with oxide of copper, and placed in a furnace. A chloride-of-calcium tube took up the water, and Liebig's apparatus absorbed the carbonic acid. The latter was in combination with a suction-apparatus (Saugapparat), which was so regulated that from 60 to 65 drops of water flowed off in the minute. Notwithstanding this slow passage of the gas, a portion of it nevertheless escaped combustion. That, however, which was decomposed in the red-hot tube was completely burned; for when, after an experiment, I closed the combustion-tube *(Verbrennungs Röhre)*, placed the other in lime-water, and strongly heated it, there was scarcely a trace of muddiness perceptible. When the flowing of the gas was interrupted but for a few seconds, the water formed in the combustion-tube moved backwards. It hence appears that the analysis of the pit-gas, by means of oxide of copper, is attended with some difficulties which affect the accuracy of the result. The following is the result of one of the most successful experiments. The carbonic acid amounted to 12.05 grains, the water 10 grains. If we assume 0.76435 as the weight of an atom of carbon, we have as the composition of the pit-gas,

Carbon,	3.322	or	74.99
Hydrogen,	1.111	or	25.01
			100.00

This agrees very nearly with the composition of carburetted hydrogen gas, but differs somewhat from the results of the analysis in the detonation-tube, according to which, owing to the mixed olefiant gas, the carbon ought to have turned out somewhat greater in amount. Probably this proceeds from the circumstance, that the dry and heated gas, which escaped unconsumed, vaporised some of the water of the solution of potash, so that the carbonic acid was estimated at too small a quantity. If the analysis were to be repeated, it would be necessary to connect a chloride-of-calcium tube with the absorbing apparatus. I satisfied myself, however, with the approximative result, as it was only intended as a check on the analysis of the pit-gas in the detonation-tube.

§ 11. *Determination of the Specific Gravity of the Pit-Gas.*

The gas was conducted into the exhausted balloon from a gasometer through a tube, which was filled with chloride of calcium, and was four feet long. The balloon was again exhausted, and new gas was in like manner very slowly introduced. In order to convince myself that the gas was perfectly dry, I put in the balloon, immediately before the first pumping out, a small tube filled with chloride of calcium. It presented, however, no increase of weight upon taking it out after the weighing of the balloon. The cock of the balloon closed it quite securely; for even after twenty-four hours the exhausted balloon presented no increase of weight. In communication with the balloon on the air-pump there stood a receiver, under which there was a thermometer and a small barometer, in order to admit of the determination of the tension of the gas, which still remained behind. In order, finally, to enable a correction to be made for the unequal aërostatic influence during the different weighings, the external volume of the balloon with its cock was determined by weighing it under pure water. The loss of weight of the balloon, filled with water, amounted in the water to 87672.5 grains at 51° F. At 32° F.

this loss of weight would therefore have been 87684.4 grains. If, now, we suppose the density of the dry atmospheric air at 32° F., and 28 inches barometer, equal to 0.00129967 the density of water, the weight of the atmospheric air displaced by the balloon, the height of the barometer and thermometer being the same, would be 113.961 grains. If, in the weighing of the exhausted balloon, we indicate the height of the barometer by b, the temperature of the air by t, and the tension of the watery vapour by e, while β, τ, and ε, denote the same quantities in the weight of the balloon filled with gas, then the following formula expresses the quantity which must be added to the ascertained weight of the gas, in order to correct the altered aërostatic influence as existing in the case of the exhausted balloon, and of the balloon filled with gas.

$$113.961\left(\frac{\beta - \frac{3}{8}\varepsilon}{28\,(1+0.00375\,\tau)} - \frac{b - \frac{3}{8}e}{28\,(1+0.00375\,t)}\right).$$

This corrected weight was then reduced in the usual way to 32° F., and 28 inches B., and at the same time the expansion of the balloon by heat was taken into account.

The following are the results of my experiments.

I. Weight of the atmospheric air in the balloon 97.4 grains. After the correction of the aërostatic influence 97.18 grains.

At the time of weighing the empty balloon, the height of the barometer 27.58 inches,*

Temperature, 53° F.

At the time of weighing the full balloon, the height of the barometer was 27.573

Barometer under the receiver, . . . 0.333

Consequently the tension of the weighed air, 27.24

Temperature 54°.5 F.

The weight of the atmospheric air, therefore, at 32° F., and at 28 inches = 104.539 gr.

II. Weight of the pit-gas in the balloon 55.8 grains. After the correction of the aërostatic influence 55.09 grains.

At the time of weighing the empty balloon, the height of the barometer was 27.93 inches.

Temperature, 54°.3 F.

* All the heights of the barometer are reduced to equal temperatures.

At the time of weighing the full balloon, the barometer stood at . . . . 27.907 In.

From this, however, is to be deducted the height of the inner level of the water in the receiver above the outer, since, after the passage of the gas from the receiver to the exhausted balloon, the water rose. This column of water, reduced to a column of mercury, gives .	0.62
	27.287 In.
Barometer under the receiver, ...	0.375
Therefore tension of the weighed air, .	26.912

Temperature, 57°.2 F.

The weight of the pit-gas at 32° F., and 28 inches B. is therefore = 60.304.

In the correction of the aerostatic influence, ε and e have not been taken into consideration, because, during the two experiments, the quantity of atmospherical vapour was not perceptibly altered. The condensation always ensued at 26°.5 F.

The specific gravity of the pit gas is therefore $\frac{60.304}{104.539} =$ 0.576856.

A second experiment gave 0.571667.

Inasmuch as the balloon was twice filled with the pit-gas, the quantity of the atmospheric air still left behind must have been so small as to be indeterminable. As, however, every gas is rendered impure by atmospheric air when the water by which it is retained stands in free communication with the atmosphere, the pit-gas, previous to its entrance into the balloon, must have been rendered more or less impure by atmospheric air. In order to be able to estimate and deduct this air, a little gas was expelled from the balloon by gentle heat.

The determination of the small quantity of oxygen in this gas was attended with difficulty. I tried in vain the employment of a solution of sulphuret of potassium. Two experiments with the gas from the balloon afforded a mean of absorption of 0.05765 vol., while, on the other hand, experiments with pure pit-gas afforded an absorption of 0.0576 *v.* The sulphuret of potassium had therefore absorbed some of the pit-gas itself. (Compare § 1 in vol. xxix, p. 316). Nitric oxide gas, however insufficient it may be for eudiometrical purposes, seemed available in the present case, where the determination of a small quantity of oxygen gas was alone concerned. This gas added to the pit-gas from the balloon, afforded, in three experiments, an absorption of 0.03, 0.03, and 0.034 vol., while the absorption in pure pit gas amounted to 0.004 vol., being the mean of three experiments. Consequently, the oxygen in the gas from the balloon amounted to about 0.009 vol.

How much nitrogen was indicated by this quantity, it is difficult to determine, because, as is well known, water exposed to the atmospheric air absorbs oxygen in a larger proportion than that contained in atmospheric air. If we should assume that the air which has passed from the water to the pit-gas was constituted like the atmospheric air, then we should have 0.0433 vol. of atmospheric air; and, according to this assumption, the specific gravity of the pure pit-gas, according to the second experiment, would be reduced to 0.552281. There is, however, no question that the quantity of oxygen and nitrogen is made much too great in this way.

If we calculate the specific gravity of the pit-gas according to the results of the analysis in the detonation-tube (§ ix.), and reckon the specific gravity of carburetted hydrogen gas = 0.5589, that of olefiant gas = 0.9804, and that of nitrogen = 0976, then we obtain 0.5952. But if, according to the analysis with chlorine, the pit-gas be composed of :—

Carburetted hydrogen, . . .	0.9388
Olefiant gas, ...	0.0380
Nitrogen, ...	0.0232
	1.0000

then the specific gravity would be 0.5846.

More confidence is plainly to be put in the analysis by the detonation-tube than in that by means of chlorine, although the specific gravity, calculated according to the latter, corresponds better with that found by direct experiment, than does the specific gravity calculated from the former.

The specific gravity of marsh-gas, according to Henry, is 0.582 to 0.586. After subtracting $\frac{1}{13}$ nitrogen, we obtain 0.556, which corresponds pretty accurately with the specific gravity of carburetted hydrogen, assumed above, as calculated from the experiments of Berzelius and Dulong. As the specific gravity of the olefiant gas was calculated from the same experiments, we must put faith in the previous calculation. On the other hand, I can also answer for the accuracy of my determinations of the specific gravity of the pit-gas. The difference between the specific gravities calculated according to the analysis, and that found by direct experiment (which of course becomes still a little greater when we take into consideration the small admixture of atmospheric air, which was mingled with the pit-gas), therefore still remains somewhat puzzling.

As, according to § viii., the sulphuric acid took up no pit-gas, there can be no vapour contained in it, which, as in the detonation of the oil-gas used for gas-light, could cause the formation of a greater quantity of carbonic acid gas than belongs to the carburetted hydrogen and olefiant gas. Can we suppose that the pit-gas contains a very small quantity of a carburetted hydrogen compound, which is not absorbed by sulphuric acid, and likewise not by chlorine in the dark? It is possible that the phenomena mentioned in § vi. (vol. xxix. p. 322), the formation of a peculiar oil-like combination during the passage of the pit-gas through a red-hot porcelain tube, might arise from such a vapoury hydro-carbon, and not from the carburetted hydrogen or olefiant gas. The disagreeable smell, likewise, which pit-gas, kept for a long time in well-closed bottles, acquires, and which resembles that of sulphuretted hydrogen, may perhaps arise from the decomposition of such a substance.

Chemical Analysis of the Pit-Gas of Gerhard's Stollen.

This investigation was conducted in the same manner as the preceding, and therefore I shall merely communicate the results.

Nitric oxide gas indicated no determinable quantity of oxygen.

Caustic potash indicated 0.039 vol. of carbonic acid gas.

Chlorine-gas afforded such small absorptions, that, according to these experiments, the existence of olefiant gas remains somewhat doubtful. Should it be present, it can, at all events, according to the determination by means of chlorine, not exceed 0.0025 vol. in amount.

The same experiments as in § v. (vol. xxix. p. 320), for the discovery of carbonic oxide gas, were also performed with this gas. The mercury was boiled in the tube, and the potassium was at first only heated to melting. The gas increased about 0.146 of its volume. Afterwards the potassium was again heated, and for so long as an increase of volume could be remarked, during which the tube acquired a dark red heat. The increase altogether amounted to 0.184 of its original volume.

Since these experiments, as well as those in § v., indicated no carbonic oxide gas, but, on the contrary, always presented an increase of volume, it seemed to me interesting to ascertain the action of potassium with an inflammable gaseous mixture, which actually contained carbonic oxide gas. In order to be quite certain that the carbonic oxide gas was free from carburetted hydrogen gas, I obtained it from oxalic acid by means of sulphuric acid, and carefully separated the carbonic acid gas. As the pit-gas from *Wellesweiler Stollen* proved to be a pretty pure carburetted hydrogen gas, I conducted 4 vol of this gas, and 1 vol. carbonic oxide gas into a tube filled with boiled mercury, and dried it by means of chloride of calcium. I continued the heating of the potassium so long as an alteration of the volume of gas could be perceived. A diminution of it took place, but it amounted only to 0.075. The potassium therefore only indicated 0.075 vol. of the 0.2 vol. which had been added.

As accordingly it was to be supposed, that potassium only indicated carbonic oxide gas in an inflammable gaseous mixture, when it was present in considerable quantity, I repeated the preceding experiment with a gaseous mixture which contained only $\frac{1}{6}$th of its volume of carbonic oxide. When the potassium was heated to the melting point, a diminution of volume of 0.02 presented itself, after the tube had been brought back to its original temperature. When, on the contrary, the potassium was heated anew and uninterruptedly until the volume was no longer altered, an increase of 0.098 presented itself after cooling.

From these experiments, it results that carbonic oxide gas, mixed with carburetted hydrogen gas, cannot be determined quantitatively by means of potassium ; that it is only indicated by a diminution of the volume of gas when it is present in considerable quantity ; and that that diminution amounts to so much the less, the more strongly and the more uninterruptedly the potassium is heated ; nay, that in this case an increase of volume may take place even when the carbonic oxide gas amounts to 14 per cent.

What, then, can be the cause of this increase ? Moisture, which in the first experiment (§ v.) seems to have had influence, was entirely counteracted in the subsequent experiments by boiling the mercury, and by chloride of calcium. There can, therefore, only have taken place a partial decomposition of the carburetted hydrogen gas *by means of the heated potassium ;* for if we heat a mixture of pit-gas from the *Wellesweiler Stollen* and carbonic oxide gas to the same point, without the presence of potassium, no increase of volume presents itself. It appears that here the potassium acts in the same manner as do metals on ammoniacal gas when heated in it. In the experiments in which potassium was heated in one of the two pit-gases without the addition of carbonic oxide gas, there was always presented a blackening of the tube in the neighbourhood of the metal.

As, therefore, potassium is not adapted for the separation of carbonic oxide gas from an inflammable gaseous mixture, there remain no other means for the analysis of any combination of the four inflammable gases, hydrogen, carbonic oxide, carburetted hydrogen, and olefiant gas, except chlorine, detonation with oxygen, and the determination of the specific gravity.

I have instituted experiments on the pit-gas of *Gerhard's Stollen* at a red heat, similar to those made with the previous gas.

The dried gas was conducted 10 times in one experiment, and 8 times in another, through the red-hot porcelain tube ; but after the fourth transmission the volume was no longer increased. This increase was somewhat smaller in the second experiment than in the first, which was probably caused by the carbon, which had been deposited in the first experiment, not having been removed.

In a mean of three experiments nearly corresponding with one another, 1 vol. of decomposed pit-gas of the first experiment had afforded, by detonation with 3 vol. of oxygen, an absorption of 1.367 vol., and potash had absorbed 0.528 vol. of carbonic acid gas. Assuming that the carbonic acid gas was produced from carburetted hydrogen gas alone, the gas experimented on consisted of

Carburetted hydrogen gas, . . .	0.528 vol.
Hydrogen gas, ...	0.207 ...
Foreign gas, ...	0.265 ...
	1.000

The pit-gas of the second experiment, when 1 vol. was detonated with 3 vol. oxygen, afforded an absorption of 1.289 vol., and the carbonic acid gas amounted to 0.5 vol. Making the same assumption as before, the gas consisted of

Carburetted hydrogen gas, . . .	0.500
Hydrogen gas, ...	0.172
Foreign gas, ...	0.328
	1.000

In the first experiment, therefore, there was decomposed only 0.10, and in the second only 0.086 vol. of the pit-gas in the red-hot porcelain tube. This difference agrees also with the smaller increase of volume in the second experiment. As, however, the atmospheric air could not be entirely removed; and as, likewise, during the long continuance of the experiment, the gas would be rendered impure by this air from the confining water, there can be no doubt that a portion of the hydrogen is burned in the red-hot tube at the expense of the oxygen of the atmospheric air. The true amount of gas decomposed must therefore have been somewhat greater. This also agrees with the fact, that in the decomposed gas about twice as much foreign gas was found as in the undecomposed gas, according to the analysis which follows below. I omitted examining more closely the foreign gas; but doubtless it consisted, at least chiefly, of nitrogen. After subtracting the foreign gas, the increase of volume amounted, accordingly, in the first experiment to 0.16, and in the second to 0.15.

This increase amounts, therefore, to somewhat more than that of the pit-gas of *Wellesweiler Stollen;* but the difference is not great. When we consider that the results of these experiments depend on circumstances which we have not under our control, this difference will not appear surprising. Indeed, it is very probable that the increase of volume which the carburetted hydrogen undergoes when exposed to a red-heat is a constant quantity, and that it does not exceed 0.2. What is true of the carburetted hydrogen gas, is probably true as regards the olefiant gas. Hence the latter gas, which, according to the analysis, is contained in both the pit-gases, was not taken into consideration.

The carbon which was deposited in the porcelain tube was quite of the same nature as in the Wellesweiler pit-gas, and the same empyreumatic smell was perceptible.

The analysis, by the detonation-tube, of the pit-gas from *Gerhard's Stollen*, was conducted in the same manner as in § ix. After it had been purified, by being washed with solution of potash, it was mixed with three times its volume of oxygen, which was likewise washed in the same way.

The following are the results of the experiments, after subtracting the watery vapour, and after reduction to 32° F. and 28 in.

	Exper. 1.	Exper. 2.	Mean.
Gaseous mixture, .	4	4	4
Absorption after detonation,	1.6972	1.7052	1.7012
Absorption by potash, .	0.8696	0.8712	0.8704

We might already reckon on the presence here of a considerable quantity of a foreign gas, from the comparatively small absorption by detonation and by potash. Therefore I agitated the residue, after the absorption of the carbonic acid, for twenty minutes, with sulphuret of potassium, and in the same way I determined also the amount of nitrogen in the oxygen employed for detonation.

The following is the result of Experiment II.

Residue after absorption of the carbonic acid, . .	1.4234 vol.
Residue after agitation with a solution of sulphuret of potassium, ...	0.2830 ...

Thence is to be subtracted the quantity of nitrogen contained in the three volumes of oxygen, . . .	0.1158 vol.
Consequently foreign non-inflammable gas in the pit-gas,	0.1672
Experiment I. gave, ...	0.1692
Mean, ...	0.1682
The quantity of inflammable gas in the gaseous mixture amounted therefore to, ...	0.8318
If, however, we assume that the inflammable gas is equal to the half of the absorption by detonation, we have,	0.8506

which harmonizes pretty nearly with the direct determination of the foreign gas. The difference cannot surprise us when we consider, that in agitating a gaseous mixture with a solution of sulphuret of potassium, some nitrogen is always absorbed along with the oxygen, and so much the more the less there is of it in proportion to the latter. Doubtless, therefore, the nitrogen is somewhat underrated in the oxygen employed for detonation. We ought, therefore, to place more confidence in the indirect determination of the foreign gas, according to which it amounts to 0.1494 vol., than in the direct mode.

According to the assumption formerly made, that the inflammable gases are equal to half the absorption by detonation, only the binary combination of carburetted hydrogen and olefiant gas can occur, and it results that the composition is the following:

Carburetted hydrogen, . . .	0.8308
Olefiant gas, ...	0.0198
Foreign gas, ...	0.1494
	1.0000

There is requisite for the perfect ignition of

0.8308 v. carburetted hydrogen gas,	1.6616 v.	oxygen.
0.0198 v. olefiant gas, . .	0.0594 v.	...
Absorbed by sulphuret of potassium,	1.1404 v.	...
	2.8614	
There was used for detonation, .	3	v. oxygen.
The nitrogen contained in it amounted to,	0.1158	
Pure oxygen, ...	2.8842	

The difference between this and the previous number is without doubt caused, at least partly, by the reason already mentioned.

Among the ternary combinations, only that could occur, consisting of hydrogen, carburetted hydrogen, and olefiant gas, if we had $2\,m < a$. If we for a moment assume this, and, contrary to all probability, fix any value for m which should be greater than 0.8506, it is easy to see, when we regard the equations for the said ternary combination,* that the amount of the olefiant gas must turn out greater than 0.0198. As now this quantity is much greater than that determined by direct analysis by means of chlorine, such an assumption is altogether deficient in probability.

Lastly, as to the possible case of the pit-gas of *Gerhard's Stollen* being a combination of all the four inflammable gases, likewise in that case must $2\,m < a$. Such an assumption would equally leave to be found a larger quantity of olefiant gas, and a smaller quantity of nitrogen, than have been ascertained by direct investigations; and hence this supposition is just as devoid of probability as that of a ternary combination.

The specific gravity of this pit-gas was determined in the same manner as that of the preceding one. After all corrections it is $= 0.651275$. If we calculate it according to the above analysis, in which we assume the foreign gas to be nitrogen, it is 0.629560. Although the specific gravity found by experiment must at all events turn out somewhat greater than that which is calculated, inasmuch as the gas in the balloon naturally cannot be entirely free from atmospheric air; yet the difference is somewhat too considerable to allow us to ascribe it merely to that circumstance. I have indeed examined the gas of the balloon, but unfortunately with a solution of sulphuret of potassium, and I perceived too late that this mode is not applicable for the separation of oxygen from inflammable gases.† I was obliged to renounce a repetition of the determination of the specific gravity, because I had not enough of the gas. If we

* These equations are

Hydrogen	$= 4\,m - 2\,a$
Carburetted hydrogen	$= 4\,a - b - 6\,m$
Olefiant gas	$= 3\,m + b - 2\,a.$

† This pit-gas was the first whose specific gravity I determined.

combine all the results of my investigations, it appears from the evidence before us, that this pit-gas is composed of carburetted hydrogen gas, olefiant gas, and nitrogen ; although the real proportions of these constituents may perhaps deviate somewhat from those determined. If a little nitrogen should be oxidised with the inflammable gases during the detonation, the quantity of that gas would be made too small, and that of the inflammable gases too great. A partial oxidation of the nitrogen by the detonation of an inflammable gas, where there is an excess of oxygen, is not only probable, but, from the experiments of De Saussure, is hardly to be doubted.

Physical and chemical examination of an inflammable gas which is evolved from an Artesian well in the Principality of Schaumburg.

Both the preceding inflammable pit-gases were evolved from the oldest coal-formation. The gases which were previously collected in the English pits, and analyzed by Henry, Thomson, Davy, &c. were also, so far as I know, from the oldest coal-formation. It therefore seemed a matter of interest to make use of an opportunity which presented itself to me of analyzing an inflammable gas from a much newer coal-formation, viz. one belonging to the lias series. This gas is evolved from an Artesian well, which was bored in a coal-pit near Lieckwege, in the principality of Schaumburg. It seems not to originate from a coal-bed ; for the bore was carried to a depth of 180 feet below the bed which is mined, without a second bed being traversed. In the neighbourhood of that bore, another bore has been even carried to a depth of 242 feet without meeting with a second bed. The gas is probably evolved from the slate-clay in which is the bore. This can easily be understood, as the slate-clay found there is very rich in carbonaceous matter.

Upon the bore of the Artesian well, there is a pumping pipe. The water flows aside from this pipe, and fourteen inches under its upper end. The space above the water is always filled with the inflammable gas. When lighted, it burns with a bluish flame, which is only coloured a little yellow at the end. If we cover the opening of the pump-pipe with a piece of wood, so that the gas is forced to pass through the seams, it burns with almost as yellow a flame as olefiant gas. It is only below that the flame is somewhat blue. When, in order to collect the gas, I introduced a funnel into the pumping-pipe, and luted it all round, so that it was necessary for the gas to issue through the narrow opening, it burnt with a flame which was two feet high, and which was only blue under from one to two inches, but otherwise was entirely of a light yellow tint. By preventing the exit of the water to the side, it was obliged to force itself through the narrow opening of the funnel, and there was then formed a fountain composed of water and gas more than two feet high. The water formed the middle of the fountain, and the gas surrounded it like a mantle. It presented a beautiful spectacle when the gas was lighted, and the flame played round the cylinder of water. This flame was also yellow, and only somewhat blue below. If the pumping-pipe was so closed that the gas was forced to issue rapidly along with watery vapour through a glass tube about two lines in diameter, the gas burned with a pure blue flame.

Hence we perceive that the colour of the flame of an inflammable pit-gas can vary very much according to the mode of its issuing forth, and even according to the greater or smaller quantity of atmospheric air with which it comes in contact. Hence, by means of this colour, we can hardly form a conclusion respecting the chemical constitution of the gas.

The gas is evolved with force.

The chemical examination afforded the following results :—

1. The gas troubled like water. In the various experiments, therefore, a gas was employed which had been washed with a solution of potash.

2. Chlorine gas, added to the gas in a flask of black, opaque glass, absorbed 0.0656 of its volume.

3. The gas contained no carbonic oxide gas.

4. It likewise contains no vapours which are absorbable by sulphuric acid.

5. According to four analyses in the eudiometer, which agree very nearly with one another, I have found that one volume of this gas, with four volumes of oxygen, affords, by the

electric spark, an absorption of 1.9041, and that there is formed 1.1131 volume of carbonic acid gas.

Hence, it follows that this gas is composed of—

Carburetted hydrogen gas, ...	0.7910
Olefiant gas, ...	0.1611
Foreign gas, ...	0.0479
	1.0000

It is very probable that this foreign gas is nothing else but nitrogen.

The previous analyses shew that the inflammable pit-gases are by no means of the same composition.

My analyses agree so far with those of the English chemists, that the gases examined by me contained carbonic acid gas and nitrogen in variable proportions, but no carbonic oxide gas. My analyses differed from them in this respect, that the presence of olefiant gas is proved as well by chlorine as by detonation with oxygen.

The question arises, Do the pit-gases analyzed by the English chemists contain no olefiant gas whatever, or did it escape their investigations? Without in the least wishing to throw doubt on the accuracy of these chemists, I must at the same time bear in remembrance that, since these investigations were carried on (a period of about twenty-five years), analytical chemistry, and more especially the analysis of gaseous substances, has made great progress. Sir H. Davy says that pit-gas does not act on chlorine in the cold; but he does not say that he remarked no change whatever of the volume. A volume of gas requires, according to him, *about* two volumes of oxygen for detonation by the electric spark, and *about* one volume of carbonic acid gas is produced; therefore, neither the quantity of oxygen employed, nor the quantity of carbonic acid gas produced, was accurately determined. It can, therefore, well have been more than double the volume of oxygen gas which was employed, and more than an equal volume of carbonic acid gas which was produced; therefore, the pit-gas analyzed by Davy may also have contained some olefiant gas.

I have already remarked above, in the analysis of the *Wel-*

lesweiler pit-gas, that the quantity of olefiant gas, when calculated from the quantity of carbonic acid formed, was greater than that afforded by direct determination by means of chlorine. The same was the case in the analysis of both the other pit-gases.

Professor Johnston*, when communicating an account of his examination of a fossil wax which occurs in cavities in the coal-pit of Urpeth near Newcastle-upon-Tyne, and which bears a great resemblance to the ozocerite, or *ozokerit*, found in large quantity in Moldavia, proposes the hypothesis that that substance, emitted in the form of vapour from the coal-strata, and carried along by the lighter gas (fire damp) given off at the surface, was partly condensed in the cavities and other cooler places with which it came in contact. Since there are so many hydro-carbons which, for the most part, are isomeric with olefiant gas (and more are daily discovered), this hypothesis has some probability. But it is very improbable, that in an inflammable gas, such inflammable vapours ever occur, as are produced by the decomposition of oil and other similar substances by means of heat. We have much rather reason to suppose, that these inflammable gases are formed in the same way as the inflammable gas of marshes.

I am of opinion that not only the inflammable vapours pointed out by Faraday, but also carbonic oxide gas, are peculiar to inflammable gas produced by heat. If these component parts are found in any inflammable gas, we may conclude as to its having been produced by heat; if we do not find them, we may assume that the inflammable gas has been formed in the same manner as the marsh-gas, that is, in the moist way.

Experiments to determine the limits of the inflammability of the inflammable pit-gases mixed with atmospheric air.

These experiments possess a practical rather than a theoretical interest, inasmuch as they shew in what relations the inflammable gas must be united with atmospheric air in order

* London and Edinburgh Philosophical Magazine, 3d series, vol. xii. p. 389.

to form an explosive mixture. The experiments are attended with some difficulties. It is necessary to pay particular attention that the two gases are as intimately united with each other as possible. This is best accomplished by first admitting the atmospheric air and then the inflammable gas into the tube (by which the latter, being lighter, is forced to ascend through the former), and then to move the tube, closed by the thumb, several times up and down.

The determination of the maximum of the atmospheric air is more difficult than that of the minimum at which the mixture ceases to detonate. If, when making experiments for the purpose of finding the latter, we are near the proportion in which the gaseous mixture ceases to be detonating, a lively detonation ensues with a certain proportion, by means of the electric spark, while, on the other hand, not the slightest detonation takes place when one part less atmospheric air is added. The more the atmospheric air exceeds that mixture in which the strongest explosion occurs, the more is the violence of that explosion diminished. Lastly, it comes to this, that a detonation, it is true, takes place, but the whole quantity of the inflammable gas is no longer consumed ; for the more the atmospheric air is increased, the less is the gas diminished by detonation.

For practical purposes, it would be sufficient to know the proportion of the inflammable gas and the atmospheric air in which the detonation takes place, without, however, being accompanied by destructive consequences. This, however, is not to be ascertained by means of the detonation-tube. There is nothing else left, then, but the determination of the proportion of the two gases in which a strong electric spark still causes a diminution of volume. This will be the proportion in which, though a detonation might occur in the pit, there would be no danger for the miners.

The following are the results of a large number of experiments, in which the power of detonating of the inflammable gas from the *Wellesweiler Stollen* (W), and from the Artesian well in the coal-pit in the principality of *Schaumburg* (S), is determined comparatively.

1. One part of inflammable gas from W. did not detonate

with sixteen parts of atmospheric air, even when two strong sparks were transmitted from the Leyden jar.

2. One part of inflammable gas from S. detonated, though feebly, with sixteen parts of atmospheric air.

3. When these experiments were repeated the following day with the gases still contained in the bottles, both detonated with sixteen times the quantity of atmospheric air.

4. One part of each of the two inflammable gases taken from a fresh bottle, detonated with sixteen parts of atmospheric air.

5. One part of inflammable gas from W. did not detonate with seventeen parts of atmospheric air.

6. One part of inflammable gas from S. still detonated, though very feebly, with seventeen parts of atmospheric air.

Perhaps the dissimilar result in the experiments of the two days was caused by the unequal temperature of the water used for keeping the gases; for on the second day it was 79° F., and on the first only 70°.

7. One part of each of the inflammable gases detonated very powerfully with seven parts of atmospheric air.

8. One part of each of the inflammable gases no longer detonated with six parts of atmospheric air.

Notwithstanding the dissimilar result of the experiments 1–6, it was nevertheless invariable that the inflammable gas from S. was somewhat more easily ignited than that from W., and this agrees very exactly with the results of the analysis.

The inflammable gas from *Gerhard's Stollen* ceases to form explosive mixtures, when one part is mixed with more than ten parts, and with less than seven parts, of atmospheric air. This also agrees with the chemical analysis, according to which 15 per cent. of nitrogen is contained in this inflammable gas.

Principal results of the previous investigations.

1. The chief component part of inflammable pit-gases is carburetted hydrogen gas, mixed with unequal quantities of olefiant gas, carbonic acid gas, and nitrogen. The similarity of these gases to marsh gas is much in favour of their being also a product of the decomposition of organic substances, probably of the coal itself.

2. Atmospheric air can form no part of these gases, at least of those evolutions which issue with force from fissures. The nitrogen, therefore, cannot arise from atmospheric air, but is doubtless a product of the decomposition of organic matter.

3. These gases contain no vapoury hydro-carbons which are condensable by sulphuric acid. Several phenomena, however, seem to indicate that a probably still unknown compound of this kind may be present in extremely small quantities.

4. Potassium is not adapted for the quantitative determination of the carbonic oxide gas in a given inflammable gaseous mixture.

5. According to the present state of our knowledge, we have no other mode of analyzing a mixture consisting of several inflammable gases, except by chlorine, the determination of the specific gravity, and detonation with oxygen, or burning with oxide of copper.

6. Carburetted hydrogen gas, contrary to what has hitherto been believed, cannot be perfectly decomposed either by red heat or by a great number of electric sparks.

7. By red heat peculiar hydro-carbons are formed from pit-gas, which colour sulphuric acid brown, and alcohol yellow. A compound seems also to be formed by electricity, which in smell resembles turpentine-oil.

8. The carbon which is separated by the decomposition in a red-hot porcelain-tube is distinguished by its very beautiful metallic lustre, and bears a great resemblance to graphite. Hence we might be induced to conclude that many of the instances of graphite occurring in fissures in crystalline rocks have been produced in the same way, viz. by carburetted hydrogen passing through these fissures while the masses were red hot.

9. Carburetted hydrogen gas detonates, with a very feeble report, with double its volume of chlorine gas, in the sun's light, by which carbon is deposited, and muriatic acid is formed. If the light of the sun be weakened by a cloud, the chlorine acts, it is true, on the carburetted hydrogen gas, but no carbon is deposited. But in this case the residue of the gas has the smell of turpentine.

10. The inflammable pit-gases exhibit a very different degree of inflammability when mixed with atmospheric air, according to the different proportions they contain of nitrogen, carbonic acid gas, and olefiant gas. The two first gases diminish, the last increases their inflammability. The larger the amount of atmospheric air with which they can be mixed without losing their detonating power, the more dangerous are the explosive mixtures formed by them in coal-mines.

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*Observations on Recent Earthquakes on the West Coast of South America.** By Mathie Hamilton, Esq., formerly Surgeon to the Potosi, Paz, and Peruvian Mining Company, &c. Communicated by the Author.

The west coast of South America, between the northern frontier of Peru and the south of Chili, includes an extent of territory 3000 miles from north to south.

Rain is very seldom seen on the coast of Peru, which is a desert, without water, vegetation, and inhabitants, except at a few spots, where a torrent from the Andes conveys the melted snow and rain, which falls in immense quantity on those sublime heights during the hotter months of the year, when almost every day after meridian a tempest of lightning and thunder, with rain, hail, or snow, is experienced, which causes the ravines to convey a flood to the arid plains below, which in some places reaches the sea, but in most parts is lost in the desert.

Along this vast extent of coast, at the distance of ten to thirty leagues from the Pacific Ocean, the mountains of the western Cordillera rise like an enormous rampart, to the altitude of 12,000 to 20,000 feet above the level of the sea,—many of their summits are shrouded in perpetual snow, glittering under the sunbeams, above the ordinary region of clouds and storms. Those mountains, in numerous parts, exhibit evidence of volcanic action, now dormant, but in some places they are still in operation, giving out smoke, and occasionally flame. From time immemorial, this portion of the earth has been subject to destructive volcanic eruptions and dreadful earthquakes. I will notice some of the more recent phenomena which have come under our immediate observation, and in a more special manner the terrible earthquake of 18th September 1833, which shook an extent of country equal to twice that of Great Britain, destroying the city of Tacna and other places in Peru. To understand more clearly what follows, I briefly notice a few geographical and

* Read before the British Association at Glasgow.

geological facts relative to Tacna and the surrounding country. Tacna, an Indian town of some antiquity, and now the capital of the province of the same name, is situated about 18 degrees south latitude, and stands in the desert, about midway between the mountains and the sea. It is NE., and forty miles from the sea-port town of Arica. The desert at this part of the coast is about fifty miles wide between the Andes and the sea, so that Tacna is twenty-five miles from the Pacific Ocean, and as has been ascertained by the barometer, is elevated 1400 feet above it; the desert there from the sea to the mountains is an inclined plain.

It may be said that there is no rain at Tacna, though at night, during winter, *i. e.* June, July, and August, there are some drizzling showers, or what we may call Scotch mist. The place is supplied with water for all purposes by a stream which issues from a ravine on the Andes; no part of this stream reaches the sea; the irrigation of the cultivated ground about Tacna takes so much of the water, the remainder is evaporated or absorbed by the desert.

The climate of Tacna is delightful, the mean annual temperature is about 64° Fahrenheit; the proximity of the snow-capped Andes on the one hand, and the ocean and trade-wind on the other, is the cause of the moderate heat.

The mountain of Tacora is due east from Tacna, its summit is a truncated cone, and appears to be of volcanic formation; it is covered with snow, and elevated 19,000 feet above the ocean.

The city of Tacna consists of three streets, which are about a mile long, and parallel to one another; they are crossed at right angles by six or seven other streets, thus dividing the town into a number of parallelograms,—at the lower extremity of the town, that nearest the sea, the cathedral, a large edifice, is, or rather was, to be seen. Prior to the earthquake of 1833, the best houses were built with adovies, *i. e.* very large flat bricks, merely dried in the sun; a house so constructed is well adapted to withstand an ordinary earthquake. About fifteen miles above the town, and half a mile from the bank of the river, there is a copious spring of hot sulphureous water; its temperature is 96° Fahrenheit. The country in the vicinity of Tacna seems to be of volcanic origin, trachyte abounding in the district,—at some parts of the desert between Tacna and Arica a hollow sound is produced by the tread of a horse or mule. At Santa Rosa de Chaca, a short distance south of Arica, grey copper-ore is found in abundance, and in the Quebrada de Achi pyritical copper-ore is obtained.

I very briefly notice some of the other places which were affected by the earthquakes of 1831–3, it may illustrate the power of the agent which operates in such an astounding manner as we have experienced in these convulsions. Across the Andes, 300 miles from Arica, is the city of La Paz, about 150 miles from it towards the south is the city of Oruro, and seventy miles east from it, and in about the same latitude, is the large city of Cochabamba; 200 miles from Oruro, in a SE. direction, is the famous city of Potosi, which stands on the internal or grand Cordillera of the Andes in Upper Peru; about 100 miles E. by N. from Potosi is the city of La Plata, or, giving its Indian name, Chuquisaca, which is the capital of Bolivia. All these towns are so far above the level of the sea as to seem incredible to some persons, who have not sufficiently considered the inequalities of the earth's surface.

The city of La Paz, though built at the bottom of a deep valley, yet is elevated 12,000 feet above the sea; Oruro is 12,400; Cochabamba is 10,000; Chuquisaca, which stands on the eastern extremity of the lateral chain of the main Cordillera of the Andes, is 9000 feet; and the parade ground in Potosi, where I have seen troops reviewed, is 13,500 feet above the level of the ocean.

Along the coast, 300 miles NW. from Arica, is the city of Arequipa; it stands on the western slope of the Andes of the coast, at the altitude of about 7600 feet above the sea-level. It has now 40,000 people, and is very well built; the best houses and public edifices are constructed with large blocks of pumice-stone, which is not unlike grey granite; many of the roofs are arched with the same sort of stone, as the arch sustains better than any other kind of roof shocks of earthquakes, of which the people there live in continual fear, for, since the foundation of Arequipa in 1540, it has been five times totally destroyed by earthquakes, the last of which happened in 1784. Behind the city, its celebrated volcano rises 17,000 feet above the ocean, its arid summit unlike the snow-capped peaks of the Andes in the immediate vicinity. This volcano is said to be the most perfect specimen of a natural cone of such magnitude yet discovered on the earth; it sides are covered with dark coloured sand or ashes, which towards the higher region is loose and deep.

On the road through the desert from Tacna to Arequipa, between Loquimbo and Moquegua, numerous marine shells are seen in the sand, at a distance from and above the sea. They are similar to those which at present are found on that coast within the tide limits, thus indicating that, at no very remote epoch, there has been in that locality either an elevation of the land, or a subsidence of the waters of the ocean. At Tacna, prior to the great earthquake of 1831, every shock was preceded by a subterranean noise; but this warning has been seldom if ever heard since 8th October 1831. Formerly people had time to run from their houses to some open place between the commencement of the noise and the shock. This subterranean noise was not unlike that of thunder, as it is heard when rolling among the valleys of the Andes, far below places where the traveller has to traverse those mountains.

Having heard it alleged, that, at Tacna, a person with his ear to the ground might occasionally hear subterranean sounds, as of heavy bodies falling, I tried the experiment, and frequently lay down on a mat; the result was, that subterranean sounds were at times heard, as if some ponderous body had fallen in a cavity, but which I suppose were only slight convulsions of the same nature as those which at times are so loud and destructive. The number of earthquakes felt at Tacna between 1826 and 1836 was very great, more than 100 smart shocks having been felt within a few weeks subsequent to the great earthquake of 1831. That convulsion extended over an immense space on sea and land, it having been felt both at a great distance on the ocean, and also at Potosi and Chuquisaca.*

When in Potosi in 1827, I made inquiry on the subject of earthquakes, and learned that, since the foundation of that city in 1545, they had been quite unknown there. The earthquake of 1831 reduced the sea-port town of Arica to a mass of rubbish, while Tacna, as on former similar occasions, escaped. Arica has been totally destroyed by earthquakes five or six times since it was first visited by the Spaniards 300 years ago: but, before 1833, the people there supposed that Tacna could not be affected in the same calamitous manner. A stranger, when first residing in that region, is surprised by frequent and novel concerts; the principal performers being dogs and donkeys, of which animals there are hundreds in and about Tacna. They howl most dismally whenever a slight earthquake occurs; and so sensitive are these creatures in this matter, that their announcement, and the tolling of the church-bell, is often the only intimation of the shock given to those persons not experienced in such accidents.

We will not advert to all the reported ominous appearances which, it was said, were seen or heard prior to the great convulsion; only that the people in a vessel from the United States declared that, on the evening before the earthquake of 1833, when they were above 100 miles from the coast of Peru, they saw a ball of fire in the air; that it was as large as the full moon, and that, after several minutes, it burst into pieces, and fell in the sea.

I will state what came under my observation. On Monday the 16th September, one hour after sunset, Tacna was shaken by a single explosion. There was not any subterranean noise, either before or after this single loud report, which, at the same instant, was accompanied by an upward movement of the ground. The night was very dark, the atmosphere being still and dense; at that time, there was not a barometer in Tacna. Next day was dull, and more lowering than usual; the evening was like the preceding, with a very thick atmosphere. The earthquake began, without any previous noise or warning, at six o'clock morning, 18th September 1833. I had the first intimation of it from the violent movement of a stout iron bedstead, and the shrieks of the people in the adjoining houses. The movement of the earth was at first vertical, or up and down, with oscillations, or lateral motion; then the movement was in undulations, like water when agitated into waves, but still accompanied with lateral, oscillatory motion. The movement of the earth was terrible, and the subterranean noise was horrific; and these, combined with the falling of houses all around, the continued lamentation of the

* *Vide* Edinburgh Philosophical Journal for 1834.

people, and the howling of animals, made a confusion of sounds indescribable. The convulsive heavings of the ground seemed to have attained their utmost force, when suddenly, as if the earth had been struggling to get rid of some mighty load, a more severe shock was felt, which was complicated in its movement, being upward and lateral. It finished the work of ruin in the town by throwing down the cathedral on the congregation, who were in it as usual at that early hour.

This terrible earthquake lasted about one minute, after which a tremulous motion of the ground was felt, and numerous gentle shocks, which continued many days.

The town presented a dismal spectacle: whole ranges of houses were in ruins, and many others were hanging over from the perpendicular, and *twisted* in a singular manner; these were speedily pulled down by a regiment of dragoons then at Tacna. Many scenes of misery were seen on that eventful morning: some houses were in ruins, burying their inmates, where, only a few hours before, joyous circles of happy beings were moving in the giddy throng, to the animating sounds of the guitar. All the congregation in the church perished: not one escaped, but none of the officiating priests were injured;—the worshippers, who were all females, were killed by the roof and walls falling on them while they were attempting to run from the church; but the priests stood still under an arch, and were saved. I now advert to some of the other effects of this earthquake. Rain began to fall at Tacna at the moment of convulsion. We had what may be called a wet season; for, during a period of six weeks after the great shock, more or less rain fell almost every day. In the first week of October I was at Arica, and experienced a deluge of rain such as had not been seen there before by persons who had lived there more than half a century. An hour after the earthquake, a well regulated thermometer in my house (which was not totally thrown down) shewed the temperature as low as I ever saw it at Tacna,—it was 58° Fahr. Four hours after the great shock, at 10 A.M., a very strong gust of wind and a whirlwind were seen carrying pillars of sand across the desert between Tacna and the sea, the movement being towards the ocean.

The earthquake affected different parts of Tacna with various degrees of destructive force. It has been noticed that the town is divided into a number of sections: in some of these almost every house was demolished, while in other portions much less damage was done. The lower divisions near the church were all but totally destroyed, but some of those situated higher sustained far less injury.

The village of Casa Blanca, between Tacna and the Andes, was overthrown in an extraordinary manner: the adovies, with which the houses there had been built, were scattered by the oscillatory movements of the earth, as if they had been laid down in detail by human agency.

A new unfinished wall, near the cathedral, was not overturned, but it was split longitudinally and perpendicularly, presenting a fissure an inch wide.

The river which supplies Tacna with water was not obstructed in its channel; but streams at other places had their courses changed, and one was noted as altogether lost subterraneously. Towards the south, along the coast, the earthquake extended many hundred miles to the desert of Atacama. At Luto, forty miles from Tacna, and near the base of the Andes, fissures were made in the ground, whence issued a dark-coloured fluid. In the province of Tarapaca,—where nitrate of soda is now obtained—villages were overthrown and gaps made in the ground. An Indian village, which stood in a ravine on the Andes, was buried out of sight with its inhabitants, the mountains closing over it. Towards the north, the nearest habitation to Tacna is the village of Samo, distant 30 miles, and the next is Loqumbo, 60 miles, both of which were destroyed. The next town is Moquegua, 120 miles; it has a population of 10,000, who suffered severely from damage to houses; and also great loss sustained by the destruction of wine and brandy, both of which are produced there in vast quantity.

This earthquake was felt at places farther north than Arequipa, which city, distant 90 miles from the sea, was much shaken; but little damage was done. In Upper Peru, the effects of this earthquake may be seen on the lofty peaks of the Andes. The church in the village of Tacora was thrown down by the convulsion: it is a small Indian village, 15,000 feet above the sea. The city of La Paz was shaken in a manner never before experienced, and also the other places in Upper Peru before mentioned.

After the calamity, when the atmosphere became clear, the Andes, as seen from Tacna, presented a novel spectacle. Those mountains, in many parts, appeared with a new surface: large portions had been thrown off, or had slid down into valleys or ravines below, leaving some of the more elevated peaks denuded of what had been their more prominent limbs; large masses of snow also were detached from the higher pinnacles. As to the size of the masses of rocks or earth which were detached from the Andes by this earthquake, I will only state, that Mr Scott, engineer, had been employed during some years prior to this earthquake by a joint-stock company in Tacna, whose object was to make a canal in that lofty region, and carry a river from it over the crest of the Andes, to fertilize a portion of the desert in the vicinity of Tacna. These operations could be carried on during a part of the year only; for the months between November and April are so tempestuous on those mountains as to prevent the work, but September is the best month in the year for cutting the canal. In these circumstances, Mr Scott was at Ochozumo, 14,000 feet above the sea, and saw the rare phenomena there presented to his view. He stated, that the motion of the ground in that elevated region was terrific, and that the noise, which was simultaneous with the convulsion, was peculiar, being as if an immense quantity of porcelain had been elevated in the air, and let fall so as to break it in pieces. It was also stated, that, within the range of his telescope, masses were thrown off from the mountains, one of which left a space whence the ruptured mass was detached as large as the Plaza of Tacna, which is as large as Saint Enoch Square, Glasgow. From what has been stated, it may be inferred that, within the last few years, an important subterranean change has happened beneath Tacna and surrounding country; also, that if the agent or moving power of the earthquake of 1833 acted throughout on the same level, it was in operation at an enormous depth below the earth's surface. We have the testimony of some who were on the ocean above 100 miles from the coast of Peru, where the sea is very deep; and besides our experience at Tacna, which is 1400 feet above the Pacific, we have evidence of the destructive effects of the convulsion in Upper Peru, at above three English miles perpendicular height above the surface of the ocean. The subterranean movement on 18th September 1833 appeared to be from north to south, with undulations. That of the year following was very different: it occurred at 8 o'clock P.M., on 21st September 1834. When the earthquake began, I was in an apartment constructed of wood, and remained within doors till the great shock had passed. The movement during this convulsion was altogether vertical, or up and down. Judging from the evolutions of the furniture, there were two upward movements every second; the shock lasted half a minute, and the subterranean noise, which was simultaneous with the motion, was like the discharge of a number of cannons, as heard a few hundred paces off. On the 20th of January 1834, the district of New Grenada was desolated by a terribly fatal earthquake. It happened at 7 o'clock A.M., when the cities of Popoyan, Pasto, and other towns, were totally overthrown, and many thousands of the population perished in the catastrophe. On that morning, so violent was the motion of the earth, that large masses of rocks were not only moved from their sites, but were thrown to a distance, as if discharged by means of gunpowder.

In the following year, a large portion of the south-west coast of America was the scene of a dreadfully destructive earthquake, which occurred on the 5th February 1835, when the south of Chili was overwhelmed by a convulsion which destroyed about *forty* towns and villages. The south of Chili is a fine verdant country, with a salubrious climate; but it is subject to dreadful earthquakes. In that of 1835, the seaport town of Conception was completely destroyed, and also Talcacuanha, the capital of the province. On this occasion, the sea retired several times to a great distance, and returned again in immense billows. It is believed that, by this earthquake, banks were thrown up at various parts along the coast of Chili, and that shoals were created where formerly was deep water; also, that a man-of-war, which was lost on the southern coast of Chili a few months after the earthquake, struck on a bank so created.

It is probable that there is a vast cavity below the surface of that portion of Peru which we have been considering, which cavity contains the chief agents of convulsion; also, that, at the moment of convulsion, the earth's surface is strongly charged with electricity.

Observations on the Glaciers of Spitzbergen, compared with those of Switzerland and Norway. By C. Martens, M.D., Member of the Northern Commission.*

Spitzbergen is a large island lying between 76° 30′, and 80° 30′ north latitude, and from 8° to 21° longitude east from Paris. Its western side is broken by a great number of deep bays, such as Horn Sound, Ice Sound, the bays of Croix, Hamburgh, and Madelaine. The shore is no where flat; in every direction conical mountains rise abruptly from the sea, attaining a medium height, which varies from 500 to 1200 metres. These mountains are separated by narrow valleys, the greater part of which open towards the sea. All of them, without exception, are occupied by glaciers which communicate with

* Bibliotheque Universelle de Geneve, Tome xxviii. No. 55, p. 139. We beg leave to refer our readers to the following memoirs relating to glaciers, arranged chronologically, which have appeared in the Edinburgh Philosophical Journal and in the Edinburgh New Philosophical Journal :—*Edinburgh Philosophical Journal*, vol. i. p. 121, Scoresby's Narrative of an Excursion upon the Island of Jan Mayen ; p. 187, Account of the Formation of the Lake of Mauvoisin, by the Descent of a Glacier, and of the Inundations of the Val de Bagnes in 1595 and 1818 ; vol. iii. p. 274, M. Venetz's Account of the Descent of the Glacier of the Weisshorn, on the 27th December 1819, and the Destruction of the Village of Randa. *Edinburgh New Philosophical Journal*, vol. ii. p. 86, Dr Latta's Observations on the Arctic Sea and Ice ; p. 107, Professor Esmark's Remarks, tending to explain the Geological History of the Earth : p. 255, M. Bohr's Account of a Visit to the Glaciers of the Justedal ; p. 382, Mr Scoresby's Remarks on Dr Latta's Observations on the Arctic Sea and Ice. Vol. iii. p. 91, Dr Latta's Observations on the Glaciers and Climate of Spitzbergen. Vol. x. p. 332, Professor Hugi's Observations on the Glaciers of the Alps. Vol. xi. p. 74, Continuation of same article. Vol. xxi. p. 210, M. Charpentier's Account of one of the most important results of the Investigations of M. Venetz regarding the present and earlier condition of the Glaciers of the Valais. Vol. xxiii. p. 346, Professor Bischof on Glaciers. Vol. xxiv. p. 158, Professor Bischof on Glaciers ; p. 364, Professor Agassiz on Glaciers, Moraines, and erratic blocks. Vol. xxv. p. 314, M. Elie de Beaumont on the Glaciers of Spitzbergen. Vol. xxvii. p. 383, Professor Agassiz's Observations on Glaciers. Vol. xxviii. p. 15, M. André de Luc on the Glaciers of the Alps. Vol. xxix. p. 181. Notice of the Tour of Professor Agassiz to the Swiss Glaciers in 1839 ; p. 274, M. Studer on some Phenomena of the Diluvian Epoch ; p. 280, M. Renoir on the Glaciers which anciently covered the southern side of the mountain-chain of the Vosges.—Editor.

those in the interior of the country. The accounts of voyagers who have visited these latitudes, such as those of Martens of Hamburg, Phipps,* Scoresby,† Parry,‡ Latta,§ and Keilhau,|| are quite at one in regard to this matter. During the two voyages of the Recherche, in 1838 and 1839, I had an opportunity of studying the glaciers of Bell Sound, Magdalena Bay, and the seven icehills which lie to the north of Prince Charles's Island. Their appearance strongly reminded me of those of Switzerland and Savoy, which I had visited in four successive journeys. It has appeared to me that the comparison of glaciers situated in countries so different, and in latitudes so remote, would not be void of interest to geologists, and would help to solve some of the questions which M. E. de Beaumont has put to us in his excellent instructions.¶

At Bell Sound, in the Bay Recherche, there were two glaciers ; one of smaller size, on the west, which we shall call the glacier *de la pointe aüx rénards ;* the other to the south, which we shall designate the principal glacier or *Great glacier of Bell Sound.* At Magdalena Bay, we remark on the right as we enter, that is to say on the south, two glaciers, that of the *Entrée*, and that of the *Pointe aux tombeaux*, then the glacier *du fond de la baie*, on which the greater number of observations have been made. There are still two others, one to the north, the other to the south, but they were of small size, and did not descend to the sea ; to these we shall seldom refer.

Extent. According to Scoresby,** the two largest glaciers of the island are that of the South Cape and another to the north of Horn Sound. The portion bordering on the sea coast is eleven miles long.†† Then come the seven glaciers, which, on an average, may be each two miles, since they extend, in a

* Plan of Fairhaven, with the island adjacent ; in a voyage towards the North Pole. 1773.

† An Account of the Arctic Regions, 1820, vol. i. p. 94.

‡ An Attempt to reach the North Pole. See the chart of the northern part of Spitzbergen.

§ On the Glaciers of Spitzbergen ; Jameson's Edinburgh New Philosophical Journal, vol. iii. p. 91.

|| Reise i Oest og Vest-Finmarken, 1831, p. 135.

¶ Voyage en Islande et au Groenland de la corvette *La Recherche*, t. i. p. 421.

** L. c. t. l. p. 102.

†† 1 mile = 1852 metres.

line north and south, from 79° 13′ to 79° 26′ of latitude:* at a distance, their form is that of so many isosceles triangles. Their length in general is not proportionate to their breadth. According to the measurement of the officers of the Recherche, the longest of them all, the principal glacier of Bell Sound was about ten miles in length by three and a third in breadth; that of Pointe aux rénards three miles long by one and a quarter in breadth. The glacier at the bottom of Magdalena Bay was 1840 metres in length by 1580 metres in breadth at the sea-shore; that of Pointe aux tombeaux 1800 metres broad; the glacier of the entry 1840 metres long by 900 broad; finally, the smallest of them all, which commanded the anchorage of the corvette, was 240 metres broad and 680 long.

The great glaciers of Switzerland or of Savoy, on the contrary, are much longer than broad. That of Bois, in the valley of Chamouni, extends to nearly five leagues without any interruption, while its breadth never exceeds one league.† According to the trigonometrical measurements of Hugi,‡ the great glacier of Aletsch is seven leagues long, with an average breadth of a league and a half; that of Ober-Aar four leagues by one; that of Unter-Aar five by one and a quarter. This difference between the glaciers of the two countries is easily explained. Although those of Spitzbergen descend to the sea, the mountains on which they rest are proportionally very low. In Switzerland, the mountains are higher and the valleys longer. Suppose that one or other of these circumstances should exist in Spitzbergen, we would have seas of ice the length of which would greatly exceed that of the most extensive glaciers of Switzerland and Savoy, for they would descend to the sea, while the lower extremities of the least elevated glaciers in the last-mentioned countries, those of Grindelwald,§ Bossons,|| Brenva,¶ and Aletsch,** have a medium height of 1230 metres above its level.

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* 1 mile = 1852 metres. Appendix, p. 74.

† De Saussure, Voyage dans les Alpes, § 522.

‡ Naturhistorische Alpenreisen. (See the map.)

§ Lower Glacier 1039 metres, upper Glacier 1299 metres. *(Hugi.)*

|| 1115 metres. Barometrical measurement of the author.

¶ 1347 metres. Escher de la Linth in Alph. de Candolle: *Hypsométrie des environs de Genève*, p. 31.

** 1349 metres. Hugi, l. c. Tab. iii.

The height of the inferior glaciers above the sea depends on two principal causes: the geographical position and elevation of the mountains from whence they proceed. This is proved by the following facts. In Norway, under 61° latitude, the glaciers of Justedal rise to a height of 1850 metres, and descend to 485 metres.* The inferior extremities of that of Lodal and Nygaard are at 577 metres and 340 metres above the sea.† In the same country, under latitude 67°, the glaciers of Sala and Almjalos descend from a crest of the Sulitelma, which rises to 1494 metres, and their lowest point is still from 778 and 974 metres above the North Sea.‡ We thus see that the difference of 6° in latitude is more than compensated by the greater elevation of the mountains of Justedal, compared with that of Sulitelma. Even under the 70th parallel, the glacier of Jœkulsfiord in Finmark does not descend to the sea. In Iceland, 64° latitude, under the double influence of one of the most rigorous climates in the same latitude and the most elevated mountains, the glaciers reach the margin of the ocean. But in no instance do they advance into it, for a free space is always left, which admits of a passage between the glacier and the waves.

Distinction between the lower glaciers and the Mers de Glace of Switzerland.—We cannot continue our comparison between the glaciers of Spitzbergen and Switzerland without making, at the outset, an important distinction. In Spitzbergen, or at least in the north of that island, the line of perpetual snow is at the margin of the sea; consequently, the glaciers cannot strictly be compared, except with that portion of the Swiss glaciers which rises above the limit of continual snow. These upper glaciers are very different from those which descend into the inhabited valleys: Saussure, Charpentier, and above all Hugi, have perfectly described and distinguished them.

* L. Von Buch: Ueber die Grenzen des ewigen Schnees in Norden. (Gilbert's Annalen der Physik, t. 41. p. 22. 1812.)

† Naumann: Einige Bemerkungen auf Ausfluegen in die Norwegischen Schneegefilde. (*Leonhard's Taschenbuch*, t. xvii., p. 167 and 186. 1823.)

‡ Wahlenberg: Bericht über Messungen und Beobachtungen zur Bestimmung der Höhe und Temperatur der Lappländischen Alpen unter dem 67 en. Breitegrad, übersetzt von Haussmann. 1812. § 17 et 29.

The Germans call them *Firne;* we call them *Glaciers Superieurs*, or *Mers de Glace.* Under the name of *Glaciers Inferieurs* (*Gletscher*), we comprehend the lower part of the glaciers, beginning at the inferior limit of the Mers de Glace. Finally, we apply the word glaciers, without addition, to the entire Mers de Glace (*Firne*) and the inferior glaciers (*Gletscher*), considered collectively. The glaciers of Spitzbergen being only Mers de Glace, as we shall afterwards see, we apply one or other of these denominations to them indifferently.

The lower limit of the Mers de Glace of Switzerland has been fixed by Hugi* at 2470 metres. It nearly coincides with the line of perpetual snow; but it is more constant, for its height is the same on the two declivities of the Alps, and it undergoes no variation in different years. Their upper limit is from 3200 to 3600 metres above the sea, in the group of the Oberland.

Declivity and perpendicular height.—The inclination of the glaciers of Spitzbergen is very trifling. In fact, the mountains on which they rest are not high, and they are, besides, so precipitous, that the glaciers cannot rise above the third of their entire height. From this it follows that the difference of level between the lowest and highest points of these fields of ice is not nearly so considerable as among the Alps. According to Scoresby,† the difference of level between the summit and bottom of the great glacier of Horn Sound is only 400 metres. I calculate that of the glacier at the bottom of Magdalena Bay at 350 metres, which reduces its inclination to about 10°. Latta‡ estimates that of the Seven Glaciers from 10° to 20°: the principal glacier of Bell Sound was nearly horizontal, owing to its great length.

The three glaciers of Beerenberg, in the island Jan Mayen (Lat. 70° 49′), are only 390 metres of vertical height above the sea. Scoresby assured himself of this by a trigonometrical measurement.§

In Switzerland and Savoy, on the contrary, the inclination

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* L. c. p. 334 to 337. † L. c. t. i. p. 102. ‡ L. c. p. 95.

§ Narrative of an Excursion upon the Island of Jan Mayen. (Edin. Phil. Journal, vol. i. p. 121. 1819.)

of the slopes often exceeds 30° or 40°, according to Saussure,* and the difference of level between the summit and the base of the glaciers varies from 1590 to 2000 metres. I shall content myself by adducing two examples, which will afford an idea of the extreme limits. When examining Mont Blanc from the height of Cramont, De Saussure † thought he could affirm that its summit was not covered with ice. He reverted to this opinion in his ascent of that mountain and of the Brevent ;‡ thus we may admit that the glaciers which surround Mont Blanc, and, in particular, the glacier of the Bossons, descend from its summit, which gives to the latter glacier a vertical height of 3795 metres. On the other hand, according to the observations of Hugi, the glacier of Loëtsch, in the Valais, is only 1605 metres in vertical height. By measuring that of glaciers which descend from mountains of small elevation, we should certainly find differences of level still less considerable.

If we seek to discover, as has been done by Hugi, that which exists between the summit and the base of the Mers de Glace (*Firne*), we find that it is about 940 metres in the Oberland. On the Col du Mont Cervin I have found it to be about 1054 metres, on the supposition that the Mer de Glace which covers the Col descends from the little Mont Cervin, for its lower limit is at 519 metres below the culminating point of the Col.§

Forms.—The surface of the Spitzbergen glaciers presents none of those sharp points or pyramidal elevations which form the beauty of the inferior glaciers of Switzerland. Their superficies is commonly uniform or slightly undulated, like that of the mers de glace in the Oberland and Savoy. It is probable, however, that some of them might be covered with sharp projections, both on account of the great inclination of their sides,‖ and the occurrence of several unusually warm summers. We know as an ascertained fact that these two

* L. c. § 524. † L. c. § 914. ‡ L. c. § 1981, *note*, and § 654.

§ Height of Little Mount Cervin, called Breithorn by Saussure, 3900 metres (§ 2247). Termination of the Mer de Glace on the north, above Zermatt, 2846 above the sea, according to the barometrical observations of the author.

‖ De Saussure, 1 c. § 632.

causes, either united or separately, contribute to the formation of needle-like points. Thus, the glacier of Talèfre, the sides of which are very steep, presents very beautiful projections of this nature, although it rises 2600 metres above the sea,* and is consequently above the lower limit of the Mers de Glace. Those of the Bossons, Bois, and Grindelwald, which unite the two conditions above mentioned, are likewise those in which the needle-like prominences are highest and most beautiful.

At Magdalena Bay, the surface of the glaciers was uniform, like all the others which had been seen by Scoresby, Latta, and Keilhau ; but at Bell-Sound, the glacier at the bottom of the bay presented a few pyramids at its lower extremity, the spot where the reverberation of the sun's rays from the neighbouring mountains must have tended to melt the ice. This peculiarity will surprise no one, when we reflect that this glacier was situated at the extremity of a deep and well sheltered bay, the climate of which is rather mild compared with that of such portions of the coast as are exposed to all the violence of the winds from the sea. During our residence at Bell Sound, from 25th July to 4th August 1838, the thermometer rose to $+ 8^{\circ}.2$, C., and never sunk below $+ 0^{\circ}.3$; the mean being $+ 3^{\circ}.8$. At Magdalena Bay, on the contrary, where we remained from the 1st to the 12th August 1839, it rose only once to $+ 5^{\circ}.7$, and often sunk to zero. The mean temperature was $+ 2^{\circ}.97$. If it be objected to this that these two summers may have been in every respect exceptions to the usual character of the seasons, I would reply that the vegetation is the best index of difference of climate in regard to two places not distant from each other, and situate, like Bell Sound and Magdalena Bay, on the shore of the same sea, and under the same meridian. Now, at Bell Sound, I found fifty-seven phanerogamous plants ; at Magdalena Bay twenty-four, and in this number only two which did not exist at Bell Sound ;† the difference of the two climates is therefore unquestionable.

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* Saussure, t. c. § 652.

† The following is the list of phanerogamous plants which we found at Bell Sound, those in italics existing likewise at Magdalena Bay :—*Ranun-*

It may be added that we find glaciers with pyramids under very high latitudes. In Norway, 67° Lat. the glaciers of Sulitelma are covered with them in every place where the declivity is steep.* In Iceland, under 64°, those of Svinafells Joekull,† and Hoelaar Joekull,‡ are remarkable for their beauty.

The lower glaciers of Switzerland are convex, that is to say, more elevated in their centre than at the sides; this disposition appears to me to be nearly invariable. It is owing to the circumstance of the upper surface of the glacier melting most on the lateral parts, which are more exposed to the reverberation of heat from all the neighbouring heights. Hugi§ properly observes that this convexity constitutes an essential character which distinguishes the glaciers from the Mers de Glace; the latter are almost always flat, as that of the Col St Théodule, and even concave, as that of Roththal, the margins of which were raised about 20 metres above the central parts.‖ In Spitzbergen the same thing occurs; the sur-

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culus sulphureus, Sol.; R. nivalis, L.; *R. pygmaeus,* Wahl.; *Papaver nudicaule,* L.; Draba algida, D.C.; D. alpina; *D. muricella,* Wahl.; D. arctica, Wahl. f.; *D. oblongata,* R. B.; D. lapponica, D. C.; D. martinsiana, J. Gay; Cochlearea officinalis, L.; *C. danica,* L.; *Cardamine bellidifolia,* L.; C. pratensis, L.; Platypetalum purpurascens, R. B.; Eutrema Edwardsii, R. B.; *Silène acaulis,* L.; Lychnis apetala, L.; Spergula saginoides, L.; *Stellaria Edwardsii,* R. B.; Arenaria rubella, Hook; A. Scandinavica, Spr.; A. ciliata, Wahl.; *Cerastium alpinum,* L.; Dryas octopetala, L.; Potentilla pulchella, R. B.; *P. emarginata,* Pursh; Saxifraga aizoides, L.; S. flagellaris, Sternb.; *S. cespitosa,* L.; S. hieraciifolia, Wild.; *S. stellaris,* β; *S. comosa,* Hartm.; *S. cernua,* L.; S. hirculus, L.; *S. oppositifolia,* L.; *S. rivularis,* L.; *S. nivalis,* L.; Chrysoplenium alternifolium, L.; Erigeron alpinum, β uniflorum, Less; Phytamocarpum taraxacoides, Vahl. f.; Empetrum nigrum, L.; Pedicularis hirsuta, L.; *Oxyria reniformis,* Hook.; *Polygonum viviparum,* L.; Salix reticulata, L.; *S. polaris,* Wahl.; Juncus biglumis, L.; Luzula arcuata, Wahl.; Carex helionastes, Ehrh.; *Phippsia algida,* R. B.; Alopecurus ovatus β muticus, Sommerf.; Festuca ovina, L.; F. rubra β arenaria, Wahl.; *Poa alpina,* L.; Dupontia Fischeri, R. B.; Trisetum subspicatum, Pal. Beauv.

At Magdalena Bay only:—Luzula hyperborea, R. B.; Poa laxa?

* Wahlenberg, l. c. § 13, and Plate II. and IV.

† Voyage en Icelande et au Groenland de la corvette *La Recherche,* planches 80 and 81.

‡ Ibid. pl. 84. § L. c. p. 177. ‖ L. c. p. 59.

face of a glacier is most frequently flat, or slightly concave.* The glaciers of Bell-Sound were obviously flat; those of Magdalena Bay presented three types. The glacier of Pointe aux Tombeaux was a little concave, that at the bottom of the bay evidently flat, particularly in its upper part. Finally, there were two small ones, which did not reach the edge of the sea, because the coast was steep, and the masses of ice fell and were broken, like those of the glacier of Gétroz, in the Valais, in proportion as they advanced and sloped down upon this vertical wall. One of these glaciers overlooked our vessel, and I was therefore able to examine it at leisure. It was only 240 metres broad, inclosed between two rocks of a dark colour, and had melted at the sides, which rendered it very convex. It was composed of layers placed one above another, which appeared to consist of indurated snow rather than true ice. Perhaps it is to the very form in question that we ought to attribute the imperfect conversion of the snow into ice. Rain water, and that proceeding from melted snow, running over this bulging surface as from a roof, could never penetrate this compact mass of snow, condensed by its own weight. When the declivities on which northern glaciers repose are very unequal, they mould themselves upon these inequalities like the glaciers of the Alps, as Scoresby has shewn to take place on the glaciers of the island Jan Mayen, which he represents as resembling an immense frozen cataract, the illusion being rendered complete by pointed rocks projecting in numerous places.†

The glaciers of Spitzbergen perhaps melt a little on the surface when the weather is mild and the air moist; but, notwithstanding this, it is to the melting of the snow accumulated on them that we must ascribe the origin of the streams of water to which they sometimes give rise. It is not a rare thing to see small cascades issuing from their upper parts. At Magdalena Bay one was observed, which flowed for several days from the sides of the glacier Pointe aux Tombeaux. That of Bell Sound was furrowed by small streams of water.

* Scoresby, l. c. t. l., p. 103. Keilhau, l. c. p. 135.

† Edin. Phil. Journal, vol. i, p. 121. 1819.

Phipps, in one of the views which ornament his work, has represented one of the glaciers of Smeerenberg, and a cascade issues from the lower third of its vertical height, and falls into the sea. Scoresby and Latta saw the seven glaciers traversed by small streams, some of which ran in the crevices, and could only be discovered by the sound of the water. The surface of the terminal glacier of Magdalena Bay was quite dry, no water being found even in its fissures.

Crevices.—In general these glaciers are covered with snow all the year. Keilhau * has verified this affirmation regarding those of the south, and the snow does not melt on those of the north. At Bell Sound, however, the great glacier at the time of our departure † had a great number of small pools scattered over its surface. This layer of snow conceals the crevices from the eye of the observer, and entirely covers them up, if they are not very wide. At Magdalena Bay, I went upon the great glacier at the bottom, in order to study them more closely. Like those of the glaciers of the Alps, they were all transverse, consequently parallel to the sea, and so much the wider the nearer they approached to it. At the surface of the glacier, they sometimes attained a width from five to ten metres ; their depth was from fifteen to twenty. Their two faces were always in contact at the bottom, which was strewed with enormous fragments of ice and indurated snow. Long stalactites hung from the walls of these crevices. Sometimes they resembled the tubes of a huge organ, at other times they were insulated, and reminded one of the pendant ornaments of a Gothic cathedral. I could never see, in the bottom of the fissure, the rock on which the glacier was resting. Saussure, in his ascent of Mont Blanc, was not more successful.‡ It is, at the same time, probable that these rents often traverse the glacier throughout its whole thickness in Spitzbergen as well as in Switzerland.§ The adventure of the Grindelwald innkeeper, who fell into a fissure and found his way to the edge of the glacier, by following the course of a rivulet, which had wrought out a passage for itself under

* L. c. p. 135. † 4th August 1838. ‡ L. c. § 1973.

§ See Hugi, l. c. p. 338 and 331.

the ice, is a fact well known to all who have travelled in the Oberland.* The formation of such crevices may be reasonably assigned to the alternate contractions and dilatations of those masses which, during the long winters of Spitzbergen, are, in all probability, exposed to a cold of — 40° C., while, in the summer, they are suddenly surrounded by an atmosphere whose temperature is always above zero. The surface is then heated more than the subjacent parts, the places covered with snow less than such as are free from it; hence, unequal dilatations followed by continuous melting throughout the mass. Hugi has often seen these crevices forming under his eyes. Respecting one of them, he gives the following details:—"I was one day walking over the glacier of the Unter-Aar, about three o'clock in the afternoon. The heat was very powerful; all of a sudden I heard a peculiar noise. I had scarcely run thirty or forty paces in the direction of the sound, when I felt the glacier tremble under my feet several times; at the same moment I saw that it was rending asunder. The fissure often advanced from ten to twenty feet in an instant: I was scarcely able to follow it. For a moment at a time it seemed about to stop, or rather its progression was very slow; but the entire mass seemed to tremble, and the rent continued to lengthen. Several times I ran on before it, that I might lie down on the glacier in the place where it must pass. Once I was violently shaken, and the ice split beneath me. In this manner I followed the rent for a quarter of an hour, as far as a central moraine, where it stopped. The breadth was an inch and a half; its inner surface, rough and unequal, exposed to view crystals of ice, partly broken, partly torn asunder; its depth did not exceed four or five feet. A few days after, I visited the glacier a second time: the rent was six inches wide, and its depth considerable. A new fissure, parallel with the first and six feet deep, had been formed in my absence."†

Many causes contribute to enlarge these crevices. In the first place, the weight of the ice beneath; then the water which

* Ebel; Manuel du Voyageur en Suisse, t. ii. p. 580.

† Hugi, l. c. p. 354.

collects in them in the form of little reservoirs or streamlets. By reason of its maximum of density, which is found to be between +4° and +4°.5, the water, which contact with the ice has brought to zero, reascends to the surface, to descend again after it has acquired, under the influence of the air, a temperature near to this maximum of density. Finally, the freezing of this water, and the enlargement of volume accompanying its change of condition, which causes it to act as a wedge, and thereby increase the fissure. I shall refer the natural philosophers, who may raise objections to this theory, to the profound discussion preserved by Gilbert in his Annalen.*

Nature of the Ice.—The ice of which the glaciers of Spitzbergen are composed resembles, in every respect, that of the superior glaciers (*Firne*) of the Alps. On the surface it is rough and unequal, never slippery. The crevices are often tinted with the most beautiful azure blue, which is likewise the case with the cavities in the floating pieces of ice proceeding from them. The latter sometimes appear striated. When examined closely, we perceive that this appearance is owing to a multitude of small bubbles of air of an elongated and pyriform shape. In some pieces these bubbles are rounded. Saussure† and Scoresby‡ have recorded observations analogous to these. This ice is not formed by the union of crystals or intimately united fragments, like that of the inferior glaciers of Switzerland. MM. Hugi§ and Kaemtz,‖ who have described them very accurately, both insist on this peculiarity, that the crystals diminish in size in proportion as we ascend a glacier. It need not, therefore, excite surprise, if they do not exist in the ice of Spitzbergen. At the same time, I have observed, both at Bell Sound and Magdalena Bay, crystalline formations of a very singular character. At Bell Sound, this was on the margin of a small rivulet; at Magdalena Bay, near a feeble streamlet, between the glacier Pointe aux Tombeaux and the sea. These two places were perfectly flat, and covered with a not very thick layer of ice and snow. Here and there certain places were to be seen, of several square metres in extent, in

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* Annalen der Physik, t. lxiv. p. 183. 1820. † L. c. § 525. ‡ L. c. t. I. p. 341.
§ L. c. p. 341. ‖ Lehrbuch der Meteorologie, t. ii. p. 160. 1832.

general somewhat convex, and which were entirely paved with prisms placed vertically by the sides of each other. Their length varied from fourteen to thirty centimeters, the greater part being from one to two decimeters ; their diameter was from one to two, rarely of three, centimeters. Their upper surfaces were perfectly level. Almost all resembled each other in shape, which was that of a prism with a square or hexagonal base ; but their faces and angles were unequal and *mal accusées.* Being simply in juxtaposition, although very close to each other, they fell of their own accord when an opening was made. These prisms did not rest on the ground, but were implanted in the mass of ice of which we have spoken. At Bell Sound my friend M. Bravais observed two stages of similar crystals placed one above the other, and separated by a plane of ice. The lower stage, the least regular, was composed of needle-like points in a state of great confusion.* The ice of these prisms was light, and filled with air-bubbles ; its colour often tinged with azure. These assemblages of ice-crystals are found in planes perfectly horizontal, where the water lodges and slowly imbibes the snow which covers the soil. Their formation may be explained (comparing it to that of crystallized salts allowed to remain for a length of time in solution) by the successive juxtaposition of new particles of ice every time the melted snow passes into a solid state, or by the effects of the moisture being withdrawn. The ice-crystals which enter into the composition of the lower parts of the Helvetian glaciers, often attain the thickness of a nut, with a length of fifty-four millimeters.† Kaemtz explains their origin in the same manner.‡

Moraines—Erratic blocks. The glaciers of Spitzbergen are overlooked by nearly vertical mountains, whose summits are composed of blocks of rock not intimately united with each other, because the water produced by the melting of the snow has separated them at the time of its congelation. Their sides are for this reason covered with an immense quantity of debris.

Between the lower extremity of the glacier and the moun-

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* Colonel Jackson has observed similar crystals on the Neva. See Transactions of the Royal Geographical Society, vol. v. p. 19. 1835.

† Hugi, l. c. p. 341. ‡ Vorlesungen über Meteorologie, p. 261. 1840.

tain, there often exists an interval, a small valley, the bottom of which is occupied by a small rivulet. The great glacier of Bell Sound was separated from each of the lateral chains which confined it by one of these valleys. On the north of the terminal glacier of Magdalena Bay, a rivulet ran between the moraine and the mountain. Those of the Entrée and Pointe aux Tombeaux supported themselves on both sides on the surrounding mountains, except at their lower extremity, which advanced a little beyond them.

The travellers who preceded us have not directed their attention to the blocks of stone which may be found on the surface of these glaciers. The following are the observations I made on this subject.

The principal glacier of Bell Sound was soiled with earth only at its sides. Those of Magdalena Bay were both covered with stones at their lower extremity. These stones occupied nearly the eighth part of the breadth of the terminal glacier; but there were none in the *middle*, either of this or of any other. Not cnly are blocks observed on their upper surface, but they are likewise found embedded in the ice. I have remarked a pretty considerable number of them over all the height of the two lateral walls of the two principal glaciers of Magdalena Bay, but *never* could I discern any on the face looking to the sea. I cannot believe that any such exist, for I have passed entire hours before the glaciers of the two bays which we visited in order to make experiments on submarine temperatures. I could thus see a great number of sections, comprehending the entire thickness of the glacier, as the latter by degrees broke down into the sea. In order to assure myself of the fact, I went in a boat to the glacier of Magdalena Bay, and entered into the little creeks formed by the breaking down of the ice, and nowhere could I see blocks nor even sand in the thickness of the ice; every where it was perfectly pure and transparent. Before passing on to the explanation of these facts, let us first examine comparatively how the erratic blocks appear on the glaciers of Switzerland.

All geologists agree in affirming, that no blocks are seen on the surface of the upper glaciers *(Firne)*. Doubtless a few insulated blocks may appear by chance, which have rolled down from the neighbouring heights ; but the surface is never entirely concealed under a layer of stones and sand, as takes place with the lower glaciers. The reason of this may be easily understood. When a block falls on one of the upper glaciers, it sinks in the snow ; as the latter never entirely melts, but is converted into ice and is covered with successive layers of new snow, it follows that the block is soon buried in ice. But the glacier having a continued progressive movement, is always descending towards the plain ; and while it advances over a slope more or less inclined, the block advances horizontally, and is soon placed at the surface of the glacier. Such, at least, is the explanation given by M. Charpentier.* Hence the existence of the central moraines *(Gufferlinien)* which are the union of the terminal moraines of several glaciers converging into one, and which reject at their surface the blocks which they transported enclosed in their substance.

But it may be said that no one has ever seen in Switzerland these blocks of stone immersed in the thickness of the superior glaciers. This is true ; but it is forgotten that the upper glaciers of Switzerland rest with both their sides on the mountains, and do not therefore admit of their lateral parts being examined throughout their whole height. Those of Spitzbergen, on the contrary, are disengaged from the surrounding mountains and advance on the shore, so that their sides can be studied as well as their surface.

Some skilful observers, Hoffmann† among others, still maintain the opinion formerly advanced by Saussure. "As the valleys," says this great observer,‡ "have all more or less the form of a cradle, and their bottoms more excavated than their margins, the masses of ice become compressed and tend towards the middle of the valleys : those which are at the sides are removed from them, slip towards the lowest point, and carry along with them towards the middle of the valleys the earth and stones with which they are covered."

Whether we adopt one or other of these theories regarding the existence of blocks in the *middle* of glaciers, whether we

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* Gilbert's Annalen der Physik, t. xliii. p. 388. 1819.

† Physikalische Geographie, t. i. p. 287.

‡ L. c. § 537.

consider them as central moraines, or as moveable blocks transported transversely from the sides towards the centre of the glacier, is of little importance, for it is easy to prove that these blocks could not be moved in this manner on the glaciers of Spitzbergen. To those who regard the blocks placed in the middle of glaciers as central moraines, I would say that the mers de glace of Spitzbergen are rarely formed by the union of many small converging glaciers. Placed in the bottom of bays, they rest on an amphitheatre of mountains completely hemmed in; consequently they never could have central moraines. However, the glacier at the bottom of Magdalena Bay is formed by the union of five small secondary glaciers; but these are very short, and the summits which overlook them of inconsiderable elevation. Hence it is that very few stones fall on these upper glaciers; below, on the contrary, the mountain is disengaged from the glacier, and every block which rolls down its sides falls upon the ice.

Those who favour the explanation given by Saussure will likewise find arguments in the phenomena of Spitzbergen glaciers. In truth, as the latter do not melt at the lower surface, as I shall attempt to prove, it follows that they do not become excavated beneath, and that the principal cause of the sinking of which Saussure speaks does not exist; consequently the blocks cannot thus be transported perpendicularly to the axis of the glacier. Let it not be forgotton that the lowest point of these mers de glace corresponds to the line which separates the upper from the lower glaciers of Switzerland. Now, it is exactly at this level that the blocks begin to show themselves on the surface of the Alpine glaciers;* for example, at the upper part of the lower glacier improperly called Mer de Glace at Chamouni. In Spitzbergen it is at the edge of the Mer, lower down than the point where they have been sunk in the earth, but at the same distance from the axis of the glacier, that is to say, on its lateral parts, that the blocks begin to rise to the surface.

In short, the existence of blocks on the sides, and their absence in central parts, are two unquestionable facts. It thence

* Hugi, l. c. p. 363.

follows that the glaciers of Spitzbergen are flanked by lateral moraines, in general little elevated, and always in immediate contact with the ice. In fact the blocks, existing only at the sides, are thrown off to the right and left. I have seen some which were simply resting on the surface, others half buried in the ice of the lateral walls, with only their outer part projecting; others, finally, which had rolled from the higher part of the glacier on the moraine. I remember one of these blocks measuring three cubic metres at least, which was suspended, so to speak, on a very steep part of the terminal glacier of Magdalena Bay. The sailors imagined that the exertions of a few men would make it roll down ; but they soon perceived that it was frozen by its base to the glacier, and that it would require to be detached from the ice before it could be thrown down. As there are no blocks in the centre of the Spitzbergen glaciers, the latter have no terminal moraines. It may be said that it is at the bottom of the sea that these ought to be sought for ; but the considerable depths found in their vicinity (64 to 110 metres) must make us reject this supposition.

The surface of the Spitzbergen glaciers scarcely ever melting, we never observe these blocks elevated on a column of ice (*Gletschertische*), which excite our wonder on the lower glaciers of Switzerland.

By speaking of these blocks of stone, some of which are embedded in the ice while others rest on the surface, I have touched upon one of the most controverted and important questions of geology, that of the transportation of erratic blocks or boulders. I shall confine myself to a short synopsis of the facts, which may afford arguments to the partisans of either of the two theories which now divide the learned.

1*st*, Blocks exist on the surface and in the interior of the glaciers of Spitzbergen.

2*d*, The blocks on the surface always lie on the sides of the glacier. Those which are embedded in the ice are seen throughout the whole height of the lateral walls.

3*d*, I have never seen blocks in the middle of the surface of a glacier, nor in the vertical wall facing the sea.

4*th*, Every thing leads me to believe that *the greater part* of the blocks are thrown off at the sides, and form the lateral moraines which accompany the glaciers.

5*th*, During the two voyages of the Recherche, we never saw blocks transported by floating ice ; but in crossing Bell Sound, a little before entering the bay, my colleague M. E. Robert has seen floating ice stained with earth on the surface, which at the moment was taken for sandbanks.

Thickness of Glaciers.—The lowest part of the glaciers of Spitzbergen, that which looks to the sea, always forms a vertical wall, the height of which varies between 30 and 120 metres. That of the glacier of Horn Sound, measured geometrically by Scoresby,* was 121 metres. The second of the glaciers of Smeerenberg or Fairhaven, 91 metres, according to Phipps.† At Magdalena Bay, the glacier of l'Entrée was 63 metres : that of Pointe aux Tombeaux, 76 metres, according to the measurement of the officers of our vessel who had charge of the hydrographical department. I estimate that of the two glaciers, which terminate the two bays, at 32 metres.

In Lapland, the great mass of ice which covers the Sulitelma presents an escarpment of 65 metres in height at its lower part.‡ In Switzerland, the inferior glaciers in general are only from 10 to 25 metres in thickness.§ However, the Oberaargletscher is 32 metres at its foot ;|| the Unteraargletscher, 38 metres ;¶ that of Aletsch, scarcely 30 metres, near the lake Mœrill ;** that of Bois from 25 to 30 metres.††

But the thickness of the upper glaciers, Hugi‡‡ thinks, must be estimated, at an average, from 40 to 60 metres. We thus see that the resemblance between the Mers de Glace of Spitzbergen and the upper glaciers of Switzerland is supported even by numerical calculations.

(*To be concluded in next Number.*)

* L. c. p. 102. † A Voyage towards the North Pole, p. 70.
‡ Wahlenberg, l. c. § 13. § Hugi, l. c. p. 330. || Ibid. p. 176.
¶ Ibid. p. 331. ** Ibid. ibid. †† De Saussure, l. c. § 523.
‡‡ L. c. p. 332.

*Description of the Process of Daguerreotype, and Remarks on the Action of Light in that process, both in respect to Landscape and Miniature Portraits.** By Mr Thomas Davidson, Optician. Communicated by the Society of Arts for Scotland.

12 Royal Exchange, Edinburgh,
9*th November* 1840.

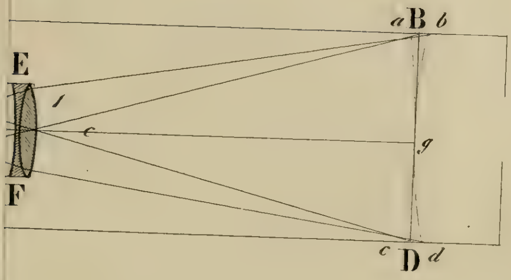
Sir,—In submitting to the consideration of your Society the few following remarks concerning my method of taking Daguerréotype pictures, &c., I beg leave most respectfully to state that, in the first part of the process, I differ from Daguérre, as I use no oil whatever in the preparation. I merely take a new plate, and polish it with the diluted acid, and a little fine rottenstone, changing the cotton and rottenstone two or three times, and then take a clean bundle of cotton-wool, which I dip or daub on a little fine dry Tripoli, and with that give a finishing lustre to the plate. I then lay the plate into a frame which exactly fits the iodine-box with its outer edge, while the plate fits the aperture exactly, having a ledge to rest on like this.

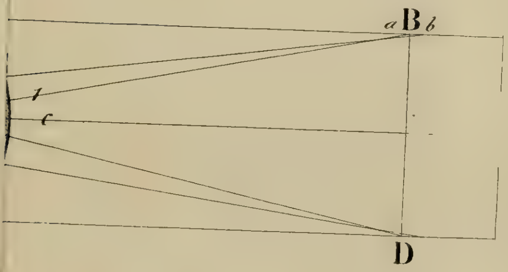
The use of this frame is to prevent the iodine from attacking the edge of the plate and spoiling it, before the centre has got enough of iodine. The iodine-box which I use is about 3 inches from the bottom to the face of the plate, which, in summer, takes from 1½ to 2 minutes in being iodinized. The plate should be reversed in its position while the operation is going on, so that it may be more regularly coated; and the lid of the box should be opened and shut very gently, lest it raise up some particles of iodine, which would attack the plate and cover it with black spots. The plate is next to be put into the frame, and exposed to the action of light in the camera, where, in summer, it should remain from 6 to 10 minutes, according to the intensity of the light and the time of the day. At present, an exposure for 15 minutes in the camera will be required.

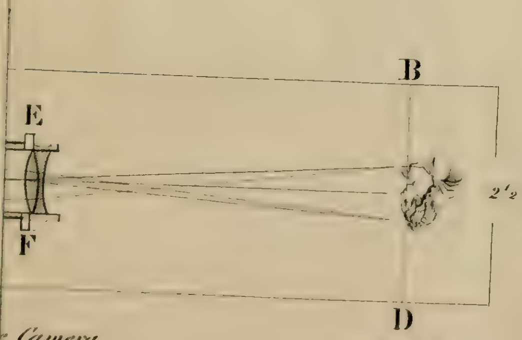
The next part of the process is the mercurializing of the picture, which is certainly the most difficult part of the whole;

* Read before the Society of Arts for Scotland November 23. 1840.

Edin^r. New Phil. Journal.
Vol. XXX, P. 178.







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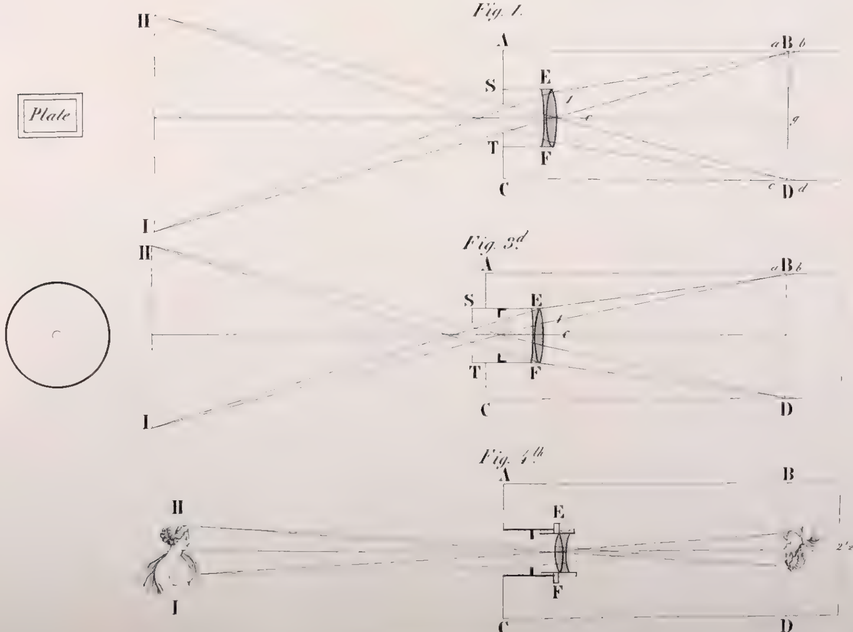
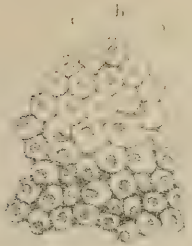
Edinr. New Phil. Journal.
Vol. XXX, P. 178.



Mr Davidson's Daguerreotype Camera.

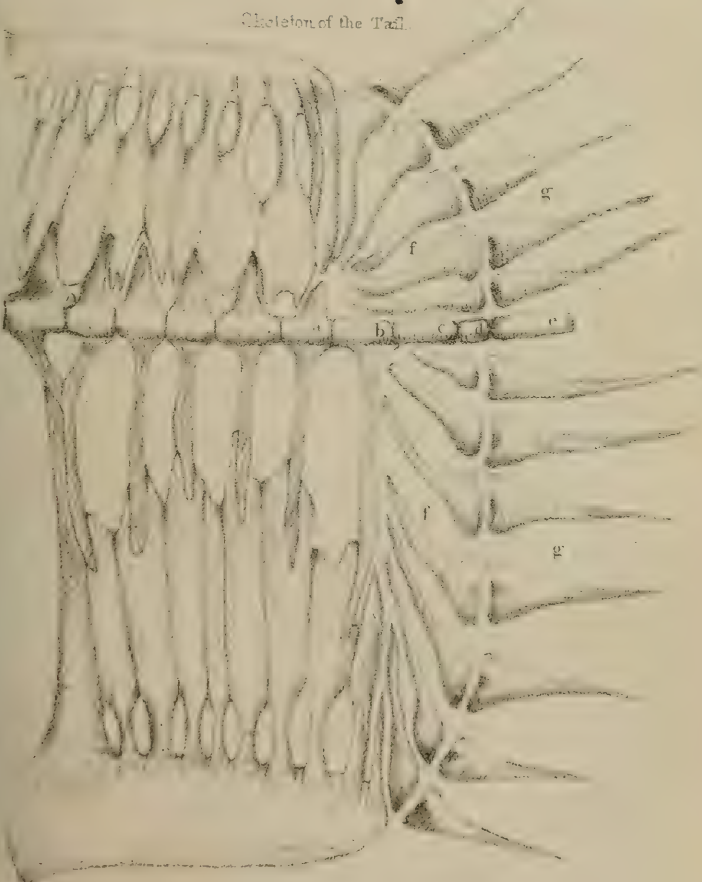
Fig. 1



Peculiar Tissue

Fig.

Skeleton of the Tail.



Short Sun Fish

and here I entirely differ from Daguérre. He recommends the mercury to be raised to about 160°, and then to take away the lamp till the mercury fall to about 120°. This will doubtless give a picture, but not a clear and brilliant one. I raise the mercury to about 165°, or from that to 170°. About 10 minutes should be occupied in raising the temperature, and about 10 or 15 in letting it fall, in order to produce a brilliant picture. The operation may be sooner completed by raising the mercury higher, but the picture would be harsh and partially blighted. The only way by which I have been able to obtain a good picture has been by finding what length of exposure in the camera was required to cause the picture appear faintly when the mercury was raised to 165° or 170°, and then, while the mercury was falling, by allowing it the time already stated, the picture came out with great force and brilliancy, free from the disagreeable dark blue tint which is invariably found in the French Daguerreotypes, and always will be by the process in the mercury, as described and recommended by Daguérre. But it must be observed that, if the picture has been kept too long in the camera, it will never stand the proper action of the mercury, but will be completely spoiled.

The last part of the process is to remove the iodine from the picture by washing it with a solution of common salt, or the hyposulphite of soda, and then with pure water.

If the plate has been exposed to the action of mercury previously to its being polished, it must be heated as recommended by Daguérre, but I polish it, as before, without oil.

Having thus given a brief description of the process of Daguerreotyping, I shall next shew the action of light in the camera, and the method by which a flat and large picture is obtained, pointing out the errors that still exist in the Daguerreotype camera, and endeavouring to demonstrate that the error in the lenses will be in Paris only the one-half of what it is here, and that the Parisians ought, therefore, to produce superior impressions. It must be obvious to all who are acquainted with the science of optics, that the lens in the Daguerreotype camera is not only placed in its worst position with respect to spherical aberration, but that the focus of the lens must be shorter in the centre than at the extremities, which I shall endeavour to explain by the following diagra m. (See Plate III. fig. 1.)

Let A B C D (Fig. 1) be a Daguerreotype camera-box, E F the lens or achromatic object-glass, H I an object placed before the camera, and DD the picture formed in the focus of the lens. Now, it must be evident to all, that, in order to give a flat picture, the focus of the lens must be longer at the edges than at the centre, in proportion as the edge of the picture at B and D is greater in distance from the lens than the centre at *g*. The lens must therefore be so constructed as to have a focus gradually increasing in length from the centre towards the circumference. Be the lens, however, as perfect as it may, yet, by attending to Fig. 1, we shall find that it will not even then give a perfect focal image. For, suppose a ray of light from the object I to fall on the centre of the lens at *c*, this ray, after refraction, cannot reach the picture at B, but its focus will be at *a*, seeing the centre of the lens is shorter in focus than the edge. Let another ray of light from the object at I fall on the extremity of the lens at E, we shall find its focus at *b*. Thus the difference of focus betwixt the central and extreme ray will be as great as from *a* to *b*, while the general focus will be betwixt the extremes as at B. Hence the picture cannot be perfectly distinct in consequence of this error, which exists not merely in theory but in practice. To demonstrate this, I took a thin plate having a hole in it about $\frac{1}{4}$th of inch in diameter, see Fig. 2; I then placed this little hole in the centre of the object-glass as at *c*, Fig. 1, and, having stationed a candle as at I, I tried to obtain the focal image as at *a*. I next put the little hole or aperture on the object-glass at 1, and found that even then the focus was considerably longer than at *c*. I now put the small hole to the extreme part of the lens as at E, when the focus was observed to be still farther lengthened, and, on comparing the experimental result with the above theory, I found them exactly to agree.

I shall next endeavour to demonstrate, from the above facts, the very great advantage the French have over us in consequence of these imperfections in the Daguerreotype camera.

Let Fig. 3 represent the same as Fig. 1, with this difference only that the stop or diaphragm S T, in front of the lens, is smaller, so that a ray of light from the object at I could not possibly fall on the centre of the lens, but half-way betwixt the centre and the edge as at 1. From what has already been said respecting Fig. 1, it will be evident that the error or disagreement of focus must in this case be only the one-half of what it is in Fig. 1, just as E 1 is half of E *c*. Hence, in Paris or Italy, if, from clearness of the atmosphere and intensity of the light, there be as much light refracted through the lens in Fig. 3 as there is through that in Fig. 1 in this country, the pictures taken there should have double the fineness and sharpness of outline in comparison with ours, if their lenses be equally good; or, should they use the same dimension of stop or diaphragm as we do, then their picture should double ours in force and brightness; which I have no hesitation in saying they would, if the process and apparatus were the same. Having thus pointed out the disadvantages we labour under in Daguerreotyping, I shall make a few observations on the taking of miniature portraits of the Daguerreotype. (Fig. 4.)

It has already been observed, that, in taking large and flat Daguerreotype views, the lens is placed in its worst position, at least if we were going to use it as a telescope; and in taking miniature portraits the lens requires to be more like that for a telescope than for a Daguerreotype, as in this case the rays require to be rather concentrated in a small space than spread over a large surface. The lens required for taking large Daguerréotype views will do equally well for taking miniature portraits. The only change required is to set the lens in a double screwed cell, as shewn at E F, Fig. 4, and to screw in the lens with its convex side to the object, the same as in a telescope, when you are taking a miniature portrait, at the same time removing the stop in front of the lens, and replacing it by one of nearly double the diameter. In using the lens for large pictures, again, it must be screwed in with its concave side towards the object, and the small stop must be restored in front. The time required for taking a miniature portrait will, by the use of the larger stop, and the consequent increase of light, be about the same as that occupied in taking landscapes or other distant objects. The Parisians will, of course, have the same advantage over us in taking portraits as in taking other objects, in consequence of their greater intensity of light.

These are the results of my experiments in Daguerreotype, which I have stated as briefly as possible, that I might not take up too much of the valuable time of this Society.

On the Epochs of Vegetation. By M. Henry Berghaus of Berlin.

Nature has made essential distinctions in the period of germination, which, as is well known, are in a great measure caused by the constitution of the seeds themselves. Thus rye, millet, and most of the cerealia, germinate in two or three days; lettuce, the gourd, and the water-cress, in from five to seven; the bean and onion in about twenty; parsley in forty days; columbine, the almond, the chestnut, the peony, the hazel-nut, &c. in from six to eight months; and, finally, the rose germinates between the first and second year.

As caloric is the most powerful of the agents which operate on vegetation, so must it also exercise the greatest and most direct influence on germination. Consequently the principle may be adopted that this phenomenon of vegetable growth stands constantly in connection with the different degrees of temperature of the soil by which the seeds are surrounded.

We recognise a more evident proof of the truth of this principle in the fact that the seeds of tropical plants, when sown in temperate climates, germinate much later than in their native soils; while the germination of seeds of colder countries is extraordinarily hastened by their being sown in temperate climates.

Hence, in the greenhouses of our botanic gardens, we must elevate the temperature considerably in order to induce germination in the seeds of tropical countries; and hence we must shelter the seeds of northern countries in the coldest and most shady spots in order to hasten their vegetation.

The difference of temperature, likewise, which occasionally occurs in one and the same season in different years exercises a great influence on the epochs of germination. In fact, we not unfrequently see that seeds of the same plants germinate much earlier when the spring arrives soon, and a mild rainy winter had preceded it, but much later when the heat of spring has been delayed by a severe winter.

Frondescence, that is the unfolding of the leaf-buds, is subjected to the same changes which are observable in the germination of seeds; for the difference of climate and that of the seasons exercise the greatest influence on this second epoch of vegetation.

The Lilac *(Syringa vulgaris)* unfolds its leaves,

In the neighbourhood of Naples (Lat. 41°), in the first half of the month of January.

Near Paris (Lat. 49°), as observed during many years, on the 12th March, as a mean.

In the middle districts of Sweden, Upsala (Lat. 60°), at the beginning of March.

The Elm *(Ulmus campestris)* unfolds its leaves,

At Naples, at the beginning of February;
At Paris, in the month of March;
In England, on the 15th April;
At Upsala, in the middle of March.

The Birch *(Betula alba)* unfolds its leaves,

At Naples, in the month of March;
At Paris, in April;
In England, the 29th March;
At Upsala, in the first days of May.

The Beech *(Fagus sylvatica)* unfolds its leaves,

At Naples, towards the end of March;
In England, the 1st May;
At Upsala, in the first days of May.

The Lime-tree *(Tilia)* unfolds its leaf-buds,

At Naples, towards the end of March;
In England, the 13th April;
At Upsala, in the first days of May.

The Oak *(Quercus)* unfolds its leaves,

At Naples, the beginning of April;

At Paris, in May;
In England, the 26th April;
At Upsala, the first days of May.

Investigations made regarding the various periods of the development of the flowers or efflorescence in the middle latitudes of Europe and North America, according to a communication made by my friend the late Mr Schübler, prove, as a general result, that the development of flowers is four days later for each degree of latitude towards the north. According to corresponding observations which were made, at the request of Schübler, by the naturalists assembled at Berlin in the autumn of 1828, during the following summer at Parma in Lombardy, and in various places farther to the north, it appears that the same plants flower at Zurich six days later than at Parma, at Tubingen thirteen days later, at Jena seventeen, at Berlin twenty-five, at Hamburgh thirty-three, at Greifswald thirty-six, and at Christiana no less than fifty-two days later than at Parma.

This likewise holds good in a corresponding way with vegetation at points at a higher elevation. In a district lying 1000 feet higher, the growth of plants, in our geographical position, is rendered at least ten to fourteen days later.

In Saxony the following observations were made in the years 1833 and 1834, regarding some of the periodical phenomena of the vegetable kingdom which depend on the influence of the climate; and the subjects of these experiments were the chief plants used as food.

Place—Absolute Height.		Period of Sowing.	Efflorescence.	Harvest.
Winter Wheat, *Triticum vulgare*.				
	Feet.			
Valley of the Elbe,	350-400	Oct. 1.-22.	May 24.-June 15.	July 20.-Aug. 8.
Freiberg,	1000-1200	Sept. 12.-Oct. 2.	June 10.-July 1.	Aug. 10.-20.
Rye, *Secale cereale*.				
Valley of the Elbe,	350-400	Oct. 1.-20.	May 20.-June 10.	July 4.-24.
Freiberg,	1000-1200	Sept. 14.-Oct. 18.	May 22.-June 12.	Aug. 1.-14.
Marienberg, Annaberg,	1900-2000	Sept. 16.-Oct. 19.	June 26.-July 9.	Aug. 10.-Sept. 15.
Wiesenthal,	2700-2800	Sept. 18.-Oct. 3.	June 28.-July 4.	Aug. 30.-Sept. 12.

Place—Absolute Height.	Period of Sowing.	Efflorescence.	Harvest.
OATS, *Avena sativa.*			
Feet.			
Valley of the Elbe, 350-400	April 1.-30.	June 1.-July 8.	July 26.-Sept. 6.
Freiberg, . 1000-1200	April 20.-May 20.	July 15.-25	Aug. 15.-Sept. 15.
Marienberg, Annaberg, 1900-2000	April 20.-May 25.	July 24.-Aug. 16.	Sept. 6-28.
Wiesenthal, . 2700-2800	May 16.-31.	July 30.-Aug. 16.	Sept. 6-28.
BARLEY, *Hordeum æstivum.*			
Valley of the Elbe, 350-400	April 14.-May 10.	June 17.-July 19.	July 18.-Aug. 19.
Freiberg, . 1000-1200	May 4.-16.	July 25.-Aug. 10.	Aug. 25.-Sept. 16.
Marienberg, &c. 1900-2800	May 5.-18.	July 30.-Aug. 15.	Aug. 25.-Sept. 20.
The POTATO, *Solanum tuberosum* (common, not early).			
Valley of the Elbe, 350-400	April 10.-May 20.	June 9.-Aug. 1.	Sept. 15.-Oct. 20.
Freiberg, . 1000-1200	April 6.-May 24.	July 1.-Aug. 26.	Sept. 20.-Oct. 28.
Marienberg, Annaberg, 1900-2000	April 19.-May 30.	July 4.-Sept. 1.	Sept. 22.-Oct. 30.
Wiesenthal, . 2700-2800	May 12.-June 6.	Aug. 18.-Sept. 12.	Sept. 28.-Oct. 30.

In higher latitudes, in districts situated in the north of Germany, the development of vegetation is less retarded than in more southern positions; the delay of the development of efflorescence between Hamburgh and Christiania amounts to only 3.4 days for one degree's approach towards the north, while that between Southern Germany and Smyrna in Asia Minor, which is in the same parallel as the most southern portions of Europe, amounts for the same space to 7.4 days.

The cause of this difference arises from the different lengths of the days, which in higher latitudes, during the warm period of the year, increase in a much greater degree than they do in southern parallels, by which the vegetation is hastened into development more speedily; and it is only in this way that it is possible in higher latitudes for various summer plants to reach their requisite maturity.

Some plants exhibit in this respect remarkable differences; the activity of vegetation is not increased in equal proportions in different plants by the same elevation of temperature. Plants of northern climates are less retarded in their development at the same low temperature than are plants of more southern regions. If we compare Europe with America, we find that

	In the New World, Perth-Amboy (Jersey), Lat. 40° 20′	In the Old World. Naples, 40° 51′	Tubingen, 84° 31′
The Peach-tree blossoms the	21st April,	the 8th February,	the 6th April.
The Pear-tree, . . .	27th April,	8th March,	4th May.
The Apple-tree, . . .	2d May,	8th March,	8th May.

Between Perth-Amboy, on the east coast of North America, and Naples, there is therefore a difference in the flowering period of the peach-tree of ten weeks, although the two places are nearly under the same parallel; and there is a difference of six weeks in the case of the pear-tree, and eight in the apple. Perth-Amboy, however, lies on the isothermal parallel of $12\frac{1}{2}°$, while Naples is nearly on that of 17°. At the former place, the winter has a temperature of only 32°.54 F., and at the latter of 50° F. The mean temperature of April at New York (Lat. 40° 40′) is 49°.1 F., and at Tubingen 48°.2 F. *Amygdalus persica* therefore requires for the development of its blossoms at least 48°.2 F., and Naples has that temperature in the month of February.

The third epoch of vegetation, the ripening of the fruit, or fructification, is subjected to the same variations as the preceding epochs, both as regards the influence of the thermal nature of the season for one and the same place, and the difference of climate in different latitudes.

The wheat harvest begins in the neighbourhood of Naples in June, in Central Germany in July, in the south of England and in the middle districts of Sweden on the 4th August.

The barley-harvest takes place at Naples in June, in Central Germany about the end of July or beginning of August, in England on the 14th August, and in the middle districts of Sweden on the 4th August.

Ripe cherries are to be had in Naples in the first days of May, in Paris towards the end of June, in Central Germany about the end of June, and in the south of England not till the 22d July.

Owing to the comparatively higher temperature which prevails in summer in Sweden, and to the more rapid vegetation there than in England, the wheat harvest does not take place sooner in the south of England than at Upsala, but occurs about the same time; and barley is ten days later of ripening in England than in Sweden. For July, in England, has a mean temperature of 60°.8 F., and about 62°.6 F. at Upsala; and August in England has a mean temperature likewise of about 60°.8 F., and in Sweden about the same, viz. 60°.26 F.

If we adopt the observations made during two years in Saxony, we find, as the mean result, that, from efflorescence to ripeness of fruit, 56 days are required for wheat, 59 for rye, 31 for barley, 45 for oats, and 68 days for the potato.

According to the observations made in Würtemberg, which were continued for several years, the same period of vegetation required 56.4 days for rye; 42½ for *Triticum spelta*; 51½ for winter barley (*Hordeum vulgare*); 25 for summer barley (*H. æstivum*); and 25½ days for oats.

In the portion of the valley of the Elbe, which is in Saxony, taking the mean of the two years 1833 and 1834, the vine flowered on the 17th June, and the vintage began on the 16th October. Between the two epochs there is a period of 121 days. In Würtemberg, the *Vitis vinifera* requires 110.3 days; and near Stuttgart the vintage commences on the 15th October, taking the mean of 65 years.

From the comparison of the Saxon data, there results that, for every 100 Parisian feet, there is a delay

	In flowering,	*In the harvest,*
Of wheat,	2.2 days.	2.2 days.
Of rye,	1.3	2.2
Of oats,	2.0	1.4
Of barley,	2.2	2.2
Of potatoes,	2.3	0.5

The approximate results differ considerably from the determinations calculated by Schübler.

As to the fall of the leaf or defoliation, the hazel-nut tree, the ash, the lime, the poplar, and the maple, lose their leaves at Upsala at the very beginning of autumn; while in the neighbourhood of Naples they remain in full foliage during the whole month of November. The apple-tree, the fig-tree, the elm, the birch, and the different kinds of oak, which in Paris are deprived of their leaves at the beginning of November, retain them at Naples till the end of December.

In England, the wallnut is one of the first trees which loses its leaves; and after it the mullberry, the ash, especially when it has had much blossom, and then the horse-chestnut. All supported trees, so long as their heads are sound, retain their leaves for a long time. Apple and peach trees often remain green till the end of November. Young beeches never cast their leaves before the New Year, and only do so when the new leaves force them off; tall beeches lose their leaves towards the end of October.—*From Berghaus's Almanach für* 1840.

On certain Peculiarities in the Structure of the Short Sun-Fish (Orthagoriscus Mola).* By John Goodsir, Esq., M.W.S. Communicated by the Author. With a Plate. (Pl. IV.)

The anatomy of an animal or vegetable may be investigated and described with two objects in view; first, the elucidation of its habits, and of its true place in the system; or, secondly, the discovery of the laws which regulate organic form and tissue (Morphological and Teleological laws).

It is with the latter object in view that I have now to offer a few observations on certain peculiarities in the structure of the sun-fish, as confirmatory of some of the principles to which I have just alluded.

The anatomy of this fish has been investigated by Dr Jacob, in a paper which I have not had an opportunity of consulting.† Cuvier and Meckel, in their Systems of Comparative Anatomy, have recorded its various peculiarities, but as the observations of these anatomists have a reference to its general structure, I shall not have occasion to refer again to their labours.‡

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* Read before the Wernerian Society 12th December 1840.

† Dublin Philosophical Journal, November 1826, referred to by Mr Yarrel.

‡ Since this paper was written, I have seen a Leyden Inaugural Dissertation, May 1840, P. H. J. Wellenbergh, "Observationes Anatomicæ de Orthragorisco Mola." The author gives a detailed account of the skeleton, intestinal canal, and heart, but throws no light on any of the subjects treated of by me.

The specimen I had an opportunity of examining was the very large individual lately procured for the Natural History Museum of the University of Edinburgh.* It measured five feet eight inches from the snout to the tail, three feet three inches from the tip of the dorsal to the tip of the anal fin, and weighed four hundred and eighty-nine pounds.

On commencing to remove the skin, which was found to be rather a difficult operation, in consequence of the total deficiency of any structure resembling the dermis, I found that the coloured and tubercular layers of the integuments were attached to the external surface of a structure or tissue of a very peculiar kind. This tissue extended in the form of a layer, varying from one-fourth of an inch to six inches in thickness, all over the body, head, and fins. It was thickest along the median line of the back and belly, of medium thickness along the sides, and thinnest on the surface of the fins. Large and thick masses of it enveloped the bones of the cranium, and enclosed the opercular laminæ and branchiostegous rays. The soft cartilaginous bones were imbedded in such a manner in its substance that they presented the appearance of nuclei in it, and resembled the first traces of the skeleton in the early embryo. The most distant or peripheral elements of the skeleton, the fin-rays, and certain parts of the opercular apparatus, were so much softer and more delicate than the tissue in which they were imbedded, and so completely deficient in any periosteal covering, that they could only be discovered in the fresh state by their translucency.

This peculiar tissue was separated from the muscular substance in its neighbourhood by the ordinary loose filamentous structure (cellular membrane). Its relation to the skin was very peculiar, and will be explained after the structure of the tissue itself has been described. The tissue was inelastic, tough, of a dead white appearance, resembling lard, granular when torn, and presented very slight traces of vascularity, and these only in the neighbourhood of certain parts of the skeleton. It discharged no oil, but on standing a quantity of watery

* This specimen was found in shallow water in the Frith of Forth at Culross, lying sluggishly on one side, but making vigorous resistance when attacked.

fluid exuded, and its bulk was considerably diminished. When boiled, it dissolved into a gelatinous mass, which passed in the form of a clear transparent liquid through flannel. A few shreds of animal matter remained. This fluid on cooling became a fine jelly, inodorous and tasteless. The greater part of this tissue, then, is composed of, or may be converted into, gelatine.*

When thin sections were examined under the microscope, it was found to consist of a congeries of cells, which exhibited all the characters of true primitive cells, each containing its own nucleus, and the latter a number of minute nucleoli. These cells were all spherical, or rather belonged to the order of spherical cells, for they appeared occasionally, when much crowded together, to be flattened at the points of contact, so as to assume the dodecahedral form. After careful examination, I could detect no fat globules either in the interior of the cells or in the spaces between them. They appeared to be filled with a transparent fluid, which, in individual cells, refracted the light with great brilliancy. The cells adhered very firmly, a circumstance which accounted for the peculiar toughness of the tissue. Bloodvessels were detected in some of the sections, but they assumed the appearance more of intercellular spaces than of true vascular canals. Not having any efficient micrometrical apparatus at the time, I could only judge of the size of the cells by comparing them with human blood-globules. I by this means ascertained that the cells in the gelatinous tissue of the sun-fish were about a third larger than human blood-globules, that is .000332 to .000505 of an inch.

There was no trace of dermis or true skin, the coloured lamina of the integument appearing to be merely the superficial layer of the peculiar cellular tissue, changed by the deposition of colouring matter in the cells to adapt it to its proper function. The peculiar tissue must either be looked upon as the true skin itself, or more correctly it must be considered as the primitive nucleated vesicular tissue of the embryo fi h,

* I am indebted to Dr Atken for this account of the chemical constitution of the tissue.

from which the pigmentary and tubercular layers of the skin has already been developed, but from which the conversion into true skin has not begun, and that of the peripheral bones has been arrested.*

From what I have now stated, it is evident that the interest to be attached to this tissue consists in its purely embryonic character. The general appearance of the cells, their nuclei and nucleoli, the uniformity of the tissue in every part of the animal, and its chemical composition, all indicate this character; and when taken in connection with the embryonic state of the bony tissue, and the rudimentary condition of the muscular system, forms a very interesting and important character in the species, and probably in the order of fishes to which the one under consideration belongs.

In a teleological point of view it is important, as it points to the existence of certain laws which regulate the development of animal tissues, namely, first, *In the organic series, tissues as well as forms undergo progressive development*; secondly, *This progressive development of the tissues may be retarded, retaining their early embryonic condition in certain beings in the series*; and, thirdly, *Tissue is subordinate to form.*† A sun-fish, in fact, as well as other fishes of its order, is as highly developed, in so far as regards form, as any in the class; in certain of its tissues it is still in the condition of an embryo.

The second peculiarity to which I shall refer, is the form of the caudal fin of the *orthagoriscus*. The naturalist is familiar with its truncated shape, but the anatomist has not yet ascertained the cause of the peculiarity of this part of the skele-

* Meckel, Comparative Anatomy, French edition, tom v. p. 185. According to the observations of Dr Jacob, in the Dublin Philosophical Journal, the cetacea have no dermis, except we consider, along with him, that the blubber is the true skin distended with oil. The subcutaneous fat of the cetacea, however, differs from the gelatinous vesicular tissue of the sun-fish in having no primitive cells in its constitution, consisting of common fibrous tissue inclosing in its areolæ fat or oil cells. It may, nevertheless, be considered as a tissue in which some of the primitive cells have been developed into fibrous tissue, while others have become filled with oil.

† For an exposition of this last law of organization, see Dr Martin Barry's memoir on Embryology, 2d series, Phil. Trans. 1839.

ton.* I found that the rays of the tail-fin, and their interspinous bones, were crowded together in a direction from behind forwards, and abutted against the superior-spinous process of the fourteenth and the inferior-spinous process of the fifteenth vertebra. The sixteenth, seventeenth, and eighteenth vertebræ assumed the appearance, the two former combined, of an interspinous bone, and the latter of a fin-ray, and could not have been distinguished from these but by their direct continuation with the bodies of the vertebræ, and their more cylindrical and shorter form. The joint between the seventeenth and eighteenth was in the line of the articulations of the fin-rays and their interspinous bones, the ultimate vertebra assuming the appearance and function of a fin-ray, the penultimate and antepenultimate combined of an interspinous bone.

The interest involved in this form of skeleton consists in the explanation it affords of the true nature of the so-called last vertebra in the spinal column of fishes. Is that fan-shaped bone a vertebra? or is it a composite bone, containing the elements of a number of vertebræ and of interspinous bones of fin-rays? I have always been led to conclude that it is a composite bone, and it required only such an arrangement of skeleton as that now under consideration to afford a natural analysis of the tail in this class of fishes, and to prove the correctness of the opinion to which I have just alluded. In many of the osseous fishes the last bone of the spine exhibits traces of a central element, and in some families (Tænioides) it appears to be prolonged far beyond the caudal fin, in the form of a fine filament, but in none, as far as I am aware, is it arranged as in the present instance.

The next peculiarity is in the muscular system. The sun-

* Meckel, Comp. Anatomy, French ed., tom. ii. p. 285. For a drawing of the tail, which appears to have been made from a dried skeleton, see Dr C. A. S. Schultze, Ueber die ersten Spuren des Knochensystems, und die Entwickelung der Wirbelsaulè, in den Thieren, Meckel's Archives, 1818. Willenbergh's drawing, which, he states, was made from a dried skeleton, is incorrect in the mode of junction of the pectoral girdle to the spine, but more particularly in the mal-representation of the mode of termination of the spinal column. He has mistaken the two or three last vertebræ for a fin ray and interspinous bone.

fish exhibits not a trace of abdominal muscles.* The viscera from the spine to the median line of the belly are inclosed by abdominal walls, consisting of peritoneum on the internal surface, of skin on the external, and of a thick layer (4 or 6 inches) of the peculiar vesicular tissue already described between them. This is a conformation exactly corresponding with the embryonic condition of all the vertebrata. The abdominal muscles are among the last to be developed, in consequence, in a great measure, of the persistence of the yolk-bag, and the evolution of the abdominal walls from the dorsal towards the ventral aspect.

The muscles of the spine, also, instead of stretching from head to tail, are reduced to a very small size, and constitute only a weak fan-shaped muscle on each side of the caudal-fin. These muscles consist of a small digitation for each of the fin-rays, and appear to me rather to be analogous to the caudal-fin muscles of other fishes than to the great lateral muscles of the spine.

The thick mass of muscle on each side of the sun-fish consists of the muscles of the anal and dorsal fins; very weak in other fishes, but developed here, in an extraordinary manner—in an inverse ratio to the spinal muscles.† This inversion of the muscular masses depends on the stunted condition of the vertebral column, and on the developed state (in regard to form) of the peripheral elements of the skeleton, and is an instance of the dependence of one organic system on another. The morphological cause of the stunting of the column is still a problem,‡ and must be sought for, probably, as a circumstance

* Meckel, Comp. Anatomy, tom. v. p. 185.

† Meckel, Comp. Anatomy, tom. v. p. 185, inclines to the opinion that the dorsal portion of the lateral fleshy mass of the sun-fish is a composite muscle, consisting of the anterior part of the usual lateral spinal muscles, and of the muscles of the dorsal fin; or that the latter had assumed the form and position of the former. I have, however, satisfied myself that this mass, although extending forwards to the head, is, in fact, the fin muscle, and that it consists of uninterrupted radiating bundles. The body of this fish, then, contains only six muscles—two for each fin. It appears to swim by a sculling action of the dorsal and anal fins, the tail being a very inefficient organ of locomotion.

‡ I have not assumed the short spinal cord of the orthagoriscus mola as the cause of the stunting of the osseous column, as there are contradictory

connected with development. Whatever it may be, it must be considered as the means of adapting the structure of the animal to some peculiarity in its habits or economy.

Explanation of Plate IV.

Fig. 1. The cells of the peculiar tissue: *a*, cell; *b*, nucleus; *c*, nucleoli.
Fig. 2. Skeleton of the tail of orthagoriscus mola: *a*, fourteenth vertebra; *b*, fifteenth; *c*, sixteenth; *d*, seventeenth; *e*, eighteenth; *f*, interspinous bones abutting against spinous processes of vertebræ; *g*, rays of caudal-fin.

On the former Existence of Glaciers in Britain.

I. *On the former Existence of Glaciers in Scotland.* By Dr Buckland.*

Dr Buckland, in a Memoir on this subject read before the Geological Society of London on the evenings of the 4th and 18th of November last, commenced by observing, that, when his attention was first directed by Professor Agassiz, in October 1838, to the polished, striated, and furrowed surfaces of the rocks on the slopes of the Jura, near Neuchatel, as the effects of glaciers, he doubted the correctness of the inference; but after devoting some days to the examination of actual glaciers, and the effects produced by them, he became a complete convert to the glacier theory, as far as relates to Switzerland. On his return to Neuchatel, in the same year, he informed M. Agassiz that he had noticed similarly polished and striated rocks, in 1811, on the left side of the gorge of the Tay, near Dunkeld, though he then attributed the appearance to diluvial action; that, in 1824, he had noticed, when in company with Mr Lyell, grooves and striæ on the surface of granite rocks, near the east base of Ben Nevis; and that near the base of Ben Wyvis, Sir G. Mackenzie pointed out to him a high ridge of gravel, arranged obliquely across a valley, and not explicable by any action of water. Those phenomena, however, since his examination of the Swiss glaciers, he has been convinced, may be explained by the friction of ice upon rocks, and the production of transverse

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facts which must be explained before we can connect the length of the latter in the embryonic and adult series with the length of the former.

* We recommend to those interested in the system of glaciers, as taught by Professor Agassiz, which is nearly that adopted by Dr Buckland and others, to read previously Professor Agassiz's system, as explained by himself in a Memoir entitled "Upon Glaciers, Moraines, and Erratic Blocks," in vol. xxiv. of this Journal, extending from page 364 to page 383; or the classical work, just published, entitled "Etudes sur les Glaciers. Par L. Agassiz."

moraines. After these preliminary remarks, Dr Buckland proceeds to describe the evidences of glaciers observed during a tour made in the autumn of this year, partly before and partly subsequent to an excursion in company with Professor Agassiz. He, however, omits for the present all details respecting parallel terraces, though he is convinced that they are intimately connected with the glacier theory. The observations commenced in the neighbourhood of Dumfries, and were afterwards extended over a line of country ranging from Aberdeen, by Forfar, Blairgowrie, Dunkeld, Loch Tummel, and Loch Rannoch, to Schihallion and Taymouth, and thence by Crieff, Comrie, Lochearn Head, Callendar, and Stirling, to Edinburgh. The tour was subsequently prolonged in England, by Berwick, the Cheviots, Alston Moor, and Shap Fell, to Lancashire and Cheshire; but the details of this portion of the series of observations will be given in a paper to be read on the 2d of December. The evidence of the former existence of glaciers in the vicinity of Dumfries, occurs in the picturesque ravine of Crickhope Linn. On emerging from the chasm at the upper end of this ravine, a remarkable example of a long terminal moraine is visible, stretching across the mountain-valley, from which the Dolland Burn descends, to fall into Crickhope Linn. When seen from a distance, it resembles the vallum of an ancient camp, being covered with turf; but is composed of rolled pebbles, chiefly of slate rock, originally derived from the adjacent Lammermuir Hills, and of a few rolled fragments of granite. It presents no traces of stratification. The height varies from twenty to thirty feet, the breadth of the base is about one hundred feet, and the length is four hundred yards, occupying the entire breadth of the valley, except near the centre, where the moraine is intersected by a road, and at the west end, where it is traversed by the Dolland rivulet. To moraines, or the detritus of moraines, Dr Buckland refers the gravel and sand which cover the granite table-land between Aberdeen and Stonehaven; the large insulated tumuli and tortuous ridges of gravel which occupy a tract of one hundred acres near Forden, one mile east of Achinbald; the blocks, and more or less stratified gravel which are spread over the first level portion of the valley of the North Esk, after it emerges from the sub-Grampians; also the ridges and cones of gravel at Cortachy and Piersie, not far from Kirriemuir. Near the summit of the hill which forms the left side of the main valley at the confluent point of the Piersie and Prosen valleys, is a polished surface of porphyry, striated in the direction which a glacier, descending the valley, would have maintained. The vast longitudinal and insulated ridges of gravel, extending two or three miles up the valley east of Blairgowrie, and the transverse barriers, which form a series of small lakes on its west, in the valley of Lunanburn, Dr Buckland considers to be moraines; also, the lofty mounds composing the ornamental grounds adjacent to Dunkeld Castle—the detritus which covers the left flank of the Tay, along a great part of the road from Dunkeld to Logierait, the left flank of the Tummel valley from Logierait to Killiecrankie, and the left flank of the

Garrie, from Killiecrankie to Blair Athol; likewise, the vast congeries of gravel and boulders lodged in the shoulders of the mountain opposite the gorge of the Tummel, and accumulated, the author believes, by glaciers which descended the valley of the Tummel, from the north side of Schihallion, and the mountains around Loch Rannoch. This elevated mass of moraines and detritus of moraines in the lateral valley of the Tummel, Dr Buckland conceives, was formed across the longitudinal valley of the Garry, in the same manner that modern glaciers of the Alps,—as in the case of the Val de Bagne,—occasionally descend from the transverse across the longitudinal valleys. The mammellated, polished, and striated slate-rocks, about one mile above the falls of Tummel, and forming the left portal of the gorge of the valley, Dr Buckland mentions as proofs of the action of a glacier which descended the gorge: he alludes also to the indications of polish on veins of quartz which project eight or ten inches above the surface of weathered masses of mica-slate, near the same locality; and to the slight scratches on mammellated rocks at Behaly, one mile and a half east of Tummel Bridge. The evidence of glaciers on Schihallion, he shews, are visible on the north and north-east shoulders of the mountain, in rounded, polished, and striated surfaces, many of which have been recently laid bare in forming a new road. The surface of a porphyry-dike, about forty feet wide, and lately exposed near the thirteenth milestone on the left flank of the valley called the Braes of Foss, is polished and covered with striæ, parallel to the line of descent which a glacier from Schihallion would assume; and on the right flank of the same valley, one hundred yards north of the eleventh milestone, is another smaller vein of red porphyry, similarly polished and striated. In the intermediate space, newly uncovered surfaces of hard slate-rocks and quartzite, present phenomena of the same nature,—and the whole of these phenomena are ascribed to the agency of glaciers. The two lofty ridges of gravel in Taymouth Park, ranging at right angles to the sides of the valley, between the village of Kenmore and the Castle, the mound on which stands the ornamental dairy, and the gravel on which are situated the woods overhanging the left bank of the lower end of Loch Tay, Dr Buckland considers to be moraines, or the detritus of moraines; likewise the deeply scored and fluted boulders of hornblende rock with other debris, which occur at the junction of Glen Moulin with the Lyon. The proofs of glacier-action in Glen Cofield, are shewn to be a remarkable assemblage of moraines upon the high land which divides the valley of the Tay from that of the Bran; also a group between the sixteenth and fourteenth milestones, consisting of forty or fifty round topped moraines, from thirty to sixty feet high, crowded together like tumuli. It is impossible, Dr Buckland says, to refer these mounds of gravel and blocks to the action of a current of water, as they are placed precisely at the point where a stream, descending from the high lands, would have acted with the greatest velocity; they, moreover, exactly resemble some of those moraines which occur in the valley of the Rhone, between Martigny and

Leuk. The village of Amulrie, Dr Buckland conceives, is situated on a group of low moraines, and he states that the road, for two or three miles towards Glen Almond, passes over similar accumulations, and surfaces of mica-slate, rounded by glaciers. The proofs of the action of ice in and near South Earn, consist, between Crieff and Comrie, in irregular terraces of gravel, or detritus of moraines; and in rounded as well as guttered surfaces of slate-rocks at the west end of Comrie, near the bridge; and in the woods adjacent to Lawers' House. In the valley of the Lednoch, Dr Buckland found farther proofs of glacial action, and at points where, if the glacier theory were true, he had assumed they ought to exist. Immediately above the gorge called the Devil's Cauldron, particularly near Tentallich, he noticed rounded surfaces of greenstone partially covered with moraines; and at Kanagart, also immediately above the gorge, a small cluster of moraines, easily separable into lateral and terminal. Two miles higher, at the confluence of Glen Lednoch with Glen Garrow, a distinct medial moraine forms an insulated ridge in front of the point of union of the two valleys. The farm-house of Invergeldy is said to stand on the detritus of a moraine, and the surface of the granite at Invergeldy, from which the stone was procured to erect Lord Melville's monument, near Crieff, is stated to bear evidence of having been rounded by glacier action. On a hill of trap, half a mile south of the farm of Lurg, on the left side of Glen Lednoch, a striated and polished surface is distinctly preserved. In Glen Turret, Dr Buckland found on the shoulder of the mountain, immediately above the south-west extremity of the Loch, a vast lateral moraine, in a deep ravine, and at the falls of Turret, at the lower extremity of the gorge, an extensive lodgment of moraines, whilst at the upper end of the gorge on the left bank of the river, near a gate which crosses the road, he noticed polished and furrowed surfaces of slate-rocks, at precisely the place where, theoretically, he had asserted they ought to be found. The banks of Loch Earn, and the surrounding country, afforded Dr Buckland the following evidence of glaciers having existed in that district. On the north bank of the Loch, he observed rounded and furrowed surfaces and portions of lateral moraines exposed in roadside sections, and at Loch Earn Head, a group of conical moraines occupying the middle of the valley, at a point where, had the detritus been brought by a rapid current, it must have been propelled into the loch, but if brought by a glacier would have been deposited as a terminal moraine. Further evidence of moraines are stated to occur in the valley of the Teith from Loch Catharine to Callender; and the lofty parallel terraces in the same valley are considered to be detritus of moraines, modified by the great floods which accompanied the melting of the ice. One of them, near Callender, has been hitherto believed to be a Roman camp, and has been mapped as such. The little lakes on the right bank of the Teith, four miles east of Callender, Dr Buckland considers due to a series of moraines obstructing the drainage of the country; and the first table-land, after crossing the river towards Doune, to be composed of re-arranged glacial detritus; also the ground on which Mr Smith's farm, near Doune, is situated. Having thus proved that glaciers once occurred in the glens and mountainous districts of Scotland, Dr Buckland was anxious to ascertain the amount of evidence which Stirling and Edinburgh would afford of their action at points but little raised above the level of the sea, and far distant from any group of mountains. He had noticed in 1824, on the summit of the hill at Stirling, that the surface of the trap then recently uncovered, between the castle and the church, was polished and striated, but at his last visit those proofs had been obliterated. The grooves and scratches, described by Sir James Hall on Corstorphine Hill, near Edinburgh, and on the Calton Hill, Professor Agassiz informed him, entirely resemble the effects produced by the under-surface of modern glaciers. In his recent examination, in company with Mr M'Laren, of the Castle rock at Edinburgh, he found polished and striated surfaces at the north-west and south-west angles; and at the base of the north-west angle a nearly horizontal portion of the rock covered with wide striæ, ranging east and west.* Some of these scratches and rounded surfaces, Dr Buckland says, may have been produced by stones projecting from the sides or bottom of floating masses of ice, but that it is impossible to account by this means for the polish and striæ on rocks at Blackford Hill, two miles south of Edinburgh. On the southern face of this hill, at the base of a nearly vertical cliff of trap, is a natural vault, partially filled with a breccia composed of gravel and sand cemented by a modern infiltration of carbonate of lime. The sides and roof of the vault are highly polished, and covered with striæ, irregularly arranged with respect to the whole area, but parallel over limited extents. It is impossible, Dr Buckland observes, to refer those striæ to the action of pebbles set in motion by water, because fragments of stone moving in a fluid cannot produce continuous parallel lines; and because, if they could produce them, the striæ would be parallel to the direction of the current. It is impossible, he also states, to refer them to the effects of stones fixed in floating ice, as no such masses could have come in contact with the roof of a low vault; but that it is easy to explain the phenomena by the long continued action of fragments of ice forced into the cave laterally from the bottom of a glacier, descending the valley, on the margin of which the vault is placed; and the irregular grouping of the parallel striæ to the unequal metion of the ice, charged with fragments of stone. The position of the cave does not exceed 300 feet above the level of the sea, and the proving of glacial action at this point, the author states, justifies the opinion that glaciers may also have covered the Calton Hill, and the Castle Hills of Edinburgh and Stirling.

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* The appearances on the Castle rock, noticed above, can scarcely be considered glacier markings.

II. *On the Geological Evidence of the former existence of Glaciers in Forfarshire.* By Charles Lyell, Esq., F. R. S., M. G. S., &c.

Mr Lyell on the 18th of November and 2d December 1840, after the reading of the memoir of Dr Buckland, laid before the Geological Society a series of observations on the ancient glaciers of the county of Forfar. Mr Lyell, after some preliminary observations, remarked that he had, by the use of the theory of M. Agassiz, become convinced that glaciers not only existed a long time in the Grampians, but extended even into the low country; and, further, that in his opinion, immediately antecedent to the present epoch, there may have been great oscillations of climate in the Northern Hemisphere.

The county of Forfar, Mr Lyell divides geologically into three principal districts,—that of the Grampians, formed of granite, gneiss, mica-slate, and clay-slate, flanked by a lower range of old red sandstone associated with trap—that of Strathmore, composed of old red sandstone—and that of the Sidlaw Hills, constituted of the inferior beds of the same formation, usually accompanied by trap; and, he adds, that the district may be considered to represent on a small scale, both geologically and physically, that part of Switzerland where the phenomena of erratic blocks are most remarkable, for the Grampians with their crystalline rocks are comparable to the Alps, the Sidlaw Hills with their secondary formations to the Jura, and Strathmore to the great valley of Switzerland; and the masses of Grampian rocks in the Strath and at considerable heights on the Sidlaw Hills, recall to mind the erratic blocks of the Pays du Vaud and the Jura. The detritus spread over Forfarshire, Mr Lyell divides into three deposits, presenting distinct characters; —1st, the thin covering on the tops and sides of the Grampians, and derived from the disintegration of the subjacent formations, with a slight intermixture of pebbles traceable to rocks not far distant and at higher levels; 2dly, the impervious till and boulders, with other unstratified, transported materials, disposed at various heights in the glens and the Strathmore; and, 3dly, the stratified gravels, sands, and clays, which overlie the unstratified. The accumulations belonging to the second division occur on both sides of every glen, frequently arranged in terraces with a nearly flat top, and sometimes with two taluses, one towards the river, and the other of less height towards the mountain. These terraces or lateral mounds generally increase in width and depth as they descend from the higher to the lower glens, attaining in the latter sometimes a thickness of 100 feet. In the inferior part they consist of large angular and rounded fragments, imbedded in unstratified mud and sand, the composition of the mass increasing in complexity as the mounds of the lateral glens unite with those of the main glen. In the higher part they are often composed of forty and even eighty feet of gravel and sand of the same nature, but stratified. These mounds acquire occasionally, as in the glen of South Esk, so great a volume as to block up the valley, leaving only space for the river to pass. The South Esk springs from a shallow lake, twenty miles from Strathmore, and nearly 3000 feet above the level of the sea. For the first six miles, the river flows through a region of granite or gneiss, and the fragments of rocks derived from it may be traced to Cortachie, a distance of twelve miles; the detritus also in this glen, and in all the others composed of granite and gneiss, preserves throughout a uniform grey colour. On entering, however, the zone of mica-slate, it is invariably tinged red in every glen, and this colour is imparted to the detritus in the lower portions of the glen, notwithstanding the intermixture of the pale brown materials obtained from the clay-slate district. Another proof of the downward course of the transported matter composing the mounds, is the rare occurrence of fragments of quartz till the glens enter the mica schist region, where thick beds and veins of pure white quartz abounds. This distribution of the detritus, and its arrangement in mounds along the sides of the glens, Mr Lyell says, agrees well with the hypothesis of glaciers and their lateral moraines; and is not reconcilable with the theory of submergence, and the subsequent removal by denudation of the central portion of a deposit supposed to have filled the bottom of the glens. The total want of stratification he also urges as a proof that the materials were not deposited from water. The glacier theory is farther shewn to offer the only explanations of the phenomena presented by Lochs Brandy and Whoral, situate 1500 feet above the sea, and 600 above the Kirktown of Clova. Loch Brandy is surrounded on three sides by lofty precipices of gneiss, while on the south it is bounded only by an enormous accumulation of sand, mud, and fragments of rocks, evidently derived from the cliffs which overhang the lake on the east, north, and west. It is impossible, Mr Lyell observes, to conceive how these great masses could have been conveyed over a deep lake; but if it be supposed that the cavity occupied now by water was once filled with a body of ice, it is easy to account for the transport of large boulders from the northern to the southern side of the cavity, and their mode of distribution beyond it. Loch Whoral presents analogous phenomena: and the immense mass of detritus which extends from its southern side, terminates in the plain of Clova, in a multitude of hillocks and ridges, resembling in shape some of the terminal moraines of Switzerland. One of the features in the transported materials of the South Esk, formerly regarded by Mr Lyell as very difficult of solution, is a great barrier at Glenairn, where the valley contracts to scarcely half a mile in width, and is flanked by steep mountains. Viewed from below, the barrier resembles an artificial dam, 200 feet high, and divided along its summit into hillocks. On the east side, it is cut through by the Esk, and its breadth is about half a mile. Behind it, is a flat plain, four or five miles long, and a mile and a half broad, through which the Esk meanders; and that it was once covered by a lake, is proved by some deep drains, which exposed a succession of horizontal beds of sand, clay, and drift-peat. The lower part of the barrier, thirty feet in depth, laid open in the river cliff, consists of unstratified mud, full of boulders; and the upper part, from 50 to 100 feet thick, of gravel and sand, inferred by Mr Lyell, from analogy, to be stratified. If this barrier be supposed to be a large terminal moraine, accumulated by a retreating glacier, Mr Lyell states that its origin is easy to be understood; and that the water produced by the melting of the ice may have overflowed the mound, and furrowed out the softer materials composing the upper part into ridges and hillocks; but, he adds, it is difficult to comprehend how a capping of such materials on the summit of a terminal moraine could have acquired a stratified structure. At Cortachie, four miles below the barrier of Glenairn, the Esk enters the lower country of old red sandstone; and a mile and a half farther down it is joined by the Proson; and a mile yet lower, by the Carity. In the district where these streams unite, there is a great amount of unstratified detritus, full of Grampian boulders, and covered, for the most part, with stratified gravel and sand, in some places from thirty to forty feet thick. The phenomena exhibited by the till in that district, Mr Lyell conceives might be well accounted for by the union of three or four large glaciers. The author then proceeds to describe the phenomena presented by Strathmore. This district is intersected by many longitudinal ridges, some of which are 200 or 300 feet above the adjacent valleys. They are generally covered with till and erratics, derived partly from the Grampians, and partly from the subjacent old red sandstone; and the covering is so prevalent in Strathmore, that the subdivisions of the rocks *in situ* are difficult to trace. This boulder till, or mortar, as it is termed in Forfarshire, forms invariably the lowest part of the transported matter of the Strath. Mr Blackadder has ascertained that it often fills hollows which would become lakes or peat mosses if the till were extracted: and Mr Lyell observes, that if the cold period came on slowly, the action of the advancing glaciers would have pushed forward vast increasing masses of detritus, and spread them over the Strath, filling up more or less the hollows and cavities previously occupied by water. The Sidlaw Hills, the highest point of which is 1500 feet above the sea, and the whole country between Strathmore and the Tay, are overspread with an impervious boulder formation. The erratics derived from the Grampians are equal in size to those contained in the till of the glens and Strath, and are associated with fragments of the subjacent grey beds of the old red sandstone. One of the Grampian boulders, which lies within forty feet of the summit of Pitscaulie Hill (700 feet above the sea), is a block of mica-slate thirteen feet long by seven feet broad, and it is seven feet high above the ground. The nearest point from which it could have been derived is fifteen miles to the north-west. In conclusion, Mr Lyell observes, that though there are evidences of glaciers having once existed in the principal highland valleys, and their tributary glens in Forfarshire, and though the Scottish mountains may have been covered with permanent ice, yet that, in consequence of the difference of latitude, Switzerland can present but an imperfect analogy of things in Scotland during the glacier period. It is, he says, to South Georgia in the 54th degree of south latitude, to Kerguelen's Land in the 50th, or to Sandwich Land in the 59th, that the nearest approach to the supposed condition of Scotland during the glacial epoch must be looked for. In those regions the glaciers extend to the sea, and there are no warm valleys into which they can descend and melt, the temperature of summer and winter being also nearly equal. They therefore probably remain stationary. In the Alps, on the contrary, the indefinite accumulation of snow is checked, 1st, by evaporation without melting; 2d, by the descent of glaciers in consequence of gravitation, a cause considered by M. Agassiz not very influential; 3d, by the descent of glaciers arising from the expansion which accompanies the alternate freezing and liquefaction of water. The last, which is the most powerful source of relief in Switzerland, must, Mr Lyell states, be comparatively feeble in countries like South Georgia and Sandwich Land; and hence that the accumulation of ice can be checked only by evaporation and the gravitation of the mass. As the study of the tertiary strata proves that a warm climate certainly preceded the assumed glacier period in the northern hemisphere, and as a milder climate has since prevailed, the author says there are three distinct phases in the action of the supposed ice:· 1st, its gradual coming on; 2d, its continuance in full intensity; and 3d, its gradual retreat. During the first epoch, Mr Lyell observes, only the higher mountains would send down glaciers to be melted in the plains, as in Switzerland; and the ice would be in constant motion, the lower boundaries sometimes advancing, sometimes retrograding; but that from century to century it would gradually extend its permanent limits, and would finally reach the sea. During the advance, he says, the terminal moraines would be pushed forward, and forced into the cavities previously occupied by lakes. While the second period continued, he conceives, the snow accumulated to vast thicknesses, filling up the glens and plains, and leaving bare only the peaks and precipices of the loftier mountains; and that from these points the fragments were detached, and progressively, but almost imperceptibly, conveyed, which are now found at great distances from the parent rock, and at high levels. To the third epoch, or that during which the snows and glaciers gradually disappeared, he assigns the deposition of the erratic blocks on the hills and in the plains, and the production of the terminal moraines, or the existing transverse mounds, as well as the accumulation of the bodies of water from the melting of the ice, which have in various localities overflowed and modified the outline of the stratified detritus.

III. *On the former Existence of Glaciers in the North of England.* By Dr Buckland.

In a paper read to the Geological Society on the 2d of December, Dr Buckland noticed, that, proceeding southward from Edinburgh, a large portion of the low lands is composed of till, or the argillaceous detritus of glaciers, interspersed with pebbles. In the valley of the North Tyne, about one mile east of Haddington, he observed a distinct longitudinal moraine, midway between the river and the highroad, and ranging parallel to them; and he directs attention to the trap-rocks which commence a little farther eastward, and are intersected by the North Tyne for four or five miles above Linton, as likely to afford scored and striated surfaces in the most contracted parts of the valley. About four miles west of Dunbar, another long and lofty ridge of gravel stretches along the valley, parallel to the right bank of the river; and for three miles south-east of Dunbar is a series of lateral moraines, modified into terraces by the action of water. At the eastern extremity of the Lammermuir Hills, in the high valleys through which the road passes from Cockburnspath to Ayton, are traces of moraines, disposed in terraces, at various elevations, on both sides of the river; and three miles north of Berwick, the road passes near an insulated group of round and oblong mounds of gravel, lodged on the slope of a hill 300 or 400 feet above the sea. On many parts of the coast of Northumberland, deposits of till repose on the carboniferous rocks, especially near Newcastle. At the village of North Charlton, between Belford and Alnwick, Mr C. Trevelyan conducted the author, in 1821, to an extraordinary ridge of gravel, then considered a work of art, but which Dr Buckland, after an examination of the upper glacier of Grindelwald and that of Rosenlaui, in 1838, was convinced is a moraine. Immediately below the vomitories of the eastern valleys of the Cheviots, enormous moraines cover a tract extending four miles from north to south, and two miles from east to west; and the highroad winds among them from near Wooler through North and South Middleton, and by West and East Lillburn, to Rosedean and Wooperton; the height of these moraines varies from thirty to eighty feet, and their surfaces are seldom too steep to prevent the passage of the plough. On the left bank of the College Burn, immediately above a bridge at Kirknewton, Dr Buckland discovered a moraine thirty feet high, only the summit of which, to the depth of a few feet, was stratified, the remainder consisting of unstratified gravel, inclosing, however, fragmentary portions of a stratified bed of sand, some of which were vertical, and others inclined; and in the greater number the laminæ were contorted in a manner explicable, he says, only on the theory of a bed of laminated sand having been severed into fragments, which had subsequently been moved and convoluted by the slow pressure of a glacier descending the deep trough of the College Burn, from the northern summit of the great Cheviot. The proofs of the action of glaciers in the mountain and lake districts of Cumberland and Westmoreland, Dr Buckland states, are no less frequent than in Scotland and Northumberland; and he adds, assuming that, during the glacial period, every lake became a mass of solid ice, large lodgments of moraines might be expected to be found in those portions of the subjacent lowlands, in front of each of the vomitories by which the waters of the lakes are now discharged from this lofty group of mountains. Thus, to the east of Penrith, near the junction of the Eden with the waters of the Eamont and the Lowther, are extensive moraines, loaded with enormous blocks of porphyry and slate, brought down by glaciers from the high valleys which, commencing on the east flank of Helvellyn, and in the mountains about Patterdale, descend into the lake of Ulleswater, and from those by which the tributaries of the Lowther are conducted from the eastern flank of Martindale, and from Hallswater and Mardale. A remarkable group of these moraines is at Eden Hall, four miles east of Penrith. On the southern frontier of these mountains in Westmoreland and Lancashire, are similar moraines on a most extensive scale. Thus, immediately below the gorge through which the waters of the Kenn descend from the mountains of Kenmuir and Long Sleddale, the valley of Kendal is covered with large insulated piles of gravel, whilst smaller moraines and the detritus of moraines nearly fill the valley from Kendal to Morecambe Bay. Five miles north-east of Kendal, in the highroad from Shap, and on the shoulder of the mountain immediately in front of the valley of Long Sleddale, is a group of conical and oblong moraines, distinguished by the superior fertility of their soil to that of the adjacent slate-rocks. South of Kendal, the highroads from Burton and Milthorpe to Lancaster, pass for the most part over moraines or their detritus ; Lancaster Castle also stands on a mixed mass of glacial detritus, probably derived chiefly from the outsweepings of the valley of the Lune. The districts of Furness, Ulverston, and Dalton, are extensively covered with deep deposits of moraines, formed from the wreck of mountains surrounding the upper end of Windermere and Coniston Lakes ; and a bed or capping of till and gravel thirty or forty feet thick overlies the great vein of hematite near Ulverston. The south-west and western portions of Cumberland, Dr Buckland has not recently examined ; but he is of opinion that many of the conical hillocks marked on Fryer's large county map, in the valley of the Duddon, will prove to be moraines, derived from the adjacent mountains ; also those on the right of the Esk, at the east and west extremities of Muncaster Fell ; and those near the village of Wastdale. Dr Buckland is further of opinion, that though no similar hillocks are given in Mr Fryer's map on the north side of the Cumberland group, yet that moraines exist near Church, in the valley which forms the outlet of the Ennerdale Water ; also between Crummock Water and Lorton, and near Isle in the valley by which the Derwent descends from Bassenthwaite Lake towards Cockermouth. Near the centre of the lake district are extensive moraines, on the shoulder of Braw Top, immediately south-east of Keswick ; and Dr Buckland states that they must have been medial moraines, formed at the junction of the valley of the Greta with that of Derwentwater. The author was prevented from seeking for polished and striated surfaces on the rocks of Cumberland, but he noticed them on greywacke in Dr Arnold's garden at Fashow, near Ambleside ; and near the slate quarry at Rydal ; also on recently exposed rocks by the side of the road ascending from Grassmere to the pass of Wythburn. The rounded and mammillated forms of many of the rocks at the bottom of the valley descending from Helvellyn, by Rydal and Ambleside to Windermere, he states, are also identical with those which come beneath the glaciers of Switzerland. The remarkable Criffel granite boulders, between Carlisle and Cockermouth, Dr B. is of opinion, were transported across the Solway on ice. The author then proceeds to describe the phenomena of the distribution of the well-known Shap granite boulders, in the valleys which lead down from the Fell, northwards towards Shap and Penrith, and southwards towards Kendal and Morecombe Bay; and on the high table-land of Stainmore Forest to the east, as well as in the same direction, in the valley of the Tees, from Lartington and Barnard Castle to Darlington. It is impossible, Dr Buckland observes, to explain satisfactorily the dispersion of these boulders northwards, southwards, and easterly, by a great diluvial current, and still more so their transport from the valley of the Eden, over the lofty summits of Stainmore Forest, into the valley of the Tees. The glacier theory, he states, offers, on the contrary, a solution of the difficulties. A glacier descending to the north from Shap Fell would convey the blocks to the village of Shap, and distribute them throughout the intervening space; another glacier ranging to the south would deposit the boulders on the hills and in the valleys descending by Highborrow bridge to Kendal; whilst a third great glacier, proceeding eastwards betwixt Crosby, Ravensworth, and Orton, would traverse transversely the upper part of the valley of the Eden, near Brough, and accumulate piles of ice against the opposite escarpment, until they overtopped its lowest depression in Stainmore Forest, and disgorged their moraines into the valleys of the Greta and the Tees. Of the existence of this glacier there are abundant proofs in large mud moraines, loaded with blocks of granite in the upper portions, over which the road passes in the ascent to the gorge between Shap Fell, and Birbeck Fell, and the rocks on both sides of this gorge are perceived in an east and west direction, striated and mammillated, especially the granite on the northern side. Dr Buckland also shews that there are other conditions in the physical structure of the district to facilitate the accumulation of glaciers, as the pressure of lateral mountains more lofty than those immediately contiguous to the longitudnal valley into which the glacier is supposed to have descended; and he concludes by referring to the results at which Professor Agassiz arrived during an independent examination of the Shap Fell district, and which results completely accord with those given by Dr Buckland in this paper.

Description of several New or Rare Plants which have lately Flowered in the Neighbourhood of Edinburgh, and chiefly in the Royal Botanic Garden. By Dr Graham, Professor of Botany.

10*th Dec.* 1840.

Begonia Dregii. *Link, Klot. & Otto.*

B. *Dregii*, caulescens, ramosa, glabra; radice tuberosa; foliis transverse rhomboideis, duplicato-crenatis, supra argenteo-maculatis, subtus rubris; pedunculis bifloris; flor. masculo perianthii segmentis duobus, fœm. segmentis sex; capsulæ alis duabus majoribus obtusangulis, una rotundata.

Begonia Dregii, *Otto*, MS.

Description.—Whole plant glabrous. *Root* tuberous, tuber flattened. *Stem* (in the specimen described 6 inches high) erect, succulent, glabrous, pale red faintly streaked with greenish-white oblong spots, many rising from the crown of the root, branched. *Leaves* (1½ inch long, 2 inches across) petioled, oblique, transversely elliptico-rhomboid, subpeltate, 5–9-veined, glabrous on both sides, green, with unequal silvery spots above, red below, darker on the nerves and their branches, doubly crenate; petioles spreading horizontally, twice as long as the leaves, having a shallow channel on the upper side. *Stipules* large, obliquely ovate, colourless, reflected on the sides, marcescent. *Peduncles* axillary, about as long as the petioles, spreading, having at the apex two opposite *bracts*, similar to the stipules, but rather smaller, more round, and somewhat unequal. *Flowers* (1 inch across) white, two arising between the bracts, one male, the other female, pedicellate, expanding about the same time; *pedicels* unequal, that of the male flower the longer, and nearly equal to the length of the peduncle. *Male flower* dipetalous, the petals subrotund, flat, slightly unequal. *Stamens* united by their filaments only at the base; connective short, broad, the two anther-cells forming lines along its edges, and of rather paler yellow than it. *Female flower* 6-petalous, petals undulate, blunt, elliptical, two opposite narrower than the others, which are subequal, styles broad, fan-shaped, undulate, revolute and twisted, bearing along the terminal edge the villous stigmata, which are of darker yellow than the styles; germen with two subequal bluntly pointed wings, which are larger than the third more rounded one.

Seeds of this plant were obtained from M. Otto, Berlin, in April 1840, with the MS. name here adopted; and I have since learned from M. Klotzsch that seeds and dried specimens were transmitted from the Cape of Good Hope to the Botanic Garden at Berlin by M. Dregè. I regret that I do not know at what distance from the Cape they were gathered, because this is the first species of the genus which has been detected on the continent of Africa, and the first any where to the southward of the tropic, though several species are now known to be native without the tropic in the northern hemisphere.

Portlandia daphnoides, *Grah.*

Portlandia *daphnoides*; foliis ovato-ellipticis, acutiusculis, subtus concavis; floribus axillaribus, sexies latitudine sua, folia triplo, longioribus; lobis calycis lineare-subulatis, base dilatatis; pistillo staminibusque inclusis.

Description.—*Shrub* (in specimen described 1 foot 3 inches high) erect. *Leaves* (3 inches long, 1¼ broad) crowded near the top, greatly resembling those of *Daphne laureola*, coriaceous, dark green above, pale, concave below, obovato-elliptical, slightly pointed, entire, central rib and oblique and scarcely divided veins prominent below, petioles very short. *Flowers* pedicellate, axillary, solitary. *Calyx* 5-partite, green, segments subulato-linear, spreading, half as long as the peduncle; corolla (9 inches long, 1¼ broad) white, pendulous, funnel-shaped, glandular without pubescence; tube in its lower half very slender, fleshy, and sharply 5-angled, in the upper half inflated, subcylindrical, and having five ribs leading from the angles of the lower half to the sinuses between the broad rounded lobes of the slightly spreading limb. *Stamens* 5, reaching to within an inch of the sinuses of the limb, from their origin at the base of the corolla; filaments hard and wiry, glabrous; anthers (1½ inch long) linear, bursting along the sides; pollen granules minute and spherical. *Pistil* as long as the stamens; stigma flat, passing over the vertex of the style, and tapering downwards on two sides for a quarter of an inch; style wiry, glabrous, colourless; germen obovate, green, with five blunt angles, inferior, and having at the apex five blunt lobes, which rise above the calyx, and surround the base of the style; ovules very numerous, on a central clavate placenta.

This plant was received at the Royal Botanic Garden, Edinburgh, from Cuba, through the kindness of the Conde de Fernandina in 1838, has been kept in the stove, and flowered for the first time in August 1840. It certainly nearly approaches to *Portlandia grandiflora*, but it seems to me specifically distinguished by the less prominent ribs of the upper part of the flower, by the included stamens and pistil, by the very different form of the calyx segments, by the more coriaceous somewhat differently formed leaf, more acuminate stipules, and, I would fain hope, by its flowering more freely; for, notwithstanding what is said in the Bot. Mag. of the easy culture and free flowering of the *P. grandiflora*, it is universally acknowledged among cultivators that no plant is with greater difficulty kept in good condition, and scarcely any flowers less frequently. A little more experience of our present plant may shew it to have the same fault in cultivation, for its habit is very much like the other.

Physianthus auricomus, *Grah.*

P. *auricomus*; caule volubile, piloso; foliis obovatis, acuminatis, basi cordatis; floribus umbellatis, pedunculo petiolis multo longiore; stigmate inappendiculato.

Gardner's Specimens, No. 1757.

Description.—*Stem* woody, with milky juice, twining, densely covered with spreading harsh yellow hairs. *Leaves* (3–4½ inches long, 2–2¾ inches broad) petioled, obovate, acuminate, cordate at the greatly narrowed base, hairy on both sides, the hairs on the middle rib being longer than the rest, and like those on the stem, entire, undulate, rather paler below than above; petiole about one-fifth part of the length of the leaf, channelled above, spreading, very hairy on the back. *Peduncles* lateral in reference to the petioles, half as long as, or equal to, the length of the leaf, hairy, umbellate, flowers expanding in succession; *bracts* involucrate, ovato-lanceolate, acuminate, deciduous; *pedicels* about one-third of the length of the peduncle, less hairy. *Calyx* 5-partite; segments ovato-lanceolate, acuminate, slightly hairy, connivent, veined. *Corolla* (1¼ inch long, 1½ inch across) perfumed, white, somewhat fleshy, funnel-shaped, having a very few erect hairs near the throat, and everywhere else glabrous; tube longer than the calyx, swollen below, and having five gibbosities alternating with the segments of the calyx, scarcely angled above, faintly marked with greenish veins on the outside; limb 5-parted, segments ovate, spreading, and reflected. *Crown* of 5 linear-oblong green flat fleshy segments, erect in the tube of the corolla, to which they are adpressed, in their lower half adherent by their backs, and alternate with its gibbosities, shorter than the tube. *Sta-*

mens monadelphous, opposite to the lobes of the crown, and subsessile upon a fleshy mass on the inside of the base of each of these, each terminated by an ovate thin and colourless membranous appendage, which is spread upon the side of the stigma alternately with small dark purple rhomboid glands, which are split vertically on the outer side, and have suspended from them, by short straight arms, two flattened elliptical yellow pollen-masses, which are lodged in cavities on the inside of the base of the membranous appendages. Each stamen has two yellow cartilaginous spurs, involute in the edges, and projecting downwards by the sides of the short stout herbaceous filament, into cavities alternating with the fleshy masses on the inside of the segments of the crown, from which the stamens arise, so that in each cavity there is a spur from two adjoining stamens, and as the glands are immediately above these spurs, the pollen-masses from each belong to two stamens. *Stigma* large, white, angled upon the sides from the indentation of the stamens, rounded on the top, without any appendages. *Styles* 2, short, erect, parallel, yellow. *Ovules* very numerous, slender, attached to large receptacles from the inside of the germen.

I first saw this handsome climber extending across the rafters from end to end of a stove in the garden at Hales, near Liverpool, the seat of ——— Blackburn, Esq. in October 1837. It was covered with blossom, each flower remaining long in perfection. I could not ascertain from whence it had been imported, but it is certainly identical with Gardner's wild specimens collected in the province of Ceara in Brazil. A cutting from the Hales plant flowered in the Botanic Garden, Edinburgh, in October 1840, but it will never be seen in the perfection I found it in at Hales, unless, as there, it be planted in a border under glass, and not kept in a pot.

Stylidium Drummondii.

S. *Drummondii;* foliis omnibus radicalibus cæspitosis, lanceolato-linearibus, acuminatis, undulatis, marginibus reflexis, coriaceis, utrinque nudis nitidis, base squamis elongatis vaginatis, scapo folia bis superante, glanduloso-pubescente, paniculato; calyce bilabiato, labio superiore 3-fido, inferiore 2-partito; corolla fauce coronata.

Description.—*Leaves* (4–8 inches long, 3 lines broad) all radical, lanceolato-linear, attenuated and rigid at the apex, coriaceous, glabrous and shining on both sides, undulate, edges revolute, middle rib prominent both above and below, collected into fascicles, and sheathed at the base by elongated red scales. *Scape* twice as long as the leaves, erect, round, glanduloso-pubescent, green, bearing upon its apex a large ovate panicle, of which the branches are glanduloso-pubescent, ascending, the lower the longer, each springing from the axil of a lanceolato-subulate glanduloso-pubescent bract, and having a similar bract at each subdivision. *Flowers* large and handsome, the terminal one of each branch, somewhat irregularly upwards, expands first, and also the terminal one of each subdivision. *Calyx* green, glanduloso-pubescent; tube adherent, twisted, elliptico-ovate; limb bilabiate, the lips placed laterally, the upper 3-fid, the lower 2-partite, the segments linear-elliptical, as long as the tube. *Corolla* (fully 1 inch broad from above downwards, 1 inch across the middle of the upper, three-fourths of an inch across the middle of the lower segments, and half an inch across the faux) very handsome; limb yellow on the outside, and brownish within before full expansion, afterwards nearly white on the outside over the whole of which surface it is glanduloso-pubescent, and bright but delicate lilac on the inside, where it is glabrous, having four segments diverging in form of a St Andrew's cross, elliptical, undulate and sinuated, the two lower segments being rather the smaller, and more nearly obovate; fifth lobe minute, reflected upon the tube, obovate, colourless and shining in the centre, having a glandular reddish-lilac border broadest at the apex, and two slender teeth curved upwards at its base; faux crowned with two erect scimitar-shaped appendages, hairy, and about one-third of the length of the upper segments at the base of which they are placed, and two small slender diverging bipartite teeth at the base of the lower segments, the former being tipped with lilac, though white below, the latter every where colourless; tube colourless, twisted. *Column* flattened, slightly dilated in the middle, where it is reflected over the fifth lobe of the corolla, above this flexure slightly coloured lilac for a little way in front, near the top pale greenish-yellow every where else colourless. *Anther-lobes* elliptical, deflected, the lobes of each being placed end to end in the direction of the column, along the mesial line before expansion brownish, pollen yellow. *Stigma* green. *Germen* bilocular in the lower half, septum imperfect in the upper. *Receptacle* central, ovules numerous.

This most beautiful species, perhaps the most desirable in cultivation, not only on account of the great size of its flowers, but because each blossom remains expanded during several weeks, and these coming in succession, keep the plant in perfection for a time, the length of which we do not yet know, was raised from seeds transmitted from Swan River to Mr Low of Clapton by Mr Drummond. This gentleman has lately added so greatly to the plants now in cultivation in Britain from this interesting colony, that I take delight in paying him the only compliment in my power, and dedicating to him what is by much the most striking of these which I have yet seen in a living state. A plant was kindly sent to the Botanic Garden by Mr Low in October 1839, and expanded its first flowers in the greenhouse early in November 1840.

Proceedings of the Royal Society of Edinburgh.

1840, *February* 17.—Dr Hope, V. P, in the Chair. The following Communications were read :—

1. On the Cosmogony described in the Sixth Eclogue of Virgil, and on its relation to the theories of Modern Geology. By the Venerable Archdeacon Williams.

2. A brief notice relative to an Aërolite which was seen to fall near Juvinas, in the Department of the Ardêche in France, on the 15th June 1821. By Professor Forbes.

March 2.—Sir T. M. Brisbane, Bart., G. C. B., Pres., in the Chair. The following Communications were read :—

1. On the Persian mode of making Malleable Iron direct from the Ore, by James Robertson, Esq. Communicated by Robert Bald, Esq.

2. On the Fatal Effects of Air drawn into the Veins during Surgical Operations on the Neck and Shoulder. By Sir Charles Bell, K. H.

March 16.—Right Hon. Lord Greenock, V. P., in the Chair. The following Communications were read :—

1. On the Sulphuret of Cadmium, a New Mineral (first observed by Lord Greenock). By Arthur Connell, Esq.

2. On an Optical Illusion giving the idea of an Inversion of Perspective in viewing objects through a Telescope. By Professor Forbes.

3. Collection of African Shells. Presented by J. O. MacWilliam, Esq., Surgeon R. N.

4. A collection of Fossil Shells, from the great deposit near Uddevalla, in Sweden, presented.

April 6.—Sir T. M. Brisbane, Bart., G. C. B., Pres., in the Chair. The following Communications were read :—

1. Notice regarding the Growth of Plants in Close Glazed Cases. By Allan Maconochie, Esq.

2. Results of Additional Experiments on Terrestrial Magnetism. By Professor Forbes.

April 20.—Sir T. M. Brisbane, Bart., G. C. B., Pres., in the Chair. The following Communications were made :—

1. " Sir Charles Bell requested leave to withdraw his paper *on the fatal effects of air admitted into the veins of the neck during surgical operations,* in order to make additions.

2. On the Origin and Progress of Grecian Sculpture. By Dr Traill. Part I.

3. Account of Earthquakes felt in Scotland during the Autumn and Winter of 1839. By David Milne, Esq.

" I. The first part of the paper described the number and intensity of the shocks felt between the 3d October 1839 and 13th April 1840, the point (situated near Comrie) from which they emanated, and the distance to which they extended.

" II. The next part of the paper was occupied with an account of the effects, both physical and moral, produced by the shocks. Under the first class were described the different kinds of injury done to walls, according to their direction, and the nature of the ground on which they were built. Under the second were mentioned the alarm felt by the inhabitants of Strathearn during the great shock of 23d October, as also the quick preception of it by the lower animals, and the terror they evinced at it.

" III. The nature of the movement of the earth's surface which caused the foregoing effects was next noticed, and data were given illustrating the form of the undulation which was produced. The noises also that accompanied the shocks were fully described, and the probable cause of them stated.

"Various other concomitant phenomena of an unusual character were noticed, such as the evolution of electricity, the diffusion throughout the atmosphere of something which caused a smell, variously described as 'metallic,' 'sulphureous,' and like the 'washings of guns;' as also the appearance in various parts of Strathearn of a fine black powder, which appeared to consist chiefly of carbonaceous, and partly of siliceous and calcareous matter.

"IV. The last part of the paper was devoted to remarks on the way in which the undulation of the earth's surface was caused, and the circumstances which may have produced the earthquakes.

"In reference to the first point, various reasons were assigned why the phenomena were more intelligible on the supposition that they were caused by vibrations transmitted through the solid crust of the earth than on the theory supported by many, that they were caused by undulations in the body of molten matter on which the earth's crust may be resting. These vibrations were probably caused by ruptures or explosions at a considerable depth beneath the earth's surface.

"In regard to the cause of these ruptures, it was observed that the hills in the immediate neighbourhood of Strathearn are chiefly primitive, and of igneous origin, and that there are numerous greenstone and basaltic dykes, indicating renewed volcanic action at subsequent periods. There are apparently extensive fissures in the earth's crust in this part of Perthshire. It was also observed that, during the month previous to the occurrence of the earthquake, the atmospheric pressure had been greatly less than usual; and that the quantity of rain which fell was almost unprecedented in the central parts of Scotland. There appeared to be some connection between the state of the atmosphere in both these respects and the occurrence of the earthquakes, judging from some observations made last autumn, but more particularly from the frequent coincidence of shocks in former years with a similar state of the atmosphere. Some views were offered as to the influence which these circumstances might have in giving rise to the earthquakes.

"Notice was taken of shocks which had been felt in other parts of Europe, at the time that shocks occurred in Scotland."

Proceedings of the Wernerian Natural History Society.

(Continued from Vol. xxix. p. 177.)

The thirty-fourth Session commenced on the 14*th November* 1840, Professor Jameson, P., in the Chair. The following office-bearers were elected for 1841:—

President.

Robert Jameson, Esq. F.R.SS.L. & E., Professor of Natural History in the University of Edinburgh.

Vice-Presidents.

Dr Walter Adam. W. A. Cadell, Esq. F.R.SS.L. & E.

Dr T. S. Traill, F.R.S.E. Dr Robert Hamilton, F.R.S.E.

Sec.—Dr PAT. NEILL, F.R.S.E.	*Assist.-Lib.*—R. J. H. CUNNINGHAM, Esq.
Assist.-Sec.—T. J. TORRIE, Esq. F.R.S.E.	*Painter.*—P. SYME, Esq.
Treas.—A. G. ELLIS, Esq.	*Assist.*—W. H. TOWNSEND, Esq.
Lib.—JAMES WILSON, Esq. F.R.S.E.	

Council.

Dr ROBERT GRAHAM, F.R.S.E.	EDWARD FORBES, Esq.
Sir WM. NEWBIGGING, F.R.S.E.	ROBERT STEVENSON, Esq. F.R.S.E.
DAVID FALCONAR, Esq.	DAVID MILNE, Esq. F.R.S.E.
Dr ROBERT PATERSON.	JOHN STARK, Esq. F.R.S.E.

Two papers were read. The first was an account of the Gold Tract of the Southern Mahratta country, by Lieutenant Newbold, Aide-de-camp to General Wilson. The author gave an interesting history of the discovery of gold in the various localities where it has been obtained in that region; and communicated geognostical observations on the mode of its occurrence, from which it appears that its position is in veins and disseminated grains in primitive slates, and that it is found most frequently near their line of junction with granites or trap-rocks. The other paper was by the Rev. John Toplis, B.D., of South Walsham, Norfolk, and was entitled Remarks on Hydrostatical Pressure, as an occasional cause of Earthquakes (published in the present No. of Journal, p. 84).

A number of donations, received for the Society during the recess, were then announced, and placed on the table; in particular, a rich collection of Scottish Shells, by the Rev. David Landsborough of Stevenston.

November 28.—Dr Traill, V. P., in the Chair. Mr Edward Forbes read the first part of his Remarks on the Classification of the Mollusca, of which the following is an abstract:—The author stated that the systematic views at present adopted regarding the mollusca are for the most part empirical and wanting in philosophical precision, and that he now attempted to remedy confusion by framing an hypothesis of the classification of the mollusca from the view of their position in the animal series, and of their relation to the other great classes. Assuming five animal types, of which the Amorphozoa, the Radiata, the Mollusca, the Annulata, and the Vertebrata, are the representatives, each of these typical classes is supposed to pass through one or more parallels corresponding to those types, and in its passage to exhibit formal and structural analogies with such animals of the other types as may be placed in the same parallel. The parallels are termed those of Amorphism, Radiism, Mollism, Annulism, and Vertebrism. The typical class Mollusca is supposed to pass through four of the parallels, its lowest forms being radiated (as the compound tunicata), its highest vertebrated (as the cephalopoda). It was proposed to ascertain the true relations of the classes and orders of the mollusca, by observing their relative position in the passage of the type through the various parallels. It was also maintained that the two spheres, animal and vegetative, are represented throughout the typical classes, and that, whenever two groups of any ranks are structurally and formally equal, one will be found to represent the animal sphere, and one the vegetative. To ascertain the true sources of character on which to base the various groups of the mollusca, that type must be viewed *generally;* 1st, in regard to the whole animal kingdom; and, 2d, *per se* as typical of some great function. In the first point of view, they represent the progression towards a cerebral concentration of the nervous system, and accordingly the highest divisions of the type must be derived from that system; in the second, they represent the function of respiration, since they exhibit all the modifications of the respiratory system, and from that system must be derived the ordinal divisions. The shell of the true mollusca is a modification of the dermato-skeleton for the express protection of the respiratory organs, and accordingly it plays an important part in the true arrangement of the order. The shell of the cephalous mollusks is of a very different nature, and owes its form and structure to the position of the shelled cephala in the parallel of annulism, *it being an articulate shell, representing the testaceous covering of the foramenifera.*

The hypothetical arrangement of the classes and orders of the mollusca resulting from this view may be expressed as follows:—

MOLLUSCA.

Classical Number.			Ordinal Number
1	Cephala,	. . . .	1
		Pulmonifera,	1
1	Paracephala,	Phyllobranchiata,	1
		Pectinibranchiata,	1
		Lamellibranchiata,	1
1	Acephala,	Palliobranchiata,	1
		Heterobranchiata,	1
3			7

There was then read a notice by Mr John Balfour of Toronto, Upper Canada, on the so-called Sulphur Showers of the Canadas and United States, and a specimen was exhibited. Dr Neill read a communication from Mr MacGillivray, stating that the *Pink-footed* or *Short-billed Goose, Anser brachyrhynchus,* occurs occasionally on the stalls of our poultry market, and is generally mistaken for the *Bean Goose, Anser segetum.* That species, however, is distinguished by very marked characters, and is the most common with our poulterers; the true Grey Lag, *Anser palustris,* of Ray and Dr Fleming, being of rare occurrence. It is but very recently that the short-billed goose has been clearly distinguished; and this, together with other occurrences of a similar nature, ought to shew that such characters as 'head and neck brown, upper parts grey, lower paler, tail of sixteen feathers,' which are common to several species, are not sufficient to enable one to determine the species of a bird that he may have procured. The peculiar habits of the present goose are as yet little known, although it has recently been found in France and England; Mr Bartlett in the latter country having described it under the name of *Anser phœnicopus*, the *Pink-footed Goose*, although it was previously distinguished by a French naturalist, and is also described in the fourth volume of M. Temminck's Manuel d'Ornithologie. A fine specimen of the pink-footed goose was exhibited.

Mr Trevelyan exhibited some curiously-fractured quartz-pebbles from the old red sandstone conglomerate of Auchmithie, on the coast of Forfarshire; and also some specimen plates of Mr Boscawen Ibbetson's application of the Daguerreotype to the representation of fossil organic remains.

December 12.—Dr Traill, V.P., in the chair. The Assistant-Secretary read notes of an expedition to the Sutledge and in the Himmalehs, by William Jameson, Esq. Assistant-Surgeon, H.E.I.C.S. During his tour, the author visited many of the localities from which the collections made by Baker, Cautley, Falconar, Colvin, Clerk, and Macleod, had been obtained, and also some new points where fossils are found. He states, that there is evidence of the occurrence of these remains throughout the whole Sivalick range which extends between the Jumna and Sutledge, or over a tract of country upwards of ninety miles in length. The sandstone containing the organic remains is inclined at a considerable angle, and the author is of opinion that volcanic action must have operated in the district posteriorly to the destruction of the animals whose remains are now met with in a fossil state. The following are the genera hitherto discovered in a fossil state in the Sivalick range :—Simia, Ursus, Canis, Hyæna, Felis, Gulo, Mus, Hystrix, Elephas, Mastodon, Hippopotamus, Sus, Palæotherium, Anthracotherium (Chaerotherium, Falc. & Caut.), Rhinoceros, Equus, Sivatherium, Camelus, Cervus, Antilope, Bos, Gaviala, Crocodilus, Emys, Trionyx, Ardea. There are species living at the present day of all the genera found, except four.

Mr John Goodsir read an account of certain peculiarities in the structure of the Short Sun-Fish, *Orthagoriscus Mola* (published in this No. of the Journal, p. 189).—A memoir by Dr John Richardson was then read, on the Frozen Soil of North America (also published in this No., p. 110).—Dr Traill exhibited specimens of Witherite or Carbonate of Baryta from a new locality in Wales, and made remarks on the mine from which it is extracted. This mine is close to the right side of the public road leading from St Asaph to Holywell, about three miles from the former town, among the clay-slate mountains that form the eastern boundary of the fertile valley of Clwyd, and near the junction of the slate with the mountain limestone. It is in the mining district of Rhualt, and the mine where it is chiefly found is named *the Pennant Mine.* The entrance to the veins containing it is by an adit near the road, which serves as a drain to the workings in the upper part of the hill, with an inclination from its upper part to its exit of 25 feet.

The carbonate is found in veins along the adit, and descending below it, and is mixed with sulphate of baryta, and some galena, but the principal product of this part of the workings is the carbonate, which occurs in such quantity as to become an object of mining industry.

It is sold, like the sulphate of the same earth raised in the upper part of the vale of Clwyd, the Isle of Arran, and Renfrewshire, for the avowed purpose of adulterating white paint, and is more difficult of detection than the other adulteration, because it effervesces with, and is soluble in, muriatic or hydrochloric acid. It is also sold on a more limited scale for chemical purposes. We have now, then, in Southern Britain, five great localities of Carbonate of Baryta—Anglezark in Lancashire, Aldstone Moor in Cumberland, Arkingarthdale or Arkindale in Yorkshire, Snailbeach in Shropshire, and Rhualt in Flintshire.

SCIENTIFIC INTELLIGENCE.

1. *Fossil Fishes of Orkney.*—On Monday, 21st December, Dr Traill exhibited to the Royal Society specimens of all the fossil fishes collected by him from the old red sandstone formation of the Orkney Islands.

A part of this collection had been submitted to the Geological Section of the British Association at Edinburgh, in 1834, when they were examined by M. Agassiz, who not only found among them new species, but new genera. Among them, he saw, for the first time, the genus *Diplopterus*, which he has slightly noticed in his work. The characters of this genus are, "Two anal, equal and opposite to two dorsal, fins; tail with nearly equal and even lobes; mouth large, and furnished with large conical teeth." The size of the mouth and form of the tail distinguish it from Dipterus, which has a small head, and a deeply bifid tail; and the possession of two equal and opposite dorsal and anal fins distinguishes it no less from Osteolepis and Palæoniscus.

The species from which M. Agassiz established the genus has not yet been published; and as another species of Diplopterus has lately been discovered in Scotland, and also, it is said, in Ireland, Dr Traill, assuming the privilege usually allowed to the finder of a new species, and desirous of connecting with his country the name of the celebrated naturalist who has done so much to illustrate its fossil ichthyology, proposes to mark this Orkney species as Diplopterus Agassis, a designation under which it has been already sent to different geological collections. The number of fossil fishes already noticed by M. Agassiz as found in Orkney, are,—

1. Osteolepis macrolepidotus.
2. ——— microlepidotus.
3. Cheirolepis Traillii.
4. Cheiracanthus minor.
5. Diplopterus Agassis.
6. Diplocanthus crassissimus.
7. Coccosteus latus.
8. Dipterus macrolepidotus.
9. Platygnathus paucidens.
10. Pterichtys Milleri?

2. *Notice of the Publications of Professor Agassiz.*—During the recent visit of Professor Agassiz to this country, we had an opportunity of seeing the newest parts of the various important works now in progress by that distinguished and indefatigable naturalist, and we think it may be useful to our readers to notice the exact state of advancement in which they severally are at present.

Of his *Recherches sur les Poissons Fossiles* the fourteenth part is now ready, and the fifteenth, which will terminate the original work, will soon appear. Owing to the abundance and novelty of the materials lately found by Professor Agassiz in Britain, he proposes publishing a supplement. The first part has appeared of the very beautiful "*Histoire Naturelle des Poissons d'eau douce de l'Europe Centrale*," containing the *Salmo* and *Thymallus;* and the second part is ready, and will be speedily published, containing descriptions and plates illustrative of the development of the *Salmonidæ.* The first of the "*Monographies d'Echinodermes vivans et fossiles*," containing the "*Salenies*," has been published; the second, containing the "*Scutelles*," and the third, containing the "*Galerites*," are nearly ready. The first and second parts of the "*Description des Echinodermes fossiles de la Suisse*,,' containing the "*Spatangoides*," "*Clypéastroides*," and the "*Cidarites*," are already before the public; the third and concluding part, containing the "*Asteries*" and "*Crinoides*," will appear about the end of 1841. The first part has appeared of the "*Etudes Critiques sur les Mollusques fossiles*," containing the "*Trigonies*." Professor Agassiz has also published a Memoir, explanatory of the casts of molluscous animals prepared, under his direction, at Neuchatel.

List of Patents granted for Scotland from 24*th September to* 22*d December* 1840.

1. To John Lambert of No. 12 Coventry Street, in the parish of Saint James, within the liberty of the city of Westminster, gentleman, being a communication from abroad, "certain improvements in the manufacture of soap."—24th September 1840.

2. To James Buchanan, merchant in Glasgow, "certain improvements in the machinery applicable to the preparing, twisting, and spinning, and also in the mode of preparing, twisting, and spinning of hemp, flax, and other fibrous substances, and certain improvements in the mode of applying tar or other preservative to rope and other yarns."—24th September 1840.

3. To Alexander Francis Campbell of Great Plumstead, in the county of Norfolk, Esquire, and Charles White of the city of Norwich, mechanic, "improvements in ploughs and certain other agricultural implements."—29th September 1840.

4. To Amand de Planque of Lisle, in the kingdom of France, but now residing at 126 Regent Street, in the county of Middlesex, gentleman, being a communication from abroad, "improvements in looms for weaving."—29th September 1840.

5. To George Delianson Clark of the Strand, in the county of Middlesex, gentleman, being a communication from abroad, "improvements in coke ovens."—5th October 1840.

6. To Richard Beard of Egremont Place, New Road, in the county of Middlesex, gentleman, being a communication from abroad, "improvements in printing calicoes and other fabrics."—7th October 1840.

7. To Robert Beart of Godmanchester, in the county of Huntingdon, miller, "improvements in apparatus for filtering fluids."—14th October 1840.

8. To Thomas Farmer of Grumesbury House, near Acton, in the county of Middlesex, Esquire, "improvements in treating pyrites and other matters to obtain sulphur, sulphurous acid, and other products."—14th October 1840.

9. To Thomas Smedley of Holywell, Flintshire, gentleman, "certain improvements in the manufacture of tubes, pipes, and cylinders."—27th October 1840.

10. To George Hickes of Manchester, agent, "an improved machine for cleaning or freeing wool and other fibrous materials of burs and other substances."—27th October 1840.

11. To Miles Berry of the office for patents, 66 Chancery Lane, in the county of Middlesex, patent-agent, being a communication from abroad, "certain improvements in the arrangement, construction, and mode of applying certain apparatus for propelling ships and other vessels."—29th October 1840.

12. To Edmund Rudge junior, of Tewkesbury in the county of Gloucester, tanner, "a new method or methods of obtaining power for locomotive and other purposes, and of applying the same."—2d November 1840.

13. To Benjamin Hick junior, of Bolton-le-Moors in the county of Lancaster, engineer, "certain improvements in regulators or governors, for regulating or adjusting the speed of rotary motion of steam-wheels, water-wheels, and other machinery."—3d November 1840.

14. To John Condie, manager of the Blair Iron Works in the parish of Dalry in the county of Ayr in Scotland, "improvements in applying springs to locomotive and railway and other carriages."—4th November 1840.

15. To Luke Hebert of Birmingham in the county of Warwick, solicitor of patents, being a communication from abroad, "improvements in the ma-

nufacture of cofered spades and shovels, soughing and grafting tools."—4th November 1840.

16. To Arthur Wall of Bermondsey Wall in the county of Surrey, surgeon, " a new composition for the prevention of corrosion in metals and for other purposes."—5th November 1840.

17. To James Heywood Whitehead of the Royal George Mills in Saddleworth in the county of York, manufacturer, " improvements in the manufacture of woollen belts, bands, or driving-straps."—6th November 1840.

18. To Samuel Wilkes of Darleston in the county of Suffolk, iron-founder, " improvements in the manufacture of vices."—6th November 1840.

19. To Joseph Bennett of Turnlee near Glossop in the county of Derby, cotton-spinner, " certain improvements in machines for cutting rags, ropes, waste, hay, straw, or other soft or fibrous substances usually subject to the operation of cutting or chopping, part of which improvements are applicable to the tearing, pulling in pieces, or opening of rags, ropes, or other tough materials."—9th November 1840.

20. To Charles Payne of South Lambeth in the county of Surrey, gentleman, " improvements in salting animal matters."—11th November 1840.

21. To Henry Hind Edwards of Nottingham Terrace, New Road, in the county of Middlesex, being a communication from abroad, " improvements in evaporation."—11th November 1840.

22. To Elijah Galloway of Manchester Street in the county of Middlesex, engineer, " improvements in propelling railroad carriages."—11th November 1840.

23. To Nathan Defries of Paddington Street in the county of Middlesex, engineer, " improvements in gas meters."—11th November 1840.

24. To Henry Houldsworth of Manchester in the county of Lancaster, cotton-spinner, " an improvement in carriages used for the conveyance of passengers on railways, and an improved seat appliable to such carriages and to other purposes."—11th November 1840.

25. To Joshua Whitworth of Manchester in the county of Lancaster, engineer, " certain improvements in machinery, or apparatus for cleaning and repairing roads or ways, and which machinery is also applicable to other purposes."—16th November 1840.

26. To Samuel Wilks of Darleston, iron-founder, " improvements in the manufacture of hinges."—17th November 1840.

27. To Thomas Horne of Birmingham, in the county of Warwick, brass-founder, " improvement in the manufacture of hinges."—18th November 1840.

28. To James Smith of Deanston Works, in the parish of Kilmadock, in the county of Perth, cotton-spinner, " improvements in the preparing, spinning, and weaving of cotton, silk, wool, and other fibrous substances, and in measuring and folding woven fabrics."—19th November 1840.

29. To Benjamin Winkles of Northampton Street, in the parish of Saint Mary, Islington, in the county of Middlesex, steel and copperplate manufacturer, " certain improvements in the arrangement and construction of paddle wheels and water wheels."—19th November 1840.

30. To Robert Hawthorn and William Hawthorn of Newcastle-upon-Tyne, civil engineers, " certain improvements in locomotive and other steam-engines in respect of the boilers, and the conveying of steam therefrom to the cylinders."—20th November 1840.

31. To Peter Bradshaw of Dean near Kimbolton, in the county of Bedford, gentleman, " improvements in dibbling and drilling corn, seeds, plants, roots and manure."—20th November 1840.

32. To John Buchanan of the city of Glasgow in Scotland, coach-build-

er, "certain improvements in wheel-carriages, whether for common roads or railways."—25th November 1840.

33. To James Molyneux Smith of Preston in the county of Lancaster, linen-draper, "an improved mode of dressing flax and tow."—26th Novem- 1840.

34. To Samuel Wagstaff Smith of Leamington, iron-founder, "improvements in apparatus for supplying and consuming gas."—26th November 1840.

35. To Frederick Theodore Philippi of Belfield Hall, near Rochdale in the county of Lancaster, calico-printer, "certain improvements in the art of printing cotton, wool and other woven fabrics."—30th November 1840.

36. To Alexander Dean and Evan Evans of Birmingham, in the county of Warwick, millwrights, "certain improvements in mills for reducing grain and other substances to a pulverized state, and in the apparatus for dressing or bolting pulverized substances."—8th December 1840.

37. To John Hawley of Frith Street, Soho Square, in the county of Middlesex, watchmaker, being a communication from abroad, "improvements in pianos and harps."—9th December 1840.

38. To Francis Molineux of Walbrook Buildings in the city of London, gentleman, "improvements in the manufacture of candles, and in the means of consuming tallow and other substances for the purpose of light."—9th December 1840.

39. To Joseph Leese junior of Manchester, in the county of Lancaster, calico-printer, "certain improvements in the art of printing calicoes and other surfaces."—11th December 1840.

40. To Phillipe Marie Moindron of New Ormond Street, in the county of Middlesex, merchant, being a communication from abroad, "improvements in the construction of furnaces and in boilers."—17th December 1840.

41. To John Cartwright of Loughborough in the county of Leicester, manufacturer of hosiery, Henry Warner of the same place, manufacturer of hosiery, and Joseph Haywood of the same place, frame-smith, "certain improvements upon machinery commonly called stocking-frames, or framework knitting machinery."—22d December 1840.

ERRATA IN PROFESSOR BISCHOF'S MEMOIRS.

[We insert for the use of those who are more particularly interested in the memoirs by Professor Bischof on springs, volcanos, earthquakes, &c., which have appeared in our Volumes from XX. to XXIX. inclusive, a carefully prepared list of errata of the whole series. The errors have chiefly arisen from the conversion from one scale to another of the extremely numerous thermometrical observations.]

Vol. XX. page 372, line 4 from the top, for *are grounds* read *are no grounds.*—Vol. XXIII p. 348, l. 15, for 34°.47, read 29°.23; l. 21, for 44°.57, read 12°.67; l. 22, for 41°.27, read 9°.27.—P. 349 l. 1, for 33°.01, read 1°.01.—P. 353, l. 34, for oldest, read coldest.—P. 365, l. 37, for 1000, read 10,000.—P. 366 in the analyses, the quantities of the chlorides of sodium and potassium, *together* are 0.00287 and 0.01488.—P. 387, l. 15, for 47.72, read 41.72; l. 23, for 9.72=58.62, read —41°.72 = 26°.62.—P. 389, l. 26, for 72°.50, read 8°.5.—P. 393, l. 20, for 35°.96, read 3°.96.—P. 394, l. 31, for 27°.50, read 38°.75.—Vol. XXIV. p. 149, l. 7 from bottom, for 0°.047, read 0°.106.—P. 151, l. 3 from top, for 0°.0267, read 8°.0601; l. 7, for 247, read 24°.7.—P. 161, l. 13, for 37°.37, read 5°.38; l. 14, for 36°.56, read 4°.56, and for 41°.94, read 9°.94; l. 16, for 4°.39 making, read 4°.38 making; l. 17, for 8°.78, read 8°.77.—P. 255, l. 25, for 15.°75, read 15°.3—P. 268, l. 17, for 6°.60, read 6°.57; l. 18, for 4°.61, read 5°.62.—P. 269, l. 1, for 36°.51, read 37°.51.—P. 277, No. 5, l. 1 from bottom, for 48°.65, read 47°65; No. 8, l. 7, for 54°.05, read 52°.25; No. 17, l. 3, for 46°.85, read 44°.5.—P. 281, l. 11 from bottom, for 52°.25, read 51°.44.—P. 282, l. 29 from bottom, for 0°.337, read 0°.56; 1, 28, for 0°.391, read 0°.596; l. 25, for 0°.225, read 0°.67.—P. 283, l. 20 from top, for 66°.70, read 67°.70; l. 25, for 52°.58 read 53°.47.—P. 288, l. 28, for 2°.25, 2°.65—4°. 4 F., read 2°.25; l. 29, for 122—6.7, read 115.—P. 290 l. 4, for 60°.23, read 60°44.—P. 292, l. 24, for 56°97, read 54°.7.—P. 297, No. 18, June *(Table)*, for 43°.57, read 43°.79; No. 28, February, for 48°.88, read 47°.88.—P. 298, No. 34, March, for 46°.03, read 47°.03, and difference for 7°.15, read 7°.38; No. 36, June, for 57°.45, read 54°.45; No. 42, March, for 48°.65, read 46°.4; No. 51, May, for 48°.65, read 46°.4; No. 54 November, for 59°.64, read 49°.64, and mean, for 58°.95, read 48°.99; No. 55, July, for 49°.01, read 48°.92.—P. 299, (erroneously paged 229), No. 59, for 43°.02, read 42°.42, and April, for 47°.97, read 47°.37; No. 63, February, for 34°.77, read 34°.47; No. 66, January, for 26°.7, read 28°.56, and December, for 47°.64, read 37°.64; No. 67, January, for 39°.32, read 40°.32; No. 69, September, for 55°.51, read 56°.41, No. 70, March, for 37°.58, read 38°.7.—Vol. XXVI. p. 360, l. 16, for *part*, read *heat.*

THE

EDINBURGH NEW

PHILOSOPHICAL JOURNAL.

Memoir of the Life and Writings of the late Professor Blumenbach of Göttingen. By Professor K. F. H. Marx.*

However lively and indelible may be our recollection of the individual who has been so lately taken from amongst us, it will, I hope, be allowed me to sketch an outline of his life and writings, and thus, as it were, to cast a flower on the grave of one who was honoured by us all, but who was peculiarly beloved by me.

It was granted to him to perform his public duties for a longer period than the usual span of human life, and to conduct the affairs of our society longer than any now present can carry back their recollections. With his memory and with his name are linked the most weighty events of this university for more than the last half century, nay, the progress of development of one of the most extensive and important branches of science is most closely interwoven with what he has undertaken, contributed, and promoted.

Of the whole series of those who laboured and investigated in the same path with himself, he stood at last like a solitary pillar, like a pyramid of antiquity, to serve as an exciting example to us, affording an instance of the course sometimes followed by Nature in impressing on mental power the seal of perfection, by means of the strength and duration of its external form.

John Frederick Blumenbach was born at Gotha on the 11th

* Read to the Royal Society of Sciences of Göttingen, 8th February 1840.

May 1752. His father, himself a zealous friend of geography and natural history, at an early period awakened a love for these subjects in his son. A manuscript note by Blumenbach will best convey an idea of some of the influences which operated on him under the paternal roof, and at his first entrance into the world. He says,—" My father was born in Leipsic, and died in 1787, protector and professor in the Gymnasium of Gotha; and to the acquirement of his philosophical attainments two individuals chiefly contributed, and thus directly influenced my education; these were the two professors of philosophy in Leipsic,—Menz and Christ. To the first he was indebted for his love of literary history and natural history, and to the latter for his taste for the fine arts and antiquity. I also found pleasure and enjoyment in these subjects; which are partly requisite, and partly at least not adverse, to the study of medicine, the pursuit selected by me from choice.

" In Jena, where I commenced my academic career, I found nourishment for literature and book-lore in the society of Baldinger, and for natural history and archæology in that of my relation J. E. Imm. Walch, professor of eloquence. When I proceeded to Göttingen to fill up some blanks in my medical studies, my former rector in Gotha, Ecclesiastical Councillor Geisler, gave me a letter to Heyne. When I delivered it, I at the same time shewed him an antique sealstone which, when at school, I had purchased from a goldsmith. Such a taste in a medical student seemed singular to him, and this little stone was the first cause of the subsequent varied and very intimate intercourse with that admirable man.

" There lived at Göttingen at that time a singular individual of extraordinarily varied acquirements, Professor Chr. W. Büttner, well known for his talents as a linguist, but who for many years had given no lectures, and was quite unknown to the students. Just at the period of my arrival, his friend and great admirer Michaelis, the orientalist, whose eldest son was then commencing the study of medicine, had persuaded him to give, if possible, a course of lectures on natural history, the subject on which he had formerly lectured, and of which he possessed a well-known collection. I was asked to attend, and, as I happened to have the hour unoccupied, I inscribed my name for the class, and thus had the opportunity of becoming acquainted with the singular but remarkable Büttner. His course was a mere *conversatorium*, in which, for weeks together, natural history was not mentioned. He had, however, selected the 12th edition of the Syst. Nat. as text-book, but the session was taken up with the discussion of such innumerable foreign subjects which he introduced, that we did not nearly get through the Mammalia. As he commenced with Man, a subject left untouched by Walch of Jena, and as he adduced illustrations from many voyages and travels, and representations of foreign nations, contained in his large library, I was incited to write my inaugural dissertation *De Generis Humani Varietate Nativa;* and the subsequent prosecution of this interesting topic gave rise to my anthropological collection, which in time became universally celebrated as unique of its kind.

"During this first winter the purchase of Büttner's collection of natural history and coins for the university was negociated by Heyne. As the objects of natural history were in a state of great disorder, an assistant became necessary to transfer and arrange them. Heyne therefore asked him, 'As you lecture on natural history, are you not acquainted with some one of your young people who would answer the purpose?' 'Yes,' replied Büttner, and named me. 'I also know him,' was the answer of Heyne; and so I gladly undertook gratuitously the duties of assistant in the arrangement of a collection which I found to be a rich one.

"Some time afterwards, when the whole had been transferred and placed for a time in what was formerly the medical class-room, the excellent minister and curator of the University, Von Lenthe, came here, examined our institution, and, as this collection was also to be inspected, and as the worthy Büttner did not seem inclined to shew it, I was hastily summoned, and succeeded so well, that, when going out, the minister took Heyne aside and said, 'We must retain that young man here.' After I passed my examinations in the autumn of 1775, I next winter gave my first lectures on natural history as private lecturer in the University, was in the same session (February 1776) named extraordinary, and in November 1778, ordinary professor."

The progress of Blumenbach in his scientific and civil career, after this auspicious commencement, is so fully in the recollection of all, that I need not enter into the details of his being admitted a member of this Society in 1784, his being appointed Councillor of State in 1788, Perpetual Secretary of the Physical and Mathematical Class of this Society in 1812, Member of the Library Commission in 1815, Knight of the Guelphic Order in 1816, Superior Medical Councillor in the same year, and a Commander of the Guelphic Order in 1821. It appears to me much more suitable to sketch here the different directions in science which he followed or communicated, his career as a teacher, his external relations, and, likewise, the chief features of his personal appearance and character.

It may first of all be said with justice of Blumenbach, that it was he especially who in Germany first extended the influence of natural history from the confined sphere of books and museums to the extensive and varied circle of life; that he rendered the results of serious and solitary investigation comprehensible and enjoyable to every educated person desirous of information, and that he more particularly understood the method of interesting the higher orders of society in his subject. By means of the comprehensive view he took of the whole range of scientific investigation, he well knew how to select what could awaken and excite the spirit of observation, and he was enabled to bring obscure subjects under a clear point of view, as well as to render what was useful practically available. This disposition and tact for generalization, this desire for popularising and making clear, did not at all interfere with the solidity of his information. He investigated with the most indefatigable zeal the most diversified portions of his science, and arrived at conclusions which threw light on the darkest portions of his subject.

Thoroughly provided with classical knowledge, sharpening and enriching his mental faculties by continued reading, and kept awake by intercourse with the noblest of his time, he was able not only continually to obtain new views of his favourite subjects, but also to communicate to these subjects a proper form of expression and representation. As, moreover, he regarded each result of his own investigations and those of others as a seed which might produce better and more enlarged conclusions, he exerted himself strenuously, by writing, conversation, and lecturing, to disseminate every one immediately, and to provide for it a productive soil. It thus followed that he was regarded as the pillar and representative of natural history, that he collected around him innumerable young people, and, by words as well as works, exercised the most decided influence on the whole study of that subject for a long series of years.

Blumenbach was already known to the Society of Sciences as a diligent student, for, at the meeting of the 15th January 1774, he communicated the, at that time, remarkable fact of his having (like Braun of Petersburg so early as 1759) succeeded in freezing mercury.*

In the year 1784 he became a member of our Society, and he soon read his paper on the eyes of the Albino and the movement of the iris.† It was a fortunate circumstance that his first literary labour related to the races of man, and that thus physical anthropology became, to speak mineralogically, the primitive form of his future labours. Hardly any other dissertation has gone through so many editions, and obtained for its author such general approbation as that entitled, *De Generis Humani Varietate Nativa.*‡ It was the origin of his subsequent gradual publication of the *Decaden* § on the forms of the skull of different races and nations, and was the foundation of his own collection.‖ To this unique accumulation of

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* Götting. gel. Anzeigen, 1774. Part 13, p. 105–7. Blumenbach himself placed no value on this experiment; he even supposed that his friends assumed too rashly the fact as ascertained.

† De Oculis Leucæthiopum et iridis motu. In Comment. Soc. R. Gött. vol. vii. p. 29–62.

‡ Published first of all in 1775.

§ The first *Decas collectionis suæ craniorum diversarum gentium illustrata* appeared in 1790 in the tenth volume of the Commentat. Soc. Sc. The last, under the title, *Nova Pentas collectionis suæ craniorum diversarum gentium tanquam complementum priorum decadum exhibita in consessu societatis, 8th July* 1826, and appeared in the sixth volume of the Commentat. recentior. p. 141–48. Also Gött. gel. Anz. 1826. No. 121, p. 1201–6.

‖ Compare his treatise on Anthropological Collections in the second ediits kind, as to every thing that characterizes the external form and the structure of the head in man, both princes and learned men contributed. Blumenbach termed it his " Golgotha," and it was a rare circumstance to see the curious and inquiring of both sexes wondering and interested in a collection of skulls.

It perhaps deserves to be recorded, that the subject of this first work of his youth was also that of his last scientific contribution, for, after the 3d August 1833, when, upon the exhibition of a Hippocratic macrocephalus, he communicated his observations upon it,* he did not again speak in public, except on the occasion of the *eloge* of Stromeyer, and the few never-to-be-forgotten words at the anniversary of the hundredth year of the Society.

As Blumenbach anxiously endeavoured to indicate the distinction between the animal and human structure, and with this view adduced, as characteristic, the upright posture of man and the vertical line, so he was equally zealous in vindicating to human nature, as such, all the powers and rights of humanity, which, without rating too highly the influence of climate, soil, and hereditary descent, he regarded as the direct consequences of civilization and refinement. Man he considered as " the most perfect domestic animal." In Blumenbach's inimitable representation of the wild *Peter von Hameln*,† he shews what man would be without the assistance of society, and how the matter stands as to innate ideas. How even the bony structure of the skull gradually approaches the animal form when unfavourable external localities and civil relations check the development of the higher powers, he exhibited in the Cretin skulls of his collection, which were purposely placed near the skull of the orang-outang, and where, at no great distance, the attention of the spectator was attracted by the extremely beautiful form of a female Georgian.

At a period when negroes and savages were regarded as half animals, and when the idea of the emancipation of slaves

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tion of his *Beiträge zur Naturgeschichte.* (Contributions to Natural History, 1806. Part i. p. 55–66.)

* Gött. gel. Anz. 1833. No. 177, p. 1761.

† Contributions to Natural History, part ii. p. 1–44.

had not begun to excite interest, Blumenbach raised his voice in order to shew that their psychical qualities were not inferior to those of Europeans, that between their races the greatest differences prevail, and that it is only opportunity for development which is wanting for the display of their higher powers.* As Blumenbach relished a joke when no one was injured by it, he wrote an essay entitled, "On the races of Men and Swine."† Man continued to be his chief subject, but not in a transcendental point of view (for that he left to moral philosophers and theologians), but as he is in the external world; and while he contributed essentially to our better acquaintance with, and appreciation of, this subject, he was not easily excelled by any one in practical acquaintance with human nature.

Natural history, in its proper sense, and not the description of nature, was the task he had undertaken. With Bacon, Lord Verulam, he regarded it as the *prima materia philosophiæ.* He understood well how to represent, by means of a few characteristic traits, what was peculiar in objects, and at the same time how to bring out their individual internal‡ properties and relations, so as to apprehend, as a whole, their economy and their position. Therefore it was that he occupied himself more especially with animal organic nature. That he was, however, no stranger to the study of geology and mineralogy, is clearly shewn by De Luc's letters § to Blumenbach, by what he wrote on Hutton's Theory of the Earth, and by his paper on the Impressions on Bituminous Marl-slate found at Riegelsdorf.||

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* In the Göttingen Magazine, 1781. No. vi. p. 409–425. "*Ueber die Fähigkeiten und Sitten der Wilden.*" (On the Capabilities and Manners of Savages.)

† In Lichtenberg and Voigt's Mag. für das Neueste aus der Physik, vol. vi. Gotha, 1789. No. i. p. 1.

‡ He laboured long on a history of natural history, but published nothing upon it. That he thought of the possibility of a philosophy of natural history is evident, among other proofs, from a letter to Moll in the *Mittheilungen* of the latter. Part i. 1829, p. 60.

§ In the *Magazin für das Neueste aus der Physik.* Vol. 8, No. 4, 1793. Compare also Gött. gel. Anz. 1799. Part 135, p. 1348.

|| In Kohler's Mining Journal. Freyberg, 1791, fourth year. Vol. i. p. 151–6. Blumenbach shewed that these impressions were of a mammal but not of a child, and therefore were not anthropolites.

Among the individuals who, by the investigation and exposition of the traces of ancient remains, have efficiently contributed to the history of the formation of our globe and its earliest inhabitants, Blumenbach's name must be included. He was the first who formed a collection of fossils to illustrate and to systematize the remains of the pre-Adamitic period.*

In the year 1790 he wrote his "*Beiträge zur Naturgeschichte der Vorwelt*" (Contributions to the Natural History of the Ancient World).† He devoted two discourses, delivered to the Society, to the consideration of the organic remains which had become known to him from the districts around the city.‡ He also pointed out the connection of palæontology and geology, illustrating the more exact determination of the relative age of different deposits in the crust of the earth by means of organic remains,§ and it was he who gave the first impulse to this subject. After a journey to Switzerland, he directed attention to those fossils of which the living analogues still exist in the same district, to such as have their ori-

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* The fossil genus Oxyporus, which occurs in amber, and which Gravenhorst mentions in his *Monographia Coleopterorum Micropterorum*, Götting. 1806, p. 235, is in the collection of Blumenbach. In regard to Blumenbach, the author says, "*Utinam Blumenbachius multorum quæ possidet electro inclusorum insectorum descriptionem et comparationem cum insectis hodiernis affinibus ederet. Ingenium viri celeberrimi et de historia naturali jam diu egregie meriti, perpensam sane hypothesin de ortu et formatione electri nobis inde impertire posset.*"

† In the *Magazin für das Neueste*, &c.

‡ Specimen Archæologiæ Telluris terrarumque inprimis Hannoveranarum, 1801. In the Commentat. vol. xv. p. 132–156. Spec. alterum, 1813. In vol. iii. recent. p. 3–24.

§ "On the Succession of the Different Catastrophes of the Globe," in the second edition of his Contributions to Natural History, 1806. Part i. p. 113–123. One of the most competent judges in this department, Link, in his work "*Die Urwelt und das Alterthum erläutert durch die Naturkunde*," which he dedicated to his master, says in the preface, that we have to thank Blumenbach and Cuvier for the elucidation of the entire difference between the ancient and the present world. Von Hoff, likewise, who is so well qualified to give an opinion, says in his *Erinnerung an Blumenbach's Verdienste um die Geologie*, Gotha, 1826, p. 3, "Among naturalists, Blumenbach was the first who assigned to palæontology its true place among the foundations of geology; he regarded it as the most useful auxiliary of that science; and he declared, as his belief, that important conclusions in cosmogony were to be anticipated from the study of organic remains, and more particularly of their various relative positions."

ginals in widely remote parts of the earth, and to those of which no living example occurs in the present creation.* Farther, he expressed his opinion on the supposed remains of fossil human bones in Guadaloupe.† His thoughts dwelt upon such subjects as well as upon more general considerations, as, for example, on "gradation in nature,"‡ and "on final causes," § but he always preferred topics upon which observation could be brought to bear, and which were susceptible of direct proof. He was not addicted to brilliant hypotheses, subtle combinations, or fanciful suppositions.

If it can be said of any scientific work of modern times that its utility has been incalculable, such may well be asserted of Blumenbach's "Handbuch der Naturgeschichte" || (Manual of Natural History). There are few civilized countries or districts where it is unknown. This work, which indicated, with each new edition,** the progress of its author, contains in small space an astonishing mass of well-arranged materials. But while it was his endeavour to make it as perfect as possible, we must acknowledge the art displayed in giving only what was essential, in communicating, in one word or observation, that which was really interesting, agreeable, or useful, and thus exciting to further study. Blumenbach knew how to render this study serviceable, not only by rendering the whole subject accessible in a simple, tangible, and clear manner, but also by calling in the aid of allied topics, and thus acquiring new points of view and a wider range,

His "Contributions to Natural History,"†† and his ten parts of "Representations of Natural Historical Objects,"‡‡ have rendered profitable service to the establishment and extension of science by their interesting expositions, by their judicious

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* In Lichtenberg and Voigt's *Mag. für das Neueste*, &c. 1788. Vol. v. p. 13–24.

† Ine *Gött. gel. Anz.* 1815. No. 177, p. 1753.

‡ In the second edition of his Contributions, 1806. Part i. p. 106–112.

§ Ibid. p. 123.

|| It first appeared in 1779.

** The editions which issued from the publisher alone were twelve, the last in the year 1830, without including the reprints and the translations into almost all the languages of civilized nations.

†† The first part appeared in 1790, the second in 1811.

‡‡ 1796–1810.

selection, and by their accuracy. He was peculiarly zealous in drawing from ancient monuments* and poets† explanations of doubtful questions, and the elucidation of different problems in natural history. The migration of animals, and their partial occurrence in prodigious quantity, and over a great extent, appeared to him a subject by no means cleared up. He did not hesitate to publish his contribution to the future solution of this important question‡

Blumenbach has been occasionally somewhat blamed for following, with but slight exceptions, the artificial Linnæan arrangement; but this adherence arose neither from motives of convenience nor from want of knowledge, but from the conviction that the period for a natural system had not arrived. It is evident that he felt the want of such a system, from his having, in 1775, sketched the attempt to form a natural arrangement of the mammalia,§ which was not confined to individual or a few characters, but included the whole *habitus* of the animals.

His papers on the sexual inclinations of animals, and on the natural history of serpents,|| display not only talent but critical observation. There is much interesting matter in his account of a kangaroo¶ which he kept alive in his house for some time, and in his observations on the pipa** and on tapeworms.††

Blumenbach was deeply impressed with the truth, that we are only properly qualified to judge rightly of the phenomena of the present, when we have rendered as clear as possible their course from the earliest period. He regarded archæology

* Specimen historiæ naturalis, antiquæ artis operibus illustratæ eaque vicissim illustrantis, 1803. In Comment. vol. xvi. p. 169–198.

† Sp. hist. nat. ex auctoribus classicis præsertim poetis illustratæ eosque vicissim illustrantis, 1815. In Comment. recent. vol. iii. p. 62–78. Compare Gött. gelehrte Anzeigen, 1815. No. 205, p. 2033–2040.

‡ De animantium coloniis sive sponte migratis, sive casu aut studio ab hominibus aliorum translatis. In Comment. recent. vol. v. p. 101–106. Gött. gel. An. 1820. No. 57, p. 561–568.

§ Gött. gel. Anz. 1775. No. 147, p. 1257–1259.

|| Magaz. für das Neueste aus der Physik, vol. v. No. 1, 1788, p. 1–13.

¶ Ibid. 1792. Vol. vii. No. 4, p. 19–24.

** Gött. gel. Anzeigen, 1784. No. 156, p. 1553–1555.

†† Ibid. 1774. No. 154, p. 1313–1316.

and history not only as the foundation of true knowledge, but also as the sources of the purest pleasure. Several years after he wrote on the teeth of the ancient Egyptians and of mummies,* an opportunity was afforded him during his stay in London, on the 18th February 1791, of opening six mummies,† and a communication addressed to Banks on the results obtained added to his celebrity. Blumenbach took part in the opinion‡ given by this Society of Sciences on Sickler's new method of decyphering the Herculanean manuscripts.

He expressed his belief that the syenite of Pliny§ is our granite. He possessed a collection of antique stones for the elucidation of the history of ancient art, and on this account he was often consulted on the decision of different questions, as, for example, the alleged steatite antiques.|| He had collected beautiful engravings and paintings illustrative of natural history and the varieties of the human race, and he placed particular value on the representations of animals in the older works and woodcuts; because the exact point which the art of observation had attained could, by means of these, be well investigated. He also was anxious to become acquainted with the first anatomical woodcuts, and published the results of his inquiries,** as previously they had remained almost entirely neglected. After a careful comparison of the older works of this kind which had become known to him, his decision††

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* Götting. Magaz. 1780. Year 1, p. 109–139.

† Philosophical Transactions, 1794. In the third edition of his Diss. de Generish. Variet. Nat. 1795, he gives his Epistola ad vir perill. Jos. Banks. This subject is fully considered in his Contributions to Natural History, part ii. p. 45–144.

‡ Gött. gel. Anz. 1814. No. 200, p. 1993.

§ Ibid. 1819, p. 1208. Blumenbach treated of the more accurate determination of the stones employed by the ancients, in the second part of the edition of his Natural History published in 1780.

|| Gött. gel. Anz. 1811. P. 2050.

¶ Gött. Magaz. 1781. No. 4, p. 136–156.

** Baldinger's N. Magazin für Aerzte, 1780. Vol. ii. p. 33.

†† De veterum artificum anatomicæ peritiæ laude limitanda, celebranda vero eorum in charactere gentilitio exprimendo accuratione. The treatise itself was not printed; but for an account of its contents see Gött. gel. Anz. 1823. No. 125, p. 1241.

was, that the praise bestowed on the anatomical knowledge of the old artists should be limited, but that their accuracy in delineating characteristic representations could not be too much acknowledged.

In literary history Blumenbach emulated his pattern and example, Albrecht Von Haller, with whom he had become acquainted while a student at Göttingen, by having, at the request of Heyne, sent him to Bern a work* which he had obtained at an auction, and which Haller had mentioned in one of his writings that he had not seen.† Afterwards he communicated to him on several occasions supplements and additions to the volumes of the Practical Medical Library.‡ Of all the bibliographical works of that distinguished man, Blumenbach prized most highly the Bibliotheca Anatomica.§ He wrote a preface|| to Haller's *Tagebuch* of medical literature, in which he praises the author's critical powers.

Although the literary portion of medicine is so frequently neglected, yet I doubt not that most medical men are acquainted with Blumenbach's introduction to the Literary History of Medicine.¶ In that work the whole range of medicine is delineated to the end of the previous century in suitable general views, and the task is performed with singular judgment, precision, and brevity. At the jubilee on occasion of the fiftieth anniversary of our university, he brought together the merits of the Göttingen medical professors** in a synopsis which doubtless served not less as an acknowledg-

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* Observationum Anatomicarum Collegii privati Amstelodamensis. Pars altera. Amstel. 1763.

† Haller's answer is dated 28th March 1775.

‡ Baldinger's N. Magazin. für Aertzte, 1780. Vol. ii. p. 33.

§ Probably no one ever read the works of the eminent Göttingen professor so carefully as Blumenbach. He found much in the collection of letters from and to Haller; and, besides many important notices for the history of medicine, he obtained that of piercing the drum of the ear for deafness. (Gött. gel. Anz. 1806. No. 147, p. 1459.)

|| Baldinger's N. Mag. 1780. Vol. ii. p. 33–39.

¶ Introductio in Historiam Medicinæ Literariam, 1786.

** Synopsis systematica scriptorum, quibus inde ab inauguratione Academiæ Georgiæ Augustæ usque ad solemnia istius inaugurationis semisæcularia disciplinam suam augere et ornare studuerunt professores Medici Gœttingenses, 1788.

ment to these men than as an incitement to subsequent teachers to emulation.

He often did homage to the memory of distinguished individuals, and chiefly in his "Medical Library,"* a journal which can hardly be excelled; but also as secretary of our society, where the mournful duty was ably performed by him of delivering addresses on occasion of the deaths of Richter (1812), Crell (1816), Osiander (1822), Bouterwek (1828), Mayer (1831), Mende (1832), and Stromeyer (1835).

His tribute to the memory of Regimental-Surgeon Johann Ernst Wreden† is so far of importance for the history of medicine, because that long-forgotten practitioner was the first who introduced inoculation on the Continent, and he did so in Hanover. His account of the Meibomic collection of medical manuscripts‡ in the Göttingen library ought not to escape the attention of those interested in the literary history of medicine.

What has been already stated would have been amply sufficient to place the services and talents of Blumenbach in a proper light; but we have still left unnoticed his chief merits, and it will become evident from what I shall mention, that in this single individual were united many qualifications of such a nature, that any one of them would have been enough to secure celebrity for its possessor.

Physiology and comparative anatomy are the two subjects in which Blumenbach's name is most pre-eminently conspicuous. What he has done for these departments by his writings and lectures, will assuredly so much the less easily be forgotten by his native country, since it was through him chiefly that foreign countries first acquired a taste for these studies, and rendered gratitude not merely to him, but generally to German science. The obscure doctrines of generation, nourishment, and reproduction, received from him light and critical explanation. Although since the period when he first began with such vigour of intellect to examine the then existing materials, and to

* Vol. i–iii. 1783–1795.

† Annalen der Braunschw. Lüneb. Churlande 1789. Year 3, No. 2, p. 389–396.

‡ In his "Medical Library." Vol. i. p. 368–377.

prosecute investigations for himself, now sixty years ago, more comprehensive results than he attained have been acquired, yet it can with truth be affirmed that his conclusions, though no doubt extended and occasionally corrected, have been in no degree overturned.

On the 9th May 1778, observations on green hydræ, then in the act of reproduction, first led him to the perception and subsequent extensive investigation of the incessantly active power of nature. In the year 1780 appeared his treatise "On the Forming Impulse (*Bildungstrieb*), and its Influence on Generation and Reproduction,"* and, the year after, the monograph† "On the Forming Impulse and the Process of Generation." At that time he also wrote a paper "On an unusually simple kind of Propagation,"‡ which was that of the conferva occurring in wells, whose mode of propagation he had discovered on the 18th February 1781.

He sent a short answer to the question proposed by the Petersburg Academy, "On the power of Nutrition,"|| on the 25th May, the day after he finished it; and obtained half the prize. He wrote remarks on Troja's investigations regarding the production of new bones.§ Respecting the "Regenerating of an eye in the Water Salamander," he communicated to a meeting of this society,¶ the fact of four-fifths of the eyeball having been cut out, and a new eye having been formed.

With great acuteness, and with rare knowledge, he pointed out the anomalous** and diseased deviations of the forming impulse (Bildungstrieb), and shewed how artificial means,†† or

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* Götting. Magaz. 1780. P. 247–266.

† 1781. And then in the Comment. vol. viii. p. 41–68 : De Nisu Formativo et Generationis Negotio, 1785.

‡ Götting. Magaz. 1781. No. i. p. 80–89.

|| De Nutritione ultra vasa. The prize was adjudged on the 4th December 1788. The number of essays transmitted was twenty-four. Nova Acta, Ac. Sc. Petropolit. v. 6. 1790. Histoire. Compare two treatises by K. F. Wolf, St Petersburg, 1789 (the second is by C. F. Born).

§ Richter's Chir. Bibliothek. Vol. vi. No. 1, 1782, p. 107.

¶ Gött. gel. Anz. 1785. Part 47, p. 465.

** De anomalis et vitiosis quibusdam nisus formativi aberrationis, 1812. Commentat. recent. vol. ii. p. 3–20.

†† Magazin für das Neueste aus der Physik, 1789. Vol. vi. part 1, p. 13.

accidental mutilations, act on animal bodies, so as, in time, to produce hereditary effects. That his doctrine of forming impulses was adopted, and, though with altered modes of expression and representation, employed as the basis of wider development, as by Kant in his Criticism on the Judgment, by Fichte in his Ethics, by Schelling in the "*Weltseele*," and by Goethe in his Morphology, afforded him peculiar satisfaction, as it proved at the same time both its soundness and productiveness.

His Handbook* of Physiology is distinguished by the elegance of its language, and, like all his works, by the well selected information, as well as the number and value of his own observations.

The question as to whether the blood should be regarded as a peculiar vital power occupied him† greatly; as did also the cause of the black colour in the Negro.‡ He confirmed by his own observations the chief experiments of Galvani.|| He endeavoured to ascertain the truth respecting the eyes of the white Negro,§ and the movement of the iris, by comparing and considering the facts previously collected, and by his own observation. On the 23d August 1782, he had examined two Albinoes at Chamouni.

In 1784 he discovered,¶ while dissecting the eye of a seal, the remarkable structure by which that animal is enabled, at will, to lengthen or shorten the axis of the eyeball, so as to

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* Institutiones Physiologicæ, 1787. Of the many editions and translations, Blumenbach placed most value on that by Elliotson, published by Bentley of London in 1814, because this was the first book printed entirely by a machine. Compare Gött. gel. Anz. 1818, No. 172, p. 1713. A 4th edition of Elliotson's translation was published in 1828.

† De vi vitali sanguinis, 1787. Comment. vol. ix. p. 1–13. Afterwards his *Programm:* De vi vitali sanguini deneganda, vita autem propria solidis quibusdam corporis humani partibus adserenda curæ iteratæ, which was occasioned by the publication of John Hunter's work on the Blood after the death of that surgeon, and appeared in the year 1795.

‡ De generis h. variet. nat. p. 122, &c. 3d edition.

|| Gött. gel. Anz. 1793. No. 32, p. 320.

§ De oculis leucæthiopum et iridis motu, 1784. Comment. vol. vii. p. 29–62. Compare Gött. gel. Anz. 1784, No. 175. Med. Bibliothek, vol. ii. p. 537–547.

¶ Commentat. vol. vii. 1784, p. 46. Handbook of Comparative Anatomy, 3d edition, p. 401.

have the power of seeing distinctly in two media of such different degrees of density as air and water. He was the first to point out accurately the nature and the character of the frontal sinuses,* as well as their relations to diseases. The crossing of the nerves of sight he regarded as an ascertained fact.† He did not feel justified in adopting the belief in a muscular coat in the gall-bladder.‡ With regard to the protrusion of the eyes in the case of individuals beheaded,|| he pointed out that the phenomenon was not entirely caused by congestion, as it is in that of persons who have been hanged. While making a communication "On a Male Goat which afforded Milk,"§ he spoke of the occurrence of milk in the breasts of men, and endeavoured to find an explanation of the phenomenon.

His "History¶ and Description of the Bones of the Human Body," in which this of itself uninviting subject is treated in the most interesting and novel manner, will also possess a permanent value. His Handbook** of Comparative Anatomy was not only the first of its kind in Germany, but in the scientific world. Before his time no book on the entire science had appeared; he first introduced it into the circle of subjects of instruction.

One of his first productions treated of the Alcyonellæ,†† found in water near Göttingen. He also furnished a comparative view of warm and cold blooded animals,‡‡ and afterwards a comparative view of warm-blooded viviparous and oviparous animals.|||| We must not pass over in silence his observations

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* Prolusio anat. de sinibus frontalibus, 1779. His *Programme* when appointed ordinary professor. See Gött. gel. Anz. 1779, p. 913–916.

† Gött. gel. Anz. 1793. No. 34, p. 334.

‡ Ibid. 1806. No. 135, p. 1352.

|| Abhandl. der Phys. Med. Societät. zu Erlangen, 1810. Part 1, p. 471.

§ Hannöverisches Magazin, 1787. No. 8, p. 753–762.

¶ First of all in 1786, and then in 1806.

** First published in 1805.

†† Götting. Magaz. 1780, p. 117–127.

‡‡ Specimen physiologiæ comparatæ inter animantia calidi et frigidi sanguinis, 1786. Comment. vol. viii. p. 69–100.

|||| Spec. physiologiæ comp. inter animantia calidi sanguinis vivipara et ovipara, 1788. Comment. vol. ix. p. 108–129. Compare Gött. gel. Anz. 1789, No. 8, p. 73–77. In this treatise he gives his views on the occurrence of the yellow bodies in the unimpregnated ovum; on the formation of the

on the structure of the ornithorynchus,* on the bills of ducks and the Toucan,† and on the sack in the neck of the reindeer.‡

In so far as Blumenbach considered physiology as the foundation of the science of medicine, it is easy to see in what point of view his contributions to practical medicine are to be regarded; he omitted no opportunity of proving his interest in this particular direction. He published his ideas on the frequency of hernia || in the Alps; on home sickness; § on melancholy¶ and suicide in Switzerland; on the expulsion of a Scolopendra electrica from the nose;** and on a case of water in the head of seventeen years' standing.†† He contributed also to materia medica, by his experiments with gases on living animals;‡‡ his communication of a new species of dragon's-blood from Botany Bay; |||| and his description of the true Winter's-bark.

Blumenbach's scientific reputation stood so high, that every hint from him was regarded and followed, such as his remarks on the best methods of collecting extracts, notes, &c.; §§ and his works, such as his handbooks, enjoyed such celebrity, that authors and booksellers¶¶ considered a preface from him as the best recommendation for their works. In this manner were

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double heart; and, on the period at which the ribs are produced in the embryo.

* De Ornithorynchi paradoxi fabrica observationes quædam anatomicæ. In the Mémoires de la Soc. Med. d'Emulation, vol. iv. Paris, 1799, p. 320–323. And Gött. gel. Anz. 1800, p. 609–612.

† In his "Specimen Physiologiæ Comparatæ inter Animantia Calidi Sanguinis vivipara et ovipara," 1789.

‡ Gött. gel. Anz. 1783. No. vii. p. 68.

|| In his "Medical Library," vol. i. p. 725.

§ Ibid. p. 732; and compare Schlözer's Correspondence. Part iii., 1778, p. 231.

¶ Med. Bibliothek, vol. ii. p. 163–173.

** Compare J. L. Welge Diss. de morbis sinuum frontalium. Götting. 1786, iv. § 4. p. 10.

†† Med. Biblioth. vol. iii. p. 616–639.

‡‡ Ibid. vol. i. p. 173.

|||| Contributions to Materia Medica in Med. Bibl. vol. i. p. 166–171.

§§ Ibid. vol. iii. p. 547.

¶¶ He added a preface to Gmelin's History of Animal and Vegetable Poisons. Erfurth, 1805.

introduced by him Cheselden's Anatomy,* Neergard's Comparative anatomy and physiology of the digestive organs,† and Gilbert Blane's ‡ Elements of medical logic.

I must now allude to one kind of knowledge in which Blumenbach had hardly his equal, that is, his acquaintance with voyages and travels. He had read all the works of that description in the Göttingen library, made extracts from them, and prepared a triple list, namely, a geographical, a chronological, and an alphabetical. He was indebted to this occupation, as he often was in the habit of saying, for no small portion of his information: he was thus furnished with an ever productive mine for his natural historical and ethnographical labours.

He himself had made comparatively few long journeys; || for he had only been through a part of Switzerland § and Holland, to England, or rather to London,¶ which he termed the sixth quarter of the globe, and to Paris, whither he went, at the Westphalian period, on a diplomatic mission, to bespeak the favourable consideration of Napoleon for the university, on which occasion De la Cépède was his intercessor and conductor. During his journeys he kept journals, in which he inserted briefly whatever was worth knowing. Of these very varied notices but little is as yet known.**

He executed a translation of the medical observations †† in the second part of Ives's Travels; he wrote a preface to the

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* German edition, by A. F. Wolf. Göttingen, 1789.

† Berlin, 1806. In the preface, Blumenbach speaks of the influence of comparative anatomy on the philosophical study of natural history generally, and more particularly on the physiology of the human body, and on the veterinary art.

‡ Göttingen, 1819.

|| When he required a jaunt for his health, he was glad to visit the Dowager Princess Christina von Waldeck at Arolsen, where he was always most graciously received; or he made a trip to Pyrmont; or to Gotha, Rehburg, Weimar, and Dresden.

§ In 1783.

¶ In the years 1791–1793.

** Remarks on some tours in Waldeck, collected in Schlözer's Correspondence, part iii., 1778, p. 229–237. Also, some natural historical remarks made during a journey to Switzerland, in the Magazin für das Neueste, &c. vol. iv. No. 3, 1787, p. 1; and vol. v. No. 1, 1788, p. 13.

†† The other part of this voyage to India was translated by Dohm. Leipsic, 1775.

first part of the collection* of remarkable voyages and travels; and he furnished a preface and remarks for Volkmann's translation of Bruce's Travels.

It is indeed not too much to assert that the desire awakened in many distinguished men to undertake extensive natural history voyages and travels, and the results thus obtained for science, were caused in a great measure by Blumenbach. Hornemann,† Alexander Von Humboldt, Langsdorf, Seetzen, Röntgen, Sibthorp, Prince Maximilian of Neuwied, were and are his grateful scholars.

To Blumenbach's unknown, or at least not sufficiently appreciated, literary services, belong his extremely numerous critiques, published not only in the "Bibliothek" edited by himself, but more especially in the Göttingen "Gelehrte Anzeigen," and which passed under review works in all his various branches of knowledge for a long series of years. His first critique was on a treatise by Xenocrates, and appeared in the 2d vol. of Walch's Philological Library.

He had himself at first experienced how unreasonably and thoughtlessly reviews are frequently executed; ‡ with him it was a fixed principle invariably to separate the person from the thing, to render the judgment as objective as possible, and never to abuse the office of scientific judge, by rendering it the vehicle for the display of personal feelings. His reviews are distinguished for their concise brevity, for seizing hold of the essential points in discussion, for interspersed jocularity, and for instructive original observations and views.

A manuscript observation by Blumenbach, which I found in a note-book which he lent me on one occasion, is worthy of attention, because, to a certain extent, it explains the fa-

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* Leipsic, 1790, in 5 volumes.

† Hornemann first expressed to his teacher his desire to travel in the interior of Africa on the 2d July 1794. Blumenbach afterwards published an account of this enterprising young man, and of the successful execution of his plan in Zach's Allgem. Geogr. Ephemeriden., vol. i., Weimar, 1798, p. 116–120, p. 368–371, and vol. iii. p. 193.

‡ As his Hand-book of Natural History was as ignorantly as inconsiderately reviewed, he wrote an article in the Göttingen Magazine, "On a literary curiosity, but one which unfortunately is no rarity." 1780, p. 467–484.

cility he had in preparing articles like those of which I have spoken. He says, " I was obliged when at school to make out a sketch of the sermons we heard in church. This practice afterwards proved of great benefit to me in reading, extracting, and reviewing, and in the performance of many public duties, because it gave me the power of selecting, retaining, and expressing concisely the essential points under consideration."

As Blumenbach was involved in few literary feuds, it did not easily happen that his critiques drew down upon him hatred or anger; but sometimes he could not avoid calling things by their proper names, and exhibiting false celebrity in its real nakedness.*

(To be concluded in our next Number.)

Observations on Solar Radiation, made at Fort Franklin in the years 1825, 1826, *and* 1827. By John Richardson, M.D., F.R.S., and Inspector of Naval Hospitals. Communicated by the Author.

While residing at Fort Franklin, in North America, in the year 1825–26, I made a series of observations on the heating power of the sun's rays with a black-bulb thermometer, and some of the results were published in the " Appendix to the Narrative of Sir John Franklin's Second Journey." It has been observed by an eminent philosopher, in reference to observations of this kind, that, as measures of solar radiation, they have generally been made on an erroneous principle, " the true indication of the force of the solar rays not being the statical effect upon the thermometer, but their momentary intensity, measured by the velocity with which they communicate heat to an absorbent body."† The actinometer has

* His critique of Kämpf's New Method of curing the most obstinate Diseases of the Bowels (Med. Bibl. vol. ii. No. i.) gave offence to the author, but yet afterwards obtained from him for Blumenbach public thanks (in the second edition of that book, Leipsic, 1786, p. 366).

† Sir John Herschel, quoted in the Report by Professor Forbes on Meteorology; Trans. of the British Association, vol. i.

since been contrived by Sir John Herschel for this purpose; but in the report of the Committee of Meteorology appointed by the Royal Society, it is said, "As the actinometer can only be observed at intervals in perfectly clear weather, additional information with regard to solar radiation, of much interest, *though not of so precise a nature*, may be obtained, by the daily register of the maximum temperature of a register thermometer, with a blackened bulb, exposed to the full action of the sun's rays. It may be placed about an inch above the bare soil, and screened from currents of air. The maximum temperature indicated by such a thermometer, even in cloudy weather, will generally be considerably above that of the air, and the maxima and mean daily maxima of its indications will, after a long series of observations, afford data of the utmost value to the history of climates." As this recommendation will undoubtedly be extensively acted upon by the expedition which has sailed to the antarctic regions, and at the observatories established in connection with it, we may expect to have in a few years a large body of facts recorded concerning solar radiation in various latitudes; and it will obviously facilitate the deduction of general laws therefrom to have the means of comparing observations made in the southern hemisphere with similar ones made in the arctic regions. With this view, I have revised the original records of the Fort Franklin observations, for the purpose of tabulating them more fully than has been done in the appendix above mentioned, so that, in conjunction with the tables there given, the most important of the results may be readily exhibited.

When I first thought of commencing the observations in question, I had no personal experience to guide me in the best mode of conducting them, nor had I read of any example that I could follow, further than the general recommendation to travellers to observe the effect of the sun on a thermometer with the bulb blackened or wrapped round with black wool. My earliest trials were, therefore, of the nature of experiments, and for two or three months were made only at times when the sun shone brightly. In February 1826 I began to observe every hour that the sun was above the horizon, and continued to do so till the end of April, when I found, on summing up the hourly columns of each month, that the curves of temperature were very irregular, the irregularities being evidently caused by the black-bulb thermometer not being properly sheltered from the wind.* I have included these months in the following tables, but they must be considered as very imperfect. In the winter there were many calm days, but as the spring advanced the air was seldom still when the sun shone; and in May, therefore, I completely sheltered the blackened thermometer by enclosing it in a large thin glass bottle. In this month the mean excess of the temperature indicated by the blackened thermometer over one in the shade rises in the morning at each successive hour of observation, attains its maximum at noon, and descends again in the afternoon, as shewn in the accompanying plate (Plate V.) And this I regret to say is the only month for which I possess hourly observations to be depended upon. I left the Fort in June, and though Mr Dease kindly continued the observations every third hour in July and August, yet as he had no watch whereby to measure the time, the results for these months must be somewhat uncertain. It is satisfactory, however, to find that both the mean excess and the maximum excess in these two months are greater the nearer the hours are to noon. From September 1826 to the end of April following (1827), the observations were continued at 8, 10, 11, A.M., and 1, 2, 4, P.M., and after the middle of February at noon also by Sir George Back and Lieutenant Kendall; and though I have not their original registers to refer to, I have extracted from Franklin's appendix as many of the results as the tables there given would furnish.

For these observations, a pair of thermometers corresponding most nearly with each other in their scales was chosen. Up to the end of April 1826, spirit-thermometers were used; in May, July, August, and September, mercurial ones were employed; and from November till the end of April 1827, the spirit ones were resumed. They were all constructed by Newman, and had spherical bulbs half an inch in diameter. The thermometer exposed to the sun was prepared by coating its

* See Franklin's Appendix above quoted, where the days on which the wind affected the black-bulb thermometer are marked by an asterisk.

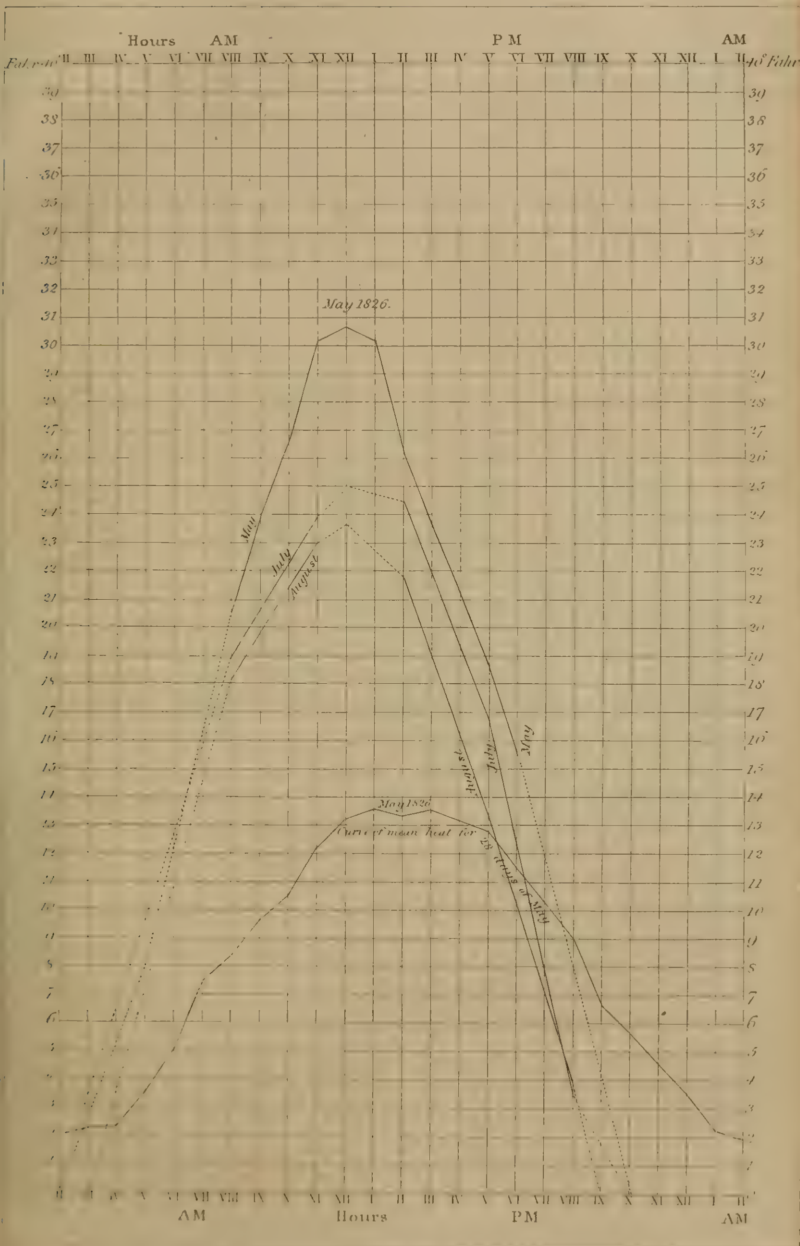

Plate showing the Curve of mean temperature in the shade for 28 last days of May 1826 and the Curves of the excess of a Radiation Thermometer for May, July, & August, same year.

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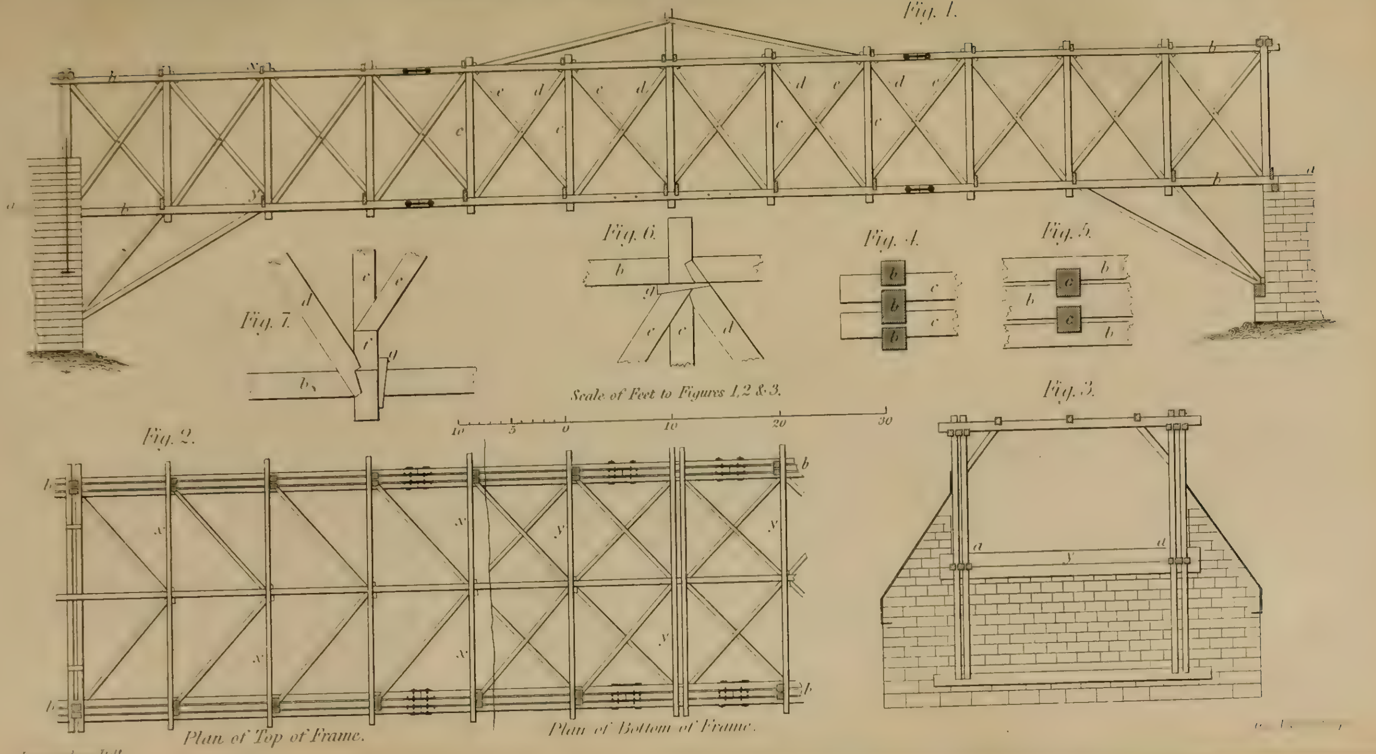
Stevenson's Notice of Long's American Frame Bridge.



bulb with a thin film of silk paper, and then blackening it thoroughly with china-ink and indigo. The silk paper was used to overcome effectually the polish of the glass and prevent the reflection of any of the sun's rays. From the commencement of the observations till the end of April 1826, the blackened-bulb thermometer was hung on the south side of a rough deal shed used as an observatory; while the corresponding thermometer with a clean bulb was secured on the north, and consequently shady side. The black-bulb thermometer was therefore sheltered from the winds that came from the northern points of the compass only. In May 1826, and all the following months, both the clean and black-bulb thermometers were secured on the top of a slender detached post rising three feet above the sandy soil. A square, thin, clear glass bottle, four inches wide, placed on the top of the post, enclosed the radiation thermometer, and protected it from the wind. Its mouth was left open. The other thermometer was secured on the same post, at the same height, and its bulb, with the lower part of its scale, were enclosed in two concentric brass cylinders, which permitted a free circulation of air, but effectually intercepted the sun's rays. This was ascertained by almost an hourly comparison with two other thermometers, one inside the observatory, which was regularly registered in connection with the magnetical observations, and another hung in the open air on the north side of the observatory. The latter always felt the influence of the sun in May and the summer months, both in the morning and evening (owing to the high latitude), and being also unsheltered from radiation of the sandy soil and of the deal observatory, was scarcely ever lower than the thermometer enclosed in the brass cylinders even at noon. The black-bulb thermometer in the bottle was very sensitive, and has been noticed to fall 10° in the short space of time occupied by a cloud passing over the face of the sun in a moderate breeze. In clear nights it often shewed a lower temperature than any of the thermometers with clean bulbs, the difference in some instances amounting to 4°. During May 1826, a spirit black-bulb thermometer unsheltered by glass was hung against the observatory and shifted from side to side with the course of the sun. A register of its indications is printed in Franklin's appendix, and when contrasted with the register of the one in the bottle, shews the necessity of protecting the radiation thermometer by glass if the observations are to be continued in windy weather.

From Tables IV. V. and VI., the daily curve of the mean effect of the sun in May, July, and August, may be constructed, and the plate on which the curves are exhibited contains also the daily curve of temperature for the twenty-eight days of May on which the observations were made (30° being subtracted from the temperature at each hour). Table VII. is added to shew the effect of the sun in raising the temperature of the atmosphere between sunrise and one or two in the afternoon, the maximum temperature at the two latter hours being recorded.

As the observations are tolerably complete only for one month (May 1826), deductions from them as to the intensity or to the total effect of the sun's rays in the different seasons of the year would carry but little weight. I have, however, in Franklin's appendix, broached an opinion, that in the high northern latitudes of America, the average intensity of the heat of the sun is greater in the spring months than near the summer solstice, the greater clearness of the atmosphere compensating for the smaller altitude of the sun. This opinion is supported by the subjoined tables. In Table I., for instance, the means of the maxima indications of the radiation thermometer for February 1827 are nearly equal to those for May 1826, though the times of observation were so much more numerous in the latter month, and consequently there was more chance of ascertaining the true maximum on each day; they are also considerably above those for the first sixteen days of May 1827, when the observations were more frequent than in February. In March and April 1827, the means exceed those of February and May; and if they do not do so also in April 1826, it is to be attributed to the effect of the frequent winds in that month on the unsheltered radiation thermometer. The column containing the monthly maxima of the black-bulb thermometer also leads strongly to the same conclusion; and here the want of shelter is not so operative, as many of the clearest days were calm, particularly in the winter and early spring months. In this column the maxima increase in amount from December to March or April, and decrease gradually in the succeeding months. The irregularity for July 1826 may be accounted for by only twenty-one days being included; and the smallness of the number of observations in the autumn of 1825 may also be considered as the reason of the small mean for October. Table VII., and what is nearly the same thing, the much greater sharpness and altitude of the mean curves of temperature for March and April, as shewn in Plate I., f. 5, Geograph. Journal, vol. ix., may also be adduced in corroboration of my remark.

I shall not repeat here the attempt I have made elsewhere to explain the cause of the clearness of the atmosphere in the spring of those climates, but shall merely remind the reader, that at Fort Franklin the snow does not disappear till the beginning of May, consequently the soil cannot before that month accumulate heat from day to day; and that when the snow is at the melting point, a powerful sun one day will have little effect in raising the mean heat of the following day.

When the solar rays are projected at low altitudes into the lower dense or cloudy stratum of the air, considerable irregularities and sudden changes of temperature must result, producing partial currents, and mingling of masses of air in different conditions, all increasing the scattering action of the strata on the solar light.* But if, from the natural effect of the climate, the air in the high northern regions of America be peculiarly free from clouds, and clear in the spring, there does not appear to me to be any great difficulty in explaining why the more oblique rays in spring should have a superior effect on the black-bulb thermometer, than the more direct ones passing through a comparatively cloudy atmosphere near the summer solstice. I have no doubt but that future experiments will shew that the sun, at *equal altitudes*, acts more intensely (in spring at least) near the poles, than near the equator, although the increase of the temperature of the atmosphere may be greater in the latter locality through indirect radiation.

* *Vide* Astronomy, by Sir J. F. W. Herschel, &c., p. 33.

TABLE I.

Of Mean daily Maximum excess for each month of the Radiation Thermometer over the Temperature in the Shade; and of the Maximum excess for each month.

Months.	No. of Days on which Observ. were made.	No. of Hours at which Observ. were made.	Mean of daily maximum excess of Black-bulb Thermometer.	Mean Temp. in the Shade at the Hours of Observation.	Maximum excess in the month of the Black-bulb Therm.	Temp. in the Shade at the time when the preceding maximum occurred.	Remarks.
1825.							
Oct.	8	53	9.00	+19.63	23.5	+25.0	Black-bulb thermometer not protected by glass, and only sheltered from northerly winds by Observatory.
Nov.	19	69	12.31	+ 2.31	35.0	— 5.5	
Dec.	23	77	11.04	—15.67	28.0	+14.5	
1826.							
Jan.	20	49	16.12	—24.61	48.0	—38.5	
Feb.	28	196	25.58	— 8.98	57.9	—15.0	
March,	31	297	34.22	— 5.50	65.0	— 8.4	
April,	30	269	24.99	+20.28	51.0	+34.9	
1826.							
May,	28	336	35.62	+44.05	49.8	+50.2	Black-bulb thermometer enclosed in a thin glass bottle.
July,	23	111	28.02	+57.46	38.5	+61.5	
August,	31	155	26.12	+56.82	41.5	+55.5	
Sept.	24	144	25.14	+45.70	42.0	+31.0	
Nov.	30	120	10.74	+ 3.57	33.5	+ 6.5	
Dec.	31	70	7.05	— 8.89	24.0	+ 2.0	
1827.							
Jan.	31	126	15.29	—22.65	38.3	+22.2	
Feb.	28	150	34.58	—19.65	53.2	—17.2	
March,	31	217	45.16	+ 1.01	68.0	+17.0	
April,	30	210	53.18	+16.15	70.0	— 2.5	

Note.—In October, November, December 1825, and January 1826, the observations were recorded only when the sun shone out favourably; in the following months, up to May inclusive, almost regularly at the stated hours. In July and August 1826, they were made at intervals of three hours; and from September 1826 to May 1827, six, and latterly seven, daily observations were made throughout each month.

The maximum excess of the radiation thermometer in the first sixteen days of May 1827 was 50°.2, and the mean of the maxima for the same time was 33°.85.

TABLE II.

Of the Maximum excess of the Black-bulb Thermometer over one in the Shade at the several hours of Observation.

Hours.	Black-Bulb Thermometer not sheltered.							Ther. sheltered by Glass.		
	1825.			1826.				1826.		
	Oct.	Nov.	Dec.	Jan.	Feb.	March.	April.	May.	July.	Aug.
4 A.M.	...	...	...	...	...	...	...	22.0	...	...
5 ...	...	...	...	...	...	...	...	22.5	..	...
8 ...	...	...	...	...	...	44.5	30.5	38.8	35.5	37.0
9 ...	...	...	...	...	40.1	48.2	48.0	38.5	...	...
10 ...	23.5	19.7	3.5	34.5	53.4	50.0	43.5	42.0	...	...
11 ...	11.5	31.0	18.0	41.0	57.9	57.0	51.0	44.0	38.5	41.5
Noon.	17.8	30.7	28.0	39.8	47.0	65.0	44.8	49.8	...	...
1 P.M.	13.0	24.0	20.0	46.0	42.8	38.9	44.0	49.5	...	...
2 ...	14.0	35.0	12.0	48.0	33.9	46.0	40.5	43.7	34.5	42.0
3 ...	...	...	...	...	30.2	35.4	38.0	40.5	...	...
4 ...	...	...	...	...	...	30.0	32.0	38.4	...	...
5 ...	...	..	...	...	...	17.5	27.5	32.0	30.0	34.5
6 ...	...	...	...	...	...	...	...	46.0	...	...
8 ...	...	...	...	...	...	...	...	...	23.0	23.5

TABLE OF TEMPERATURES IN THE SHADE, WHEN THE ABOVE MAXIMA (TABLE II.) IN THE SUN WERE OBSERVED.

Hours.	1825.			1826.				1826.		
	Oct.	Nov.	Dec.	Jan.	Feb.	March.	April.	May.	July.	Aug.
4 A.M.	...	...	...	...	...	...	...	+44.0	...	...
5 ...	...	...	...	...	...	...	...	+38.0	...	. .
8 ...	...	...	...	...	...	—30.0	— 9.5	+39.2	+58.0	+61.0
9 ...	...	...	...	...	—25.0	—20.0	+29.0	+13.0	...	...
10 ...	+25.0	—9.2	—34.0	—38.5	—19.8	— 8.8	— 4.5	+39.0	...	...
11 ...	+30.5	—8.5	—39.8	—41.0	—15.0	—11.5	+34.9	+40.0	+61.5	+55.5
Noon.	— 5.8	—8.5	+14.5	—39.8	—26.0	— 8.4	— 1.8	+50.2	...	...
1 P.M.	+32.0	—7.0	+15.0	—29.0	— 9.6	— 3.9	— 1.0	+44.0	...	...
2 ...	— 5.0	—5.5	+14.0	—38.5	—13.0	—10.0	— 0.5	+43.8	+57.0	+58.0
3 ...	...	...	...	...	—14.0	— 0.4	0.0	+43.0	...	...
4 ...	...	...	...	...	...	+14.0	+ 0.2	+34.3	...	...
5 ...	...	...	...	...	...	—23.5	0.0	+45.6	+56.0	+51.5
6 ...	...	...	...	...	...	...	...	+44.0	...	...
8 ...	...	...	...	...	...	...	...	+48.0	...	+51.0

TABLE III.

Of the Monthly Mean excess of the unsheltered Black-bulb Thermometer over one in the Shade, at the several hours of Observation.

Hours.	1825. October.	1825. November.	1825. December.	1826. January.	1826. February.	1826. March.	1826. April.
8 A. M.	...	...	...	...	4.45	16.04	11.67
9 ...	...	...	. .	...	7.99	22.46	15.41
10 ...	7.05	5.99	2.37	13.50	16.57	25.00	16.38
11 ...	4.50	10.38	7.03	22.57	17.13	24.86	17.63
Noon.	7.88	10.32	11.25	15.13	17.31	23.97	17.72
1 P. M.	7.25	6.02	7.29	16.59	14.45	22.02	17.76
2 ...	5.90	7.07	4.16	26.13	12.43	22.51	18.64
3 ...	...	...	...	...	7.36	18.12	13.76
4 ...	...	...	...	...	4.57	13.97	10.87
5 ...	...	...	...	...	...	10.72	7.77

TABLE OF THE MEAN TEMPERATURE IN THE SHADE AT THE HOURS OF OBSERVATION OF PRECEDING TABLE.

Hours.	October.	November.	December.	January.	February.	March.	April.
8 A. M.	...	...	...	...	...	— 11.11	+ 15.18
9 ...	...	...	...	...	— 13.70	— 6.95	+ 17.72
10 ...	+ 24.20	— 3.28	— 16.12	— 29.30	— 11.14	— 3.38	+ 19.50
11 ...	+ 17.50	+ 1.78	— 17.34	— 33.89	— 9.20	— 1.95	+ 21.07
Noon.	+ 18.92	+ 2.91	— 14.42	— 25.35	— 8.33	— 0.03	+ 21.51
1 P. M.	+ 21.88	+ 1.60	— 13.72	— 25.60	— 7.72	+ 0.84	+ 22.22
2 ...	+ 20.38	+ 2.32	— 15.45	— 29.60	— 8.24	+ 1.71	+ 21.44
3 ...	...	...	...	...	— 9.35	+ 1.47	+ 22.84
4 ...	...	...	...	...	...	— 0.24	+ 22.35
5 ...	...	...	...	...	...	— 2.94	+ 19.64

The observations were recorded only at favourable times in the months previous to February. Thus, in

October at 10 h. there were but 6 obs.; at 11 h., 4 obs.; at noon, 6 obs.; at 1 h., 4 obs.; and at 2 h., 8 obs.

November, at 10 h., 8 obs.; 11 h., 12 obs.; noon, 16 obs.; 1 h., 9 obs.; 2 h., 11 obs.

December, at 10 h., 4 obs.; 11 h., 18 obs.; noon, 22 obs.; 1 h., 20 obs.; 2 h., 13 obs.

January, at 10 h., 7 obs.; 11 h., 8 obs.; noon, 16 obs.; 1 h., 7 obs.; 2 h., 6 obs.

February, only 2 obs. at 8 A.M., but daily at the other hours, except 4, when the observations were necessarily confined to the last 14 days, the sun not being above the horizon before.

March, daily at each hour, except at 11 h., when 29 obs.; 4 h. when 30; and 5 h., 18 obs.

April, at 8 h., 28 obs.; 9 h., 29 obs.; 10 h., 29 obs.; 11 h., 29 obs.; 2 h., 20 obs.; 4 h., 28; 5 h., 21; at the other hours daily.

TABLE IV.

Of the Mean hourly excess shewn by a Black-bulb Mercurial Thermometer sheltered by Glass over a clean Thermometer in the Shade, for the twenty-eight last days of May 1826 *(from fourth to thirty-first day inclusive).*

Hours.	Mean heat in the Shade.	Excess of Black-bulb in the Sun.	
		Mean.	Maximum.
8 A.M.	+38.25	20.42	38.8
9 ...	39.70	23.84	38.5
10 ...	40.51	26.59	42.0
11 ...	42.14	30.10	44.0
Noon.	43.27	30.74	49.8
1 P.M.	43.51	30.16	49.5
2 ...	43.36	26.27	43.7
3 ...	43.42	23.70	40.5
4 ...	43.04	21.14	38.4
5 ...	42.75	18.59	32.0
6 ...	41.39	15.56	46.0

An observation made at 5 A.M. from the 4th to the 17th inclusive, yields a mean excess of the black-bulb of 6.29°, and a maximum of 22.5°; and one at 4 A.M. from the 18th to the end of the month, a mean for the 14 days of 6.36°, and a maximum of 22°. The mean temperature for the whole 28 days at 4 A.M. was +32.35°, and at 5 h., +33.51° F.

TABLE V.

Containing similar Observations for 21 *last days of July* 1826.

Hours.	Mean heat in the Shade.	Excess of Black-bulb in the Sun.	
		Mean.	Maximum.
8 A.M.	+56.45	18.93	35.5
11 ...	59.74	23.90	38.5
2 P.M.	65.38	24.40	34.5
5 ...	56.08	16.75	30.0
6 ...	51.89	3.62	23.0

TABLE. VI.

August 1826.

Hours.	Mean heat in the Shade.	Excess of Black-bulb in the Sun. Mean.	Maximum.
8 A.M.	51.78	18.17	37.0
11 ...	56.83	23.08	41.5
2 P.M.	56.88	21.68	40.0
5 ...	52.63	13.37	34.5
6 ...	48.89	3.69	23.5

The observations for July and August were made by Mr Dease at intervals of 3 hours, but as he had no watch to regulate his time, the hours could not be very accurately kept. He was accustomed to judge of the time of day by the position of the sun, and a meridian line was traced, by which he could ascertain noon.

TABLE VII.

Exhibiting the Monthly Mean difference of Temperature in the Shade at Sunrise, from the Mean Maximum at 1 *or* 2 P.M.

Months.	Mean Temp. at Sunrise.	Mean Temp. at 1 or 2 P.M.	Difference of these two.	Mean Temp. of the Month.
1825.				
September,	+42.14	+44.82	2.680	+42.92
October,	+18.32	+23.75	5.430	+20.27
November,	+ 1.77	+ 4.83	3.064	+ 2.79
December,	—14.45	—12.05	2.405	—14.35
1826.				
January,	—24.86	—21.48	3.378	—23.77
February,	—14.79	— 7.72	7.072	—12.70
March,	—15.42	+ 1.71	17.129	— 8.27
April,	+ 7.47	+23.21	15.735	—15.20
May,	+28.27	+41.73	13.462	+36.33

Note.—The mean temperature in the third column is for the most part that at 1 P.M.; but when the temperature at 2 was greater, that is given.

Remarks on the Preceding Paper. By Professor FORBES.

Dr Richardson has, I think, fairly deduced from his observations, confessedly imperfect as they are, that his photometric apparatus was more affected by sunlight in March and April than during the summer months. Whether this be due to the greater intensity of the solar rays in spring, as he supposes, may perhaps be considered as not so fully proved. The principle of measuring the intensity of solar radiation by a blackened thermometer, is due to Lambert, and was ingeniously and elegantly applied by Leslie. Such instruments, *carefully sheltered by glass* (as in Dr Richardson's later experiments), are certainly capable of yielding valuable results, although we have not yet learned to interpret them aright. Their indications are, however, very different from the *sensible* effects of the sun's action on the animal frame for instance, and from the more direct measure of it which is obtained by means of Herschel's Actinometer.* Any measures sufficiently often re-

* It is a curious fact, which has only come lately to my knowledge, that in Sir J. Leslie's earliest paper, read to the Royal Society in 1793, but first published twenty-six years later in Thomson's Annals of Philosophy, vol. xiv., he has laid down with perfect clearness the principle of the actinometer, which he has described as the only true measure, and which yet he wholly overlooked in the final construction of his Photometer, which measures the statical maximum of temperature which a blackened ball is capable of assuming, instead of the momentary increment of heat which it receives. His words are: "The initial change on the thermometer is in

peated, under circumstances nearly the same, have a certain degree of comparability with one another; and Dr Richardson's curve of diurnal radiation is so regular as to confirm the comparable character of his method. The results obtained in different months, (Table I.) are less satisfactory; but by projecting both the mean and extreme numbers, I find a coincidence which seems to indicate a maximum effect about the month of April, and not in June, as we might expect from observations in other climates. Dr Richardson declares that experience has convinced him that the sky is clearer in spring than in summer in the arctic regions, and has assigned reasons for this difference. (Franklin's Second Journey, *Appendix*, p. cx.) But without disputing the fact, we must be allowed to doubt (as contrary to general experience) whether the increased power of the sun's rays, owing to the shorter tract of air traversed, is not far more effective (a few degrees of elevation at low altitudes making a difference of thickness to be traversed quite enormous, varying nearly as the secant of the zenith distance) than any diminution due to slight vapours raised by the solar heat. We must not, however, rest in mere conjecture. Dr Richardson has pointedly alluded in the preceding paper (p. 245), and in Franklin's Appendix (p. cx.,) to the *presence of snow on the ground** as accompanying the peculiar atmospheric condition which he considers so favourable to the solar action. I am disposed to attribute the effect *solely to the mechanical action* of the snow in reflecting the solar light to the instrument. Dr Richardson has alluded to a fact well known to those who have used Leslie's photometer, that the presence of clouds, when dense, white, and luminous, affects most strongly its indications, and that the scattered light even of a blue sky equals sometimes the direct effect of the sun itself. This paradoxical result shews the necessity that there is for cautious deduction from an instrument so curiously sensitive, and as yet so imperfectly studied; but as these facts cannot be doubted, there can be no difficulty in believing, that the reflection from a boundless field of daz-

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every case the only certain and accurate measure of the communication of heat." (Thomson's Annals, 1819, xiv. p. 7.)

* "It" (the force of the sun's rays) "was much stronger in the spring months, WHEN THE GROUND WAS COVERED WITH SNOW, than in the summer months, when the altitude of the sun was greater."

zling snow must be most energetic indeed, and that the disappearance of the snow in May causes an apparent diminution of solar intensity, because the rays which before were reflected are now chiefly absorbed by the vegetable and earthy surface of the ground.*

I regret very much that a fit opportunity has not offered itself since Dr Richardson's paper came into my hands of verifying my conjecture by direct observation; but snow and bright sunshine are elements more rarely combined in the latitude of Edinburgh than that of Fort Franklin.

Observations with the actinometer would fairly eliminate the disturbing influence of the snow and all others, and therefore can alone be perfectly relied on in deciding this question.

16th February 1841. JAMES D. FORBES.

Observations on the Progress of the Seasons as affecting Animals and Vegetables at Martin's Falls, Albany River, Hudson's Bay. By GEORGE BARNSTON, Esq. Communicated by Dr RICHARDSON, F. R. S., Inspector of Hospitals, Haslar.

THE following Paper was drawn up in consequence of a printed communication from Dr Richardson, handed to me last September. Martin's Falls is a station on Albany river, thirty miles below Gloucester, which is marked in the maps. Having no sextant, I am unable to give the exact latitude.† Our geological position is upon the confines of the great basin of James' Bay, an immense extension of the older calcareous strata. Between the falls and the coast the bed of the river is composed of limestones and clays, both containing extinct genera of shells; while above, towards the interior, little is to be seen but gneiss and greenstone schist, with a mixture here and there of less fissile granitic rocks. The fossils which I have been able to procure in this neighbourhood are principally spirifers, producta, terebratula, and impressions of trilobites. Although in winter we have the cold of Russia, in the months of July and August we enjoy the climate of Germany and the north of France.

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* Since this explanation occurred to me, I find that M. Arago had adverted to the glare of the snow as a reason of the apparently greater intensity of solar radiation in high latitudes, in his criticism on Mr Daniell's Meteorological Essays.

† About Lat. 51° 30′ N.; Long. 86° 20′ W.

December, January, February, } Our dead winter months. { We are frequently visited by the White Owl or Harfang from the Bay, but the Hawk-owl is our most common bird of prey. Besides our three indigenous species of grouse (*Tetrao umbellus, Canadensis,* and *Phasianellus*), we have the Willow Grouse, or White Bird as it is called, (*T. saliceti,* Temm. *T. albus,* Auct.) from the northward.

March. Martens pair and soon afterwards rabbits (*Lepus Americanus*).

15. In the middle of the month the snow often melts in the height of the day, and by the 20th a Snow-bird may be seen if the season be early.

20. Tops of the higher grasses appear. A few brown feathers appear on the necks of some Willow-birds (*Tetrao albus*). They now leave us.

April. There is a slight crust on the snow, from the thaw of the day and the frost of the night. When the weather is mild and the sun shines, a few insects appear.

8. Two species of Perla and one of Nemoura come up through the crevices of the ice and the porous snow, and all proceed in direct course for the nearest bank.

10. The cold renders them too weak to fly, though most of them have got rid of their nymph spoil before emerging from the ice.

15. Snow-birds have become plentiful, and are now joined by another Bunting with black head (Emberiza Lapponica?) and the Yellow-breasted Lark (*Alauda alpestris*).

20. The Flesh-fly still scarce. The small Owl (*Scops*) calls in the warm nights. The common Woodpecker (*Picus pileatus*) drums on the hollow trees.

22. The Grey Goose of Canada and Stock Ducks sometimes appear, but are frequently forced to return to the southward by a northerly blast and want of water.

25. A few spots of ground bare.

28. The American Robin (a species of Thrush, with red breast) and the Cattle Blackbird (*Xanthonus,* with yellow eye) are now arriving, and pick up the benumbed grubs and caterpillars. Goshawks arrive.

May. Ground getting barer; snow melting rapidly.

5. Wild-geese and Ducks passing to the northward. Hawks still arriving.

10. Every fine day brings an accession to the small bush birds,—Muscicapæ, Motacillæ, &c. Food for these is still scarce, and they approach the houses in quest of the Dipteræ which rise from the manure and rich earth around the place. Snow-birds have left us. Ermines and Rabbits become altogether brown. The ice is now shingly and dangerous. Strong currents and rapids

open everywhere. Wavies, as they are called (Snow Geese), and Barnacles passing in large flocks for the bay. No weather now stops them.

May.

12. The Northern Diver or large Loon and black Ducks (*nigra, fusca, perspicillata*) are still scarce, but are sometimes seen. The buds of Poplar, Aspen, and of various Willows, swell. On the latter may be found a few of the earliest Tenthredines. The tender bud is the nursery of their larvæ. Two species of Butterfly (*Vanessa* and *Argynnis*) sport over the ice and snow, when these are not gone.

15. The larger rivers break up. Fish ascend the small streams. The Jack fish and Perch (*Luciopcrca*) spawn. The Suckers or Carp soon follow. Trout take the bait greedily. The Cliff-Swallow is seen. Swamps and stagnant pools are thawed. A frog may be heard attempting to croak: A Musquito (*Culex*) felt to bite.

20. Shells (*Limnæi*) begin to move in the pools along the river. Snails (*Limax, Helix, Bulimus, &c.*) remove from under stones and fallen timber. The end of the month discloses some species of Moths (*Noctualites*).

25. Our only Goatsucker (the white patched wing) and the Ground Woodpecker (*Picus auratus*), the last of our spring birds, arrive. Beavers, Otters, and Musks, have their young. In late seasons, the lakes to the northward break up.

28. The Poplar and Aspen leaves expand. The Rein-Deer, or Grey Deer of Hudson's Bay, has young.

June. Sturgeon begin to frequent the Falls and Rapids, and to spawn.

5. Insects on warm days are busy, the Tenthredenitæ on the bushes, the Sphingides, Andrenetæ, and Pangoniæ, on the ground, all attended by a great variety of parasite ichneumons. The first flowers blow, and those of the Willow are surrounded by Sylphides and Flower Flies (*Anthomoyæ*.)

10. A night of frost will still sometimes intervene, and in the woods the ground is still solidly frozen at a foot from the surface. Vegetation nevertheless still goes forward. Gnats become a torment, the swamps and puddles swarm with their larvæ. Small Tadpoles abound in the pools.

12. The country is now covered with verdure. Birds are nestling, Geese and Ducks hatching. Indians generally occupied with the Sturgeon fishery.

15. The latest shrubs have leaves, and the majority of Moths and Butterflies are disclosing themselves. The large species of Ephemera, Perlæ, and Phryganeæ issue from the water.

20. Trout take the fly-hook. White fish rise to the surface. Cattle seek the houses to get rid of their tormentors the Tabani. In

dry seasons the creeks become low, although the large rivers retain their strength.

July. Our warmest month. The river usually gets low. Sturgeon fishing continues. Cattle are lean, feeding only at night, tormented by flies during the day.

10. Many genera of Coleoptera appear, some of them peculiar (if I may say so) to warmer climes. We have Cicindelæ, Necrophori, many Buprestes, and a species allied to Lucanus.

20. Of those whose larvæ live on wood, the Serropalpus, a very fine Dorcacerus, Cerambyx, Callidium, Lamia, and numerous species of Lepturetæ. Neuroptera are abundant on the banks of the river—Libellulæ, Agrion, &c., and on the leaves, Hemerobius, Panorpes, Sialis ;—in the other orders there are also many genera to keep up the character of the month. It ends with bringing us strawberries, which have been hitherto scarce, and in sending off the Sturgeon, which return to the depths.

Aug. The Raspberry begins to ripen. Young Ducks are well feathered. We have sultry weather for a few days, and then thunder storms with chilly nights.

10. Pigeons are numerous. Young Geese can fly. Gnats decrease, but Sandflies (*Similium*) supply their place.

15. The Raspberry and red and black Currants ripen. Grasshoppers are full grown. Trout move about, ascending the river. Sturgeon are very scarce. Grass becomes brown in dry situations.

20. The noisy Yellow Leg (*Totanus*) appears, and if we have much rain we are visited by a species of Snipe. The Golden and Ring Plovers are not uncommon.

Sept. The air generally is cooler, the winds stronger, and frosty nights may be expected.

10. Trout spawn. Insectivorous birds of many kinds leave us. The Migratory Pigeon, so frequently seen during August, disappears. Hawks and the large Yellow Horned Owl (*Strix Virginiana*) are common. Frosts frequent at night.

15. Tops of potatoes always blackened. Caterpillars nearly all cased. Trout refuse the fly-hook, but still take the bait. They are now poor fish. Grey Geese begin to pass to the southward. Ducks abound in the grassy lakes. Leaves turning rapidly yellow.

20. Pleasant weather in the middle of the day, but cold at night. The Fall Moth (*Phalæna autumnalis*) is now to be seen. Sandflies bite only in the height of the warmer days. The Musquito is utterly defunct. Diving Ducks common, the others gone.

Oct. Pools and swamps crusted with ice. White fish begin to spawn.

5. Suckers and Trout desert the small streams. Foliage is yellow, and falls. Rutting season of Deer. Instead of rain we have

snow, which, however, generally melts, the earth being warmer on the surface than the atmosphere.

Oct.

10. A single blast of northerly wind will suffice to bare the trees, strip the shrubs, and send all water-fowl to the south. The last of these are the weak or lingering flocks of Snow-Geese or Wai-wai-duck, which may frequently be observed passing. They seldom alight except when met by adverse winds.

20. The small lakes and rivers sometimes fast, that is, frozen. Tullibee (*Coregonus*) spawns. Animals get well furred. The Willow Grouse arrives from the north. There is usually a little snow on the ground, and the American Hare, as also the Ermine, are changing colour.

Nov. The ground covered with snow, which in mild weather is often blackened by a species of Podura, like a grain of gunpowder.

10. I have also frequently observed crawling about at this time and later, a species of Tipula. It is wingless, and I have named it *Chionea hiemalis*. Can it be this insect which gives rise to the idea of Spider rains?

20. Large rivers, as well as the lakes, are often solidly frozen, strong rapids filling up, and setting fast. Rabbits and Ermines are entirely white, the swamps are passable, and winter may be deemed to have set fairly in.

GEO. BARNSTON.

Martin's Falls, 10th June 1840.

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*On the Evaporative Power of different kinds of Coal.** By ANDREW FYFE, M.D., F.R.S.E., President of the Society of Arts for Scotland. Communicated by that Society.

THE experiments, the results of which I am now to bring before the Society, were undertaken with the view of ascertaining the comparative evaporative power of different kinds of coal. Of course, in this investigation, my attention has been directed solely to the power of the fuels in raising steam, with the view of testing their comparative value for steam-engines.

This subject has lately engaged much of the attention both of scientific men and of practical engineers, and much valuable information has been communicated regarding it.

* Read before the Society of Arts for Scotland 8th February 1841, and ordered to be printed in its Transactions.

Much, however, yet remains to be done; and now, when owing to the rapid increase of steam-machinery the demand for fuel is so greatly increased, it becomes the duty of all who can add to the sum of our information, to make public the result of any experiments they have made on this important subject.

Very different opinions have been, and are still, entertained regarding the source of heat during combustion, and of the power of different inflammables for evolving heat. The recent experiments of Despretz on this subject are perhaps the most important, as tending to the deduction of a law by which, if correct, we shall be enabled to calculate with accuracy the amount of heat evolved by different combustibles. From his numerous experiments, Despretz has drawn the conclusion, that the heat disengaged during combustion is in proportion to the quantity of oxygen with which the combustible unites. Thus applying this rule to hydrogen, carbon, alcohol, and ether, and taking the quantity of water raised from 32° to 212° as the means of measuring the comparative amount of heat disengaged, he found by experiment that the quantity of water brought to the boiling point by the union of

1 lb. of oxygen	with hydrogen, was .	$29\frac{1}{2}$ lb.
Do.	with carbon, was . .	29
Do.	with ether, was . .	$28\frac{1}{2}$
Do.	with alcohol, was . .	28

results so very nearly agreeing with each other, that, keeping in view the unavoidable sources of error in all experiments of this kind, we may consider the quantity the same in all. The average of the above trials is $28\frac{3}{4}$. Hence, when one pound of oxygen enters into union with any inflammable, heat is evolved, which, according to Despretz, is sufficient to raise $28\frac{3}{4}$ lb. of water from the freezing to the boiling point. It may be stated in round numbers as 29 lb. We have thus, then, a method of procuring a standard for the amount of heat disengaged during combustion. Thus 1 of carbon unites with 2.66 of oxygen, and $2.66 \times 29 = 77.14$, so that, by this process of calculation, 1 lb. of carbon ought to raise 77.14 lb. of water from 32° to 212°. This is rather below what is stated by Despretz. He has fixed it at 78.15.

Different statements have been given of the quantity of caloric received by water during its conversion into steam, in other words, of the latent heat of steam. If we suppose, as has been stated by Lardner and others, that it requires $5\frac{1}{2}$ times as long to evaporate water that it does to raise it from the freezing to the boiling point, then the latent heat will amount to 990. But others have made it lower than this. According to Despretz it is only 955.8. Assuming this as correct, then in steam the total number of degrees of temperature beyond 32 is (180 + 955.8) 1135.8—say 1136. Hence if 1 lb. of carbon will raise 78.15 lb., as stated by Despretz, from 32 to 212, it will evaporate 12.3 lb. from 32—and this is the quantity fixed on by him.

It is well known that the different substances used as fuel consist, in their original state, chiefly of carbon and of hydrogen, in addition to which there is generally a minute quantity of oxygen and of nitrogen, and there is always a portion of earthy and metallic matter, constituting the ashes. The only one of these which, in addition to the carbon, will evolve heat during the combustion, is the hydrogen. Now 1 of hydrogen combines with 8 of oxygen, or exactly three times as much as carbon requires. 1 lb. of hydrogen will therefore evaporate 37 lb. of water from 32.

It is evident from this, that if we know the composition of the fuel, we can calculate the evaporative power by knowing the quantity of oxygen necessary for converting the carbon and hydrogen into carbonic acid and water. Of course, the greater the proportion of hydrogen, the greater ought the evaporative power to be. If the fuel contain nitrogen, a part of the hydrogen must be deducted from the whole quantity, because the nitrogen will unite with it to form ammonia; and, again, if oxygen exist in the fuel, the hydrogen which is requisite to convert that oxygen into water must also be deducted, and, accordingly, in addition to the carbon, it is only the hydrogen over and above what is required for uniting with the nitrogen and oxygen, that are to evolve heat by the combustion.

The most recent account of the analysis of various kinds of coal, is that published by Mr Richardson in the Trans. of the Nat. History Society of Newcastle, and also in the Lond.

and Ed. Phil. Mag. for August 1838. From his experiments he has given the composition of the coal, and the quantity of oxygen necessary for the combustion. The following table shews the results, dividing the coals into four classes, as mentioned by Dr Thomson.

Coal.	Locality.	Carbon.	Hydrogen.	Oxygen and Azote.	Ashes.	Oxygen required for combustion of 100 Parts.	Heat evolved by the same weight Edin. 100.	Heat given out by the same vol. Edin. 100.
Cannel,	Edinburgh,	67.597	5.405	12.432	14.566	217.6	100.	100.
—	Lancashire,	83.573	5.660	8.039	2.548	256.6	117.83	117.91
Splint,	Wylam,	74.823	6.180	5.085	13.712	240.1	110.34	108.99
—	Glasgow,	82.724	5.491	10.457	1.128	250.5	115.12	114.15
Cherry,	Newcastle,	84.846	5.048	8.430	1.676	253.9	116.68	112.07
—	Glasgow,	81.204	5.452	11.923	1.421	244.0	112.12	107.78
Caking,	Newcastle,	87.952	5.239	5.416	1.393	266.7	122.56	119.03
—	Durham,	83.274	5.171	9.036	2.519	250.2	114.98	111.31

From this table it appears that there is not much difference in the heat evolved by the perfect combustion of the varieties of coal mentioned; assuming that the heat is in the ratio of the oxygen consumed, thus leaving out the cannel coal, which is not used for steam-engines, and also the Wylam, which is not now in the market, the extremes are 122.56, and 112.12; that is, as 100 to 109.3. Or, taking the average of the two samples of Scotch coal and that of the English; it is as 100 and 109.6. These numbers may therefore be considered as representing, the former the Scotch, and the latter the English, caking coal.*

The analysis of organic matter, such as that used for fuel, requires a great deal of nicety in the manipulation; it has therefore been proposed to adopt a simpler method for arriving at the quantity of oxygen necessary for the combustion, than by ascertaining the proportions of the ingredients. A very ingenious method of doing so has been recommended by Berthier, founded on the decomposition of metallic oxides by inflammable matter. It is well known that when these oxides are heated with carbon, carbonic acid is expelled, and the

* Higher results than that given above have been obtained by others; but I wish those stated to be considered as applying merely to the coals used; though, I believe, they will be found as very nearly the average comparative power of Scotch and English coal.

metal is reduced, and the same also occurs when hydrogen is passed over the oxide previously heated. Now when coal is exposed to heat along with an oxide, both the carbon and hydrogen unite with the oxygen, and metal is set free. If therefore we previously knew the composition of the oxide, and after heating it with the combustible, we can ascertain the weight of the metal produced, we of course know the quantity of oxygen which that metal has yielded to the combustible. By far the best oxide for that purpose is litharge, because it parts with its oxygen easily, while both the metal set free and the excess of oxide used are melted, and during the subsequent cooling and congelation the heavy metal falls to the bottom of the vessel, and when solid can be easily removed from the oxide and weighed. For the necessary precautions in conducting this process, the reader is referred to Berthier's Traité des essais par la voie sèche.

As carbon requires 2.66 of oxygen for complete combustion, it will set free 34.66 of metallic lead from litharge. Now 1 of carbon will boil off 12.3 of water; accordingly a fuel which, when heated with litharge, yields 34.66 times its weight of lead, ought to evaporate 12.3 times its weight of water from 32, provided the combustion is perfect, and provided also the whole of the heat evolved by the combustion is absorbed by the water. Should the quantity of lead be greater or smaller, then the evaporative power of the coal will be proportionably greater or less. It is well known, however, that in burning fuel the combustion is rarely, if ever, perfect, and it never happens that the whole of the heat dissolved is taken up by the water; as to the latter, there always is, indeed, in the common way of consuming fuel, there must be, a waste, which is necessary to keep up the draught; but in addition to this, much of the heat must also be lost by the ascending current of air, which in some furnaces is excessive, and of course the waste is enormous, frequently amounting to one-third, sometimes to one-half of that evolved. With regard to the imperfect combustion, much must depend on the nature of the fuel, and on the particular construction of the furnaces. Thus, when the fuel in common use is heated, it gives off gaseous inflammable matter, which, if it be brought in contact with air at the requisite temperature, will be inflamed; but if air be not present, or if there be a deficiency of it, then the greater part of the gas will escape without undergoing combustion, and hence the waste which, it may be said, occurs in every furnace constructed in the usual way. It has been supposed by some, that the combustion is perfect where there is no smoke, but this is by no means a proof that the whole of the inflammable is consumed; a part of the gaseous matter may be escaping in the state of hydro-carbon, or of carbonic oxide, without undergoing any action, and if so, it is just so much fuel wasted. It is evident, then, that the *practical evaporative power* must depend very much on the manner in which the combustion is effected, and also on the peculiar constitution of the coal; even when the combustion is as perfect as we can expect, still there may be a loss of heat from the generation of the gaseous materials; for when the carbon and hydrogen are evolved as hydro-carbons, they must absorb caloric to enable them to assume the gaseous form. Though we should naturally expect, therefore, that the heat evolved by those coals which contain much hydrogen should be greater than when little of it is present, this is not always the case; indeed we shall find that the practical evaporative power is greatest when the fuel contains a great deal of fixed carbon; for when the carbon is in that state, it must, before it can escape, combine with oxygen and thus be consumed, whereas, as already mentioned, the hydrocarbons may partly fly off without being burned.

From what has been said it is evident that the method proposed by Berthier is well adapted for ascertaining with ease the amount of heat that ought to be evolved by the combustion of a fuel; yet it does not indicate the available heat, in other words, what may be called the *evaporative power in practice*. The only method, I conceive, by which this can be done, is by actual combustion in properly constructed furnaces, and by measuring the quantity of water that is evaporated by the use of a given weight of the fuel, and we can then compare this with the quantity which carbon will evaporate, and which, as already stated, is, according to Despretz, 12.3 times its own weight from the temperature of 32.

I am aware that to this method the objection may be urged, that as our furnaces are never so constructed as to effect complete combustion, and that as the combustion varies very much in different furnaces, the results ought not to be relied on. Now this is undoubtedly so far a valid objection; but if our trials are conducted with what will be allowed to be a furnace constructed on proper principles, and if the trials are made on the different fuels, making such adjustments and alterations as may be requisite for the fuel under use, then the results, though they do not give what ought to take place, yet give the comparative evaporative power in practice, and in this respect become extremely valuable, as pointing out the kind of fuel that is most beneficial for the purposes required.

From the opportunities I have had of testing different fuels in this way, both on a large and on a small scale, I trust the results will be found of sufficient importance to require no apology for my laying them before the Society.

In the following statement I shall give the results by actual trial in furnaces, and compare them with those which ought to be got according to the composition of the fuel, and also according to the method proposed by Berthier.

In conducting the analysis of the fuel, I have not thought it necessary to ascertain minutely the proportion of all the ingredients. I have determined merely the quantity of moisture, of gaseous matter evolved by heat, of fixed carbon, and of ashes or incombustible material; and this was done in the usual way by expelling the moisture by cautious application of heat; then driving off the volatile matter by exposure to a strong heat, excluded from air; and lastly, heating in contact with air, to burn off the fixed carbon, and thus to ascertain the proportion of it and of the ashes. The combustion by the process of Berthier was conducted with the usual precautions, so as to secure success.

With regard to the furnaces in which these trials were made, they were sometimes small, at other times and most frequently large, such as those attached to a four and ten-horse engine. In carrying on the experiments with one particular kind of fuel, I mean anthracite, it was necessary to have recourse to a peculiar construction of furnace, so as not only to secure the complete combustion of the coal, but also to try its power in raising steam rapidly, which, it is well known, is necessary when the fuel is to be used for steam-engine furnaces; and it is of the utmost consequence to keep this in view; for however great the evaporative power even in practice may be, that fuel is of little value if it cannot be burned so as to make it applicable for the purposes for which fuel is generally used, such as the rapid raising of steam, and keeping up a sufficient supply for the engine.

The experiments, the results of which I am first to detail, were made with the view of ascertaining the comparative practical evaporative power of anthracite and of Scotch coal. They were conducted with a furnace attached to a four-horse high-pressure boiler; the furnace bars, of which there were from 12 to 14, according to circumstances, were each 3 feet 6 inches in length, and in all 2 feet 4 inches in breadth, including the spaces between them, giving 7 feet of fire surface. It was at first fitted up with flash-flues, which were afterwards changed to the common flue, 18 inches by 14, passing along the boiler on one side, returning on the other, and then entering the chimney. The chimney-stalk was 22 inches by 18, and 33 feet in height, in addition to which an iron tube of a foot in diameter and 12 feet in height was attached, thus making the whole height 45 feet. To this furnace and boiler there was attached an apparatus by which the fuel could, when required, be supplied with warm air. It consisted of a metallic box placed immediately beyond the end of the bars forming the floor of the furnace, from which there proceeded tubes that passed through the boiler, and so placed in it as to be surrounded by the water. From these, after passing through the front of the boiler, a larger tube was transmitted to the ash-pit. A drain was carried from the side of the furnace to the box situate beyond the fire-bars to supply air, and the ash-pit was furnished with a door which fitted tight so as to prevent any current up through it. By this adjustment, when the ash-pit door was shut, the air for combustion was supplied through the box, heated to a considerable degree, and after passing through the tubes in the boiler, then proceeded to the ash-pit, and rushing up through the fuel kept up the combustion. By apertures left for the purpose and supplied with plugs, the temperature of the air passing into the ash-pit was ascertained.

The apparatus just described is that patented by Mr Bell, with the view of increasing the heating surface exposed to the water in the boiler, and thus increasing the amount of evaporation. I do not, however, bring the experiments forward with the intention of proving the efficacy of this patent. It is not at all my intention to enter into its merits. I had recourse to this apparatus, merely as a means of enabling me to burn the anthracite, so as to make it subservient for the purpose of raising steam, and of keeping up the supply; and thus also to enable me to compare its practical evaporative power with that of bituminous coal.

The mode of conducting the experiments was as follows:—One person was intrusted with the weighing of the coals, the weighing being at the same time checked by another. To another was intrusted the measuring of the water supplied to the boiler. This was done by measuring it by a four-gallon measure into a vessel, in which was the mouth of the pipe connected with the force-pump by which the water was forced into the boiler. The water was in all of the experiments at the temperature of 45°. The height of the water in the boiler was known by a float connected with a wheel and index, which moved easily as the fluid rose and fell. This index was always brought to the same point at the commencement and termination of the trial; and, accordingly, the water in the boiler was always at the same height at these periods. Before beginning, the fire was examined a considerable time after the stoking, and adjusted to the proper height, and it was brought as nearly as possible to the same state at the termination. In this way there was a constant quantity in the furnace at the commencement and termination, and accordingly it was only that which was supplied during the continuance of the trial that was considered as the fuel consumed.

I am aware that objections may be made to this mode of ascertaining the quantity of fuel used, as it is difficult to bring the fire exactly to the same state at the different periods mentioned. This objection, I allow, would apply, were the trials of short duration; but in a six or eight hours' trial, any slight difference in the state of the fire must make a very trifling difference in the total result, where the quantity of fuel employed amounts to several hundredweight.

Scotch Coal.—I conceive it unnecessary to give the result of the numerous trials made with this fuel. They, in general, come very near to each other. I give the particulars of one only, which was conducted with great care, and in the result of which the utmost confidence may be placed. The coal used was from Middlerig, and is considered of good quality.

Time.		Fuel.	Water Evaporated in Gallons.	Temperature of Air thrown into Ash-Pit.	Cinders in Ash-Pit.
H.	M.				
10	35				
11	35	112 lb.	64	415 at 11	
11	35				
12	55	112	68		
12	55				
2		112	68	410 at 1.30	
2					
3	5	112	68		
3	5			350 at 3.30	
4	25	112	80		
4	25				
5	25	112	68		
5	25				
7	35	112	72	300 at 6.15	
9		784	488		52 lb.

In this trial the furnace was supplied with the cwt. of coal at three different stokings. The pressure on the boiler was 17 lb. beyond the atmospheric pressure.

The above table shews, that 784 lb. of fuel were used, and that 488 gallons, that is 4880 lb. of water were evaporated from the temperature 45°, thus giving a result of 6.22 lb. for each pound of coal, at 17 lb. pressure.

On subjecting the coal used in this trial to analysis, I found it to consist of—

Moisture, ...	7.5
Volatile matter, . . .	34.5
Fixed carbon, ...	50.5
Ashes, ...	7.5
	100.0

I may here also state the result of another trial made with a Scotch coal got from a different place, and with another furnace, not fitted with the hot-air apparatus. It was one attached to a ten-horse high-pressure engine. This trial was conducted in the same way as the former, and nearly at the same pressure, due attention being paid to the weighing of the fuel, the state of the fire at the commencement and termination, and also to the height of the water in the boiler, which was ascertained also by a float and index. Without giving the particulars, I may merely mention, that the trial lasted from eleven till four o'clock. The total quantity of coal used was 540 lb., and the total amount of evaporation was 3580, thus making the result 6.62 lb. for each pound of coal used.

In the former trial the water supplied to the boiler was at 45°, in the latter the return-water from the boiler was thrown into the supply tank, and hence the temperature was higher. It was found on an average to be at 170°, which will so far account for the greater amount of evaporation.

In the table given by Richardson, already referred to, the quantity of oxygen necessary for the combustion of the specimens of Scotch coal analysed, is 247, which would make the evaporative power 11.3 compared to that of pure carbon as 12.3. By the test with litharge as proposed by Berthier, I found that the greatest quantity of oxygen required for the combustion of the fuel with which the first trial above given was made, was 205, which would make its evaporative power 9.48. But by the furnace trial only 6.22 were evaporated.

Now $9.48-6.22=3.26$ and $9.48:3.26::100:34.38$; without taking into account the slight difference in temperature between 32° and 45°, there was therefore a loss of 34.38 per cent. of the heat supposed to be evolved, provided the whole of the fuel was consumed; but in this trial the cinders in the ash-pit amounted to 52 lb., and deducting this from the fuel used, there were only 732 lb. actually consumed, which would make the evaporation amount to 6.66, and $9.48-6.66=2.82$, and $9.48:2.82::100:28.97$: thus giving a loss of 28.97 per cent. of the heat evolved, supposing 732 lb. of coal had undergone complete combustion.

I am aware that it may be objected to this trial, that as the loss is so great, the furnace must have been very defective in its construction. I may here state, however, that with the exception of a slight deficiency in draught, the furnace, which had from the commencement of the experiment undergone numerous alterations and improvements, was considered by competent judges as built on the most approved principles; but though there is a vast loss of heat in this instance, we shall find that the result, so far from being a bad one, is rather beyond what has been stated by others. When coal is used for steam-engines, of course the quantity used must depend very much on the power of the engine, its construction, and the use to which it is applied. It has been stated, that taking the average of many trials, from 10 to 15 lb. of coal are required for each horse-power. Now, it is generally allowed that for each horse-power a cubic foot of water must be passed off in steam, the weight of which is 6.22 lb. Accordingly, taking the smallest quantity above given, then each pound of fuel will evaporate 6.22 of water. It must be allowed, that the mode of testing the power of a coal by the quantity used in reference to the power of the engine, is a very fallacious one; at the same time, however, as what I have stated is a practical result, deduced from numerous trials, I bring it forward to shew, that that obtained in my experiment is not, as some may at first sight be inclined to suppose it, below what usually occurs.

Anthracite.—The analysis of several specimens of Anthracite is given by Professor Johnson in the Journal of the Franklin Institute, Pennsylvania, of which the following is a tabular view:—

Water, . . .	3.43	3.26	0.00	2.19	0.40
Volatile Matter,	4.08	1.05	9.60	4.23	5.51
Fixed Carbon,	87.48	91.69	85.34	92.30	91.01
Ashes, . . .	5.01	4.00	5.06	1.28	3.08
	100.00	100.00	100.00	100.00	100.00

Of 12 specimens analysed by Berthier, the mean per centage of ingredients was—

Volatile matter,	7.37
Fixed Carbon,	79.15
Ashes, . .	13.25

A much more minute analysis of anthracite from Wales is given by Dr Schafhaeutl, in a report submitted to the Anthracite Patent Company. The following is the composition of two samples:—

Moisture, .	00.00		0.300	
Carbon, .	92.42		94.100	
Hydrogen, .	3.37	Vol. matter, 5.97	2.390	Vol. matter, 4.6
Oxygen, .	1.43		1.336	
Nitrogen, .	1.05		0.874	
Sulphur, .	0.12		trace	
Ashes, .	1.61		0.932	
	100.00		99.932	
		Loss,	.068	
			100.000	

In these specimens, the quantity of volatile matter varies from 4.31 to 9.6. It may be stated to be on an average about 5 per cent. The greatest amount of carbon is 94.1. It is generally allowed, that anthracite contains on an average about 92 per cent.

The coal with which my trials were made was much mixed, some parts being of good quality, others containing an admixture of impure coal, having a good deal of iron-pyrites and of earthy matter. In its composition it resembled the specimens analysed by Berthier. Its specific gravity varied from 1303.5 to 1406.6. To procure an average sample for analysis, a considerable quantity was taken from the heap at random, which was bruised, and from this a smaller quantity was removed and then reduced to powder. This, on analysis, yielded

Moisture, . .	4.5
Volatile matter,	13.3
Fixed Carbon,	71.4
Ashes, .	10.8
	100.0

Anthracite of good quality burns with very little or no flame, and without smoke. When first thrown on the fire it decrepitates, and small pieces are thrown off from it, which choke up the spaces between the fire bars, and also the flues, and in a great measure prevent the combustion from going on. Though it can be burned in common grates, yet, unless consumed in some particular way, it does not answer well for raising steam, owing to the slowness of the combustion. A patent was lately taken out by Mr Player for effecting the consumption of anthracite, by previously heating the coal, by making the part of the apparatus containing the fuel to be supplied to the furnace, pass through the boiler, by which means it derives heat from the water. The method I adopted was to supply the coal with air previously warmed, which was done by making it traverse the apparatus already described, and by which the temperature was on an average about 350°. By this contrivance, the decrepitation was almost entirely prevented, the combustion was rapid, and the steam was easily kept up. I have already mentioned that the anthracite I employed was of inferior quality; it contained not only a large quantity of ash, but also of volatile matter, the latter of which made it burn with flame, while the former, owing to the intense heat generated, formed a tough slag, which adhered firmly to the bars, and in a great measure retarded the combustion.

The trials were conducted with the coal without being picked. Numerous experiments were made with the furnace when fitted up with the flash-flues, but with this the draft was evidently deficient, and the furnace was therefore taken down and rebuilt with the returning flue, and an addition made to the chimney to bring it to the height formerly mentioned, by which I found that the evaporation was considerably increased. Being totally unacquainted with the burning of anthracite, it was necessary, before beginning to record the results of any trials with accuracy, to find out the best means of accomplishing the combustion. As good anthracite burns with little or no flame, and as, of course, there is no bituminous matter, which causes the swelling of other kinds of coal, I found that the furnace bars must be brought nearer to the bottom of the boiler, than when Scotch or English caking-coal is employed. In my experiments I had them generally at the distance of about 8 inches. The fuel was in general about 4 inches in depth, and in stoking it was thrown as equally as possible over the whole surface of the fire.

It is unnecessary to state the result of the numerous trials made after having satisfied myself that I had effected completely the combustion of the fuel. I will confine my remarks to one only, performed when the furnace was in good working condition, the pressure on the boiler being 17 lb. It was continued from ten in the morning till half after six in the evening. Subjoined is a tabular view of the working, which will shew the mode of proceeding, and the regularity with which the fuel was supplied, and how the evaporation went on.

Time of Stoking.		Fuel used.	Water evaporated in Gallons.	Temperature of Air to Ash-Pit.	Cinders in Ash-Pit.
Hrs.	Min.				
10	0			240	
10	30				
11	0				
11	30	112	88		
12	0				
12	30				
1	0				
1	30	112	88		
2	0			220	
2	30				
3	0				
3	30	112	88		
4	0			270	
4	30				
5	0				
5	30				
6	0				
6	30	112	92	270	
8	30	448	356	Average, 250	40½ lb.

In this trial the total quantity of coal used was 448 lb., and the evaporation amounted to 3560 lb.; there were therefore evaporated 7.94 lb. for each pound of coal. The evaporative power of this anthracite, when tried by the litharge test, was found to be 10.78, compared to that of carbon as 12.3.

Now $10.78 - 7.94 = 2.84$, and $10.78 : 2.24 :: 100 : 26.34$; there was therefore a loss of 26.34 per cent., supposing the whole of the coal thrown into the furnace were consumed. The coal found in the ash-pit was 40.5 lb., thus making the quantity actually consumed amount to 407.5, which would make the evaporation 8.73 for each pound of coal undergoing combustion. The evaporative power of this fuel, it has been already stated, as tried by the litharge test, was 10.78.

Now $10.78 - 8.73 = 2.05$, and $10.78 : 2.05 :: 100 : 19$; there was therefore a loss of 19 per cent. of the total heat evolved, supposing the 407½ lb. fuel consumed to have undergone complete combustion. With the Scotch coal the loss amounted to 28.97 per cent.

So far as I know, there is only one instance on record of the evaporation produced by anthracite, as tried by actual combustion; it is that contained in the report lately given in by Dr Schafhaeutl to the Anthracite Company. It was conducted with coal of good quality, and with a furnace and boiler fitted up with the patent apparatus of Player, by which, as already stated, the fuel is gradually heated before it is burned, and by which, it is said, the decrepitation is prevented, and the combustion is rendered complete and effective. This coal was on analysis found to contain 92.42 per cent. of fixed carbon, 5.97 of volatile matter, and only 1.61 of ashes. Its evaporative power, calculated from its composition, is stated by Schafhaeutl to be 12.3, or the same as pure carbon. When consumed in Player's furnace, it was found that by the combustion of 372.28 lb. there were evaporated 3934.3 lb. of water, under a pressure of 13.3 lb., thus giving 10.56 lb. for each pound of coal actually consumed, for in this result the cinders were deducted. This is much greater than I obtained, but the smaller amount in my trial is, I conceive, easily accounted for. The coal I used, as tested by litharge, had an evaporative power of 10.78, while that used by Dr Schafhaeutl was 12.3. In my trial the loss of heat was 19 per cent., in the other it was only 14.1.

$$(12.3 - 10.56 = 1.74, \text{ and } 12.3 : 1.74 :: 100 : 14.1.)$$

Though the amount of evaporation in my trial was small, yet there is not much difference according to the calculated evaporative power of both it is only 4.9 per cent. This may also in a great measure be accounted for. The pressure at which my trials were made was 17 lb., that of Schafhaeutl was 13.3 ; and though, as is well known, the heat necessary to convert water into steam is the same at all pressures, yet in practice it is allowed that the higher the pressure the greater is the heat required, owing to a loss occasioned by different circumstances. Making allowance for pressure, according to Schafhaeutl, the quantity that would have been evaporated in his trial, supposing the temperature at 212°, would be 11.11 ; in mine it would have been, according to the same rule, 9.39 ; which would make the loss of heat in the former amount to 9.6 per cent., in the latter to 12.3 per cent. ; thus giving a difference of only 3.3 per cent. As to this greater deficiency, it was most probably owing to the inferior quality of the fuel, perhaps also owing to the defective draught of the furnace, but more particularly to the former. When, for instance, a coal contains much incombustible matter, a greater deduction must be made from the heat that is likely to be available, than in the ratio of the per-centage of the non-inflammable materials, and especially if these contain metallic matter, for they then form slag on the bars, and prevent the due transmission of air through the furnace. Hence, which is a very great impediment to the practical application of the heat, it is necessary from time to time to remove the slag from the bars, by which not only is the fire disturbed, but what is a very great drawback, during the whole of the time that the fireman is employed in doing this, cold air is rushing into the furnace, and impinging on the boiler, lowers the temperature, and consequently reduces the evaporation. This happens with common coal, but in addition to this, when anthracite is disturbed in this way, the combustion is materially affected by the cold air. In my experiments this frequently occurred. It was necessary to remove the slag, and as it adhered to the bars, it required a considerable time to get it done. I have no hesitation, therefore, in stating, that had an anthracite with less volatile ingredients, and more especially with a smaller quantity of incombustible matter been used, there would have been a greater amount of evaporation, of course a smaller loss of the heat evolved by the combustion.

The next set of experiments was made with the view of ascertaining the evaporative power of English caking, compared with common Scotch coal. Of course, the strength of these must vary according to circumstances, the results now to be stated must therefore be considered as applying only to those on which the trials were made. The English coal used was imported at Leith and said to be of the best quality; being got direct from the vessel, it may be considered as free from admixture. The Scotch coal with which it was compared was from the neighbourhood of Edinburgh, and is generally reckoned good.

I regret that I had not an opportunity of trying these coals in the furnace used for the anthracite. I was obliged to have recourse to one of much smaller dimensions, and in which, though the combustion was as complete as frequently occurs, yet there was a considerable waste of heat. I give these experiments, therefore, not with the view of shewing what can be done by these coals; but as the waste would affect both fuels equally, the results will prove their comparative evaporative power in practice.

The boiler employed was made of thin sheet copper. It was of the waggon form, with a flue returning through it, and capable of holding 50 gallons of water. The fire surface of the furnace was 16 inches by 14, and the surface of the boiler exposed over the fire and otherwise to the heated products of combustion, was in all 18 feet. As the boiler was open, of course the evaporation was conducted under the usual atmospheric pressure. The water supplied was in all the trials at the temperature of 50°. It was measured when thrown into the boiler, and to check the results, a graduated gauge was attached to the boiler, by which the height of the fluid within could be ascertained, and the amount of evaporation known. Six trials were made with the Scotch coal, each lasting nearly nine hours. It is unnecessary to give the results of the whole. I select one which was conducted with great care. The following is a tabular view of the working:—

Time.		Coal in lb.	Water evaporated, in lb.
Hrs.	Min.		
11	0	4	
11	30	4	
12	0	4	
12	30	4	
1	0	4	80
1	30	4	
2	0	4	
2	30	4	
3	0	4	100
3	30	4	
4	0	4	
4	30	4	
5	0	4	114
5	30	4	
6	0	4	
6	30	4	
7	0	4	96
7	15		10
8	15	68 lb.	400 lb.

In this trial, which lasted for $8\frac{1}{4}$ hours, 4 lb. of coal being supplied regularly each half hour, there were used in all 68 lb., and 400 lb. of water were evaporated, thus giving 5.88 for each pound of coal.

Three trials were made in the same way with English coal, due attention being paid to the supply of fuel to the furnace, so as to secure the proper combustion, and as the quantity put in at a time was small, the combustion was tolerably regular. I select one of the trials, with which the others very nearly agreed.

Time.		Fuel in lb.	Water evaporated in lb.
Hrs.	Min.		
10	0	3	
10	30	3	
11	0	3	
11	30	3	
12	0	3	106
12	30	3	
1	0	3	
1	30	3	
2	0	3	78
2	30	3	
3	0	3	
3	30	3	
4	0	3	100
4	30	3	
5	0	3	
5	30	3	
6	0	3	90
6	24		26
8	24	51 lb.	400 lb.

In this trial 51 lb. of coal were used, and 400 lb. of water evaporated; hence, for each pound of coal used, 7.84 of water were evaporated.

Different results were obtained in some of the other experiments, some of them giving a lower amount of evaporation. Taking the average of the whole it very nearly agreed with the above; according to it, 5.88 : 7.84 : : 100 : 133.

The average of the trials properly conducted gives a result as 100 to 131.5. We are not therefore far from the truth if we take it as 100 to 133, that is as 3 to 4.

The composition of the Scotch and English coal was found to be

	Scotch.	English.
Moisture, . .	7.5	1.5
Volatile Matter, .	34.5	29.5
Fixed Carbon, . .	50.5	67.0
Ashes, . .	7.5	2.0
	100.0	100.0

The table given by Mr Richardson already quoted, shews that the evaporative power, calculated according to the quantity of oxygen required for combustion, ought to be in the ratio of 100 to 109.6. In the experiments I have made on the coals used in the trials above referred to, I found the evaporative power by the litharge test to be as 100 to 105.2, numbers not differing much from those of Richardson. But the evaporative power by the furnace trials was as 100 to 133, which is very different from the other. It becomes, therefore, an important question, what is the cause of this difference? for if we can solve this, it may perhaps lead to the means of making the available power of the Scotch approximate to that of the English coal. To this I will afterwards refer.

On viewing the results of the experiments now detailed, one remarkable circumstance at once strikes us,—it is the great practical evaporative power of anthracite over the other kinds of coal, which, though they contain less fixed carbon, yet have much more volatile matter, the hydrogen of which, I have already said, gives out thrice as much heat as carbon does. In the anthracite which I employed, the fixed carbon amounted to 71.4 per cent., in the caking coal it was

67, and in the Scotch coal only 50.5. Now, with regard to the comparative practical evaporative power of the Scotch and English coal, I have stated them as 100 to 133, and as 50.5 : 67 : : 100 : 132.

Again, with regard to the Scotch and anthracite, I have given the evaporative power as 6.66 to 8.73, and 50.5 : 71.4 : : 6.66 : 9.41. Thus, not only is the evaporative power in practice in the ratio of the fixed carbon, but there is a very remarkable approximation in the evaporative power to the proportion of this ingredient in each.

In the anthracite the volatile matter, independent of the moisture, was 13.3, in the English it was 29.5, and in the Scotch it amounted to no less than 34.5 per cent. We would therefore naturally expect that the English and Scotch coal would have a greater evaporative power, whereas that which has the least of these volatile ingredients has the greatest. It is evident from this, that, in the combustion of fuel in furnaces as commonly constructed, the loss of the heat evolved is occasioned in a great measure by the volatile matter; much of these we know is consumed, but even in the best constructed furnaces, there is an escape of a considerable proportion, owing to the want of a due supply of air, or from want of the proper temperature even though air is present; indeed it is chiefly owing to the latter, for we never find that the air that has passed up through the fuel is entirely deprived of its oxygen.

Numerous methods have been recommended for securing the perfect combustion of all the gaseous matter, such as the cautious introduction of the fuel near the front of the furnace, instead of throwing it carelessly over the whole heap in a state of combustion, by which the gradual discharge of the volatile matter is occasioned, and which, being thus expelled and passed over the ignited fuel, should be burned, provided air in sufficient quantity is present. Many others have also been proposed,—such as that patented by Mr Williams of Liverpool, which consists in allowing air to flow by small streams into the furnace behind the bridge, by which the volatile matter that has escaped combustion is to be consumed. Another is that lately introduced by Mr Ivison of this place. It consists in throwing in small jets of steam at the front of the furnace, immediately over the surface of the fuel, at the same time admitting air, at the furnace-door or otherwise, also over the fuel. It is not my intention to enter into a discussion as to the merit of these processes for effecting the objects proposed. I will, perhaps, at a future period make some remarks regarding the latter, with which I have had many opportunities of operating, and with which I have obtained the most unlooked-for, but, at the same time, the most satisfactory results. Keeping out of view these and other contrivances of a similar nature, it is evident that the power of anthracite is far beyond that of other kinds of fuel, more particularly when it is of good quality, that is, rich in fixed carbon, simply because there being little, indeed in some kinds of it we may say no, volatile inflammable matter, the whole of the combustible substance is consumed, and the only loss of heat arises from that which must pass up the chimney, and by which a draft is secured, besides what is given off in the flues and otherwise by communication, through the materials of the furnace.

There is still another point to which I wish to advert. I have already alluded to the great practical evaporative power of those fuels containing much fixed carbon. On still farther viewing the results of the experiments which have been detailed, we find that not only does the evaporative power bear a ratio to the proportion of this element in the different fuels, but it would appear to be almost in the exact ratio of its quantity in each. Thus the fixed carbon in the anthracite I employed was 71.4 per cent.—now, taking that of pure carbon as 12.3

$$\text{as } 100 : 12.3 :: 71.4 : 8.77,$$

the practical working of the fuel actually consumed was 8.73. The fixed carbon in the Scotch coal was 50.5 per cent. and

$$\text{as } 100 : 12.3 :: 50.5 : 6.21,$$

the practical working was 6.66. In the anthracite used by Dr Schafhaeutl, the fixed carbon was 92.42,

$$\text{and as } 100 : 12.3 :: 92.42 : 10.36,$$

the practical working was 10.56. The evaporative power in practice seems therefore to be very nearly as the quantity of fixed carbon in the fuel. I am strongly inclined to think that this will be found to be the case with all fuels in their natural state; and if so, then in using fuel in furnaces as now constructed, supposing the combustion to be as perfect as we can expect to make it in these furnaces, we shall find the practical evaporative power to be according to the per-centage of fixed carbon. If this should be found to be the case, then we have an easy method of knowing the practical evaporative power of a fuel—which is merely to ascertain the quantity of fixed carbon. This may be done by exposing a certain weight of it in a covered crucible for some time to a red heat, the time depending in a great measure on the quantity used; then when cold to weigh the residue, and afterwards to expose it to a red heat in an open crucible, or on a platinum foil, till it ceases to contain any admixture of carbon, which is easily known by the appearance of the residue; the loss of weight thus sustained, deducted from what was left by exposure to heat excluded from air, will give the weight of fixed carbon. Thus 100 gr. of the Scotch coal, used in my trials, exposed to heat in a covered crucible, lost 42 of moisture and volatile inflammable matter—the 58 gr. of residue, consisting of fixed carbon and incombustible substance, when heated in air, left 7.5—the loss, amounting to 50.5, was of course fixed carbon, which was consumed by its union with oxygen.

From the few experiments I have made, I do not feel that I am warranted in drawing it as a general conclusion, that the practical evaporative power will be found to be as the fixed carbon, but from what I have seen, I have every reason to think that is either so, or very nearly it; of course it will require a more extended series of experiments, made on a larger scale, before such a conclusion can be proved to be correct. The subject is important, and well worthy of farther prosecution, and I trust that those who have opportunities for testing the practical evaporative power in furnaces properly constructed, will, from what I have advanced, have their attention drawn to it. Till this is properly tried, it is useless to enter into speculation regarding the heat evolved by combustible matter; at the same time, however, there is one point to which I cannot refrain from alluding. Whether or not it is found that the practical evaporative power is as the fixed carbon, it must be allowed that in the trials, the results of which

I have given, it bears a close approximation to it;—then a question arises, What becomes of the heat evolved by the combustion of the volatile ingredients? We know that the greater part of these is consumed, and they must give out heat by their combustion; if so, how does it happen that the evaporation is not greater than what should be occasioned by the consumption merely of the fixed carbon? This may, to a certain extent, be explained by the volatilization of the elements that assume the gaseous form. When heat is applied to coal, by which it is made to unite with oxygen and itself give forth heat, a part of that heat must be spent in enabling the hydrogen, with the equivalent proportion of carbon, to assume the elastic form. Now, if the whole of this was to escape without undergoing combustion, while, at the same time, the fixed carbon only is burned, we can easily conceive that the evaporative power of a coal thus consumed, would be less than in the ratio of the fixed carbon it contains; but this is never the case in properly constructed furnaces; a great part of the gaseous matters is consumed, and they, by their combustion, again evolve the heat which was absorbed by them when assuming the elastic form. Perhaps in furnaces as now constructed, even on the best principles, the whole, or nearly the whole, of the heat thus disengaged is so required; of course, the greater the proportion of volatile matter, the greater will be the abstraction of heat while they become gaseous, and, consequently, it will require the consumption of a proportional part to supply the heat thus abstracted. While, therefore, the heat evolved by the combustion of the fixed carbon is a fixed quantity in each fuel, and as, when the gaseous matter evolved is great, a proportionally smaller quantity of it is generally consumed, hence the lower evaporative power of those fuels containing much of the elements that yield the volatile matter; and, again, as it is much easier to effect the complete combustion of fixed carbon than of all the ingredients, both fixed and volatile, of a bituminous coal, hence the value of those that contain much of the fixed element, when consumed in furnaces as now usually constructed. I know that what I now assert is at variance with the opinions entertained on this point by practical engineers, who generally think that a great deal depends on the flame of the volatile matter. If, however, any reliance is to be placed in the experiments the results of which I have detailed, I think they will be forced to allow that I am correct in what I assert: that the greater the proportion of fixed carbon in a fuel, the greater will be the practical evaporative power. In a national point of view, then, now that the demand for fuel has become so great, and that for long voyages it is of the utmost consequence to have the fuel powerful, so as to occupy as little space as possible, or rather, if I may be allowed the expression, to have a greater quantity of an evaporative power stowed away in the same space, it is of vast importance that attempts should be made to introduce the anthracite fuel.

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*Notice of Delafosse's Memoir on Crystallography.**

It is known that when we compare the physical characters of the forms which compose the different systems of crystallization with their proper geometrical characters, we arrive at the following general fact: that, in a crystal, all the parts of the same geometrical species are modified at once and in the same manner, or, reciprocally, that the parts of different geometrical species are modified individually or differently. We meet, however, with certain bodies in which the modifications take place differently than in others, insomuch that all the parts geometrically alike are no longer found to be modified in the same manner. Now, says M. Beudant, it has happened with regard to this observation, what we too often witness in the sciences—observers have seen only one possible conclusion, without suspecting that another may exist quite as admissible. It has been simply concluded, in this instance, that these circumstances constituted an exception to the law of symmetry; and trusting implicitly to the adage,

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* At a meeting of the Academy of Sciences of Paris on the 25th January 1841, M. Beudant read a report, drawn up conjointly by MM. Brongniart and Cordier, on a crystallographical memoir presented by M. Delafosse, of which the above is a notice.

happily rejected by the exact sciences, that *the exception confirms the rule*, no farther progress has been made. M. Delafosse has now come to a conclusion diametrically the opposite to this. There is no anomaly, he affirms, in the law of symmetry. That law remains, in this case, in full force, and it is the identity of the parts which is not complete. There is indeed a perfect geometrical, although there is not a physical identity, and thence result the differences we witness; or rather these differences ought to make us modify the notions hitherto entertained respecting the internal structure of crystals, which have been considered only on their purely geometrical relations.

It is in this point of view that M. Delafosse has given a general exposition of the rationale of the various changes which it has appeared to him are necessary to be made on the crystallographic theory of Haüy. He has applied his principles to divers natural substances, *boracite*, *common pyrites*, *tourmaline*, *quartz*, and *beryl*. We shall give a comprehensive idea of his observations and conclusions.

Boracite and *common pyrites* are referable geometrically to the cube, like many other substances, but they present at the same time certain peculiarities which distinguish them. In *boracite* there are only four solid angles in the cube which are modified altogether in the same manner, and as the eight solid angles of a cube are geometrically identical, it has been concluded, from the time of Haüy to the present day, that we had here an exception to the law of symmetry. M. Delafosse, reasoning otherwise, has come to the conclusion, that if the solid angles of this substance are geometrically identical, they are not so physically; which means that the geometrical cube of boracite is not composed molecularly in the same manner as the cube met with in many other substances. One can imagine, in fact, that a cube may be composed, geometrically speaking, in a multitude of different ways, for example, of small cubes, small tetrahedrons, minute rectangular prisms, &c. If the crystals were always simple, we could never perceive these differences, at least by observing the external form alone; but the modifications they present, and to which the optical and acoustic properties may be added, ought to lead us to the determination of this molecular form.

In the present case, the apparent anomaly leads us to adopt the regular tetrahedron as the molecule, and to conceive these small solids arranged in rows, in such a manner that at one of the angles of the resulting cube there is found a base, while at the opposite angle a summit presents itself; from this it will follow that the two opposite angles, which are geometrically identical, are found completely different in a physical respect, and the law of symmetry will appear in all its force in such a system, if, as nature presents it to us, one of the angles is modified differently from the other. In this supposition of molecular forms and arrangement, the edges of the cube are all geometrically and physically identical, since they all correspond to the edges of the tetrahedron. This also takes place with the modifications of boracite, for the twelve edges of the cube are found to be modified in it at the same time and in the same manner.

If we study, comparatively, substances in which all the angles of the cube are modified altogether and in the same manner, for example *fluor-spar*, we will conclude that all the parts are geometrically and physically identical, which leads us to admit the cube itself as a molecule, since it is the only solid that can fulfil this condition.

Comparing the conclusions relating to *fluor-spar* with those stated in reference to boracite, we at once perceive that nature presents us with two kinds of cubes; one sort, formed of tetrahedrons, which possess the character of having four of their solid angles physically different from the four opposite ones; the other, formed of cubical molecules, or octahedrons, in which all the angles are identical both in a physical and geometrical sense. But besides these there is a third.

The crystallization of *pyrites*, as well as that of *grey cobalt*, is referable to the cube, but the modifications in these cases present an inverse order of things from what takes place in boracite. In the latter substance the solid angles are physically of two different sorts, and the edges are identical in every respect. In the two others, every thing is the reverse of this; the angles are all identical, and the edges are not so.

In fact, these edges are modified in a different manner, and in such a way as those may be which represent the three dimensions of a right rectangular prism. The cube of the pyrites cannot therefore be formed either of the small ordinary cubes, which would render all the parts of it identical each to each, or of small tetrahedrons, which would render the edges identical by forming two kinds of solid angles. This kind of cube is necessarily formed of small solids of which the three dimensions are different, whether we admit geometrical differences, or suppose that physical or chemical differences exist. The molecule which must be admitted in this species is the limit of the cube and of the right prism, as the solid which would result from the replacing of the superior edges of a rhombohedron in which the plane angles should be of 60° and 120°, would be found to be the limit of the cube and the rhombohedron. It may be conceived that at these limits the forms will have properties analogous to those of the allied solids, and that then they differ from each other in respect to symmetry.

Thus, then, we have three kinds of very distinct cubes in the substances which crystallize in the cubical system, or, if the expression be preferred, three systems of cubical crystallization. We may even see the possibility of a fourth and of a fifth for the limit of the cube and the rhombohedron, of the cube and of the square prism, &c. All the other crystalline systems at present admitted appear to present analogous circumstances, and M. Delafosse points out many substances which ought particularly to attract attention in this light. But, in his present memoir, he only considers the rhombohedral system by treating of the *beryl*, *quartz*, and *tourmaline*, referring for comparison to the *carbonate of lime*.

All these bodies, as is known, may be referred theoretically either to the rhombohedron, or to the prism with regular hexagonal bases, or finally to the dirhombohedron (bipyramidal dodecahedron); but when we take into consideration the peculiarities they present, and that, instead of imagining in some of them constant anomalies,—an expression unquestionably very singular—we see nothing but positive facts, to which we may henceforth rigorously apply invariable epithets, it will be admitted that the form taken as the point of departure must, in each of these bodies, possess a special molecular composition.

Rigorously following up the principles adopted, M. Delafosse endeavours to find, in divers substances, molecules which should be in relation with their physical properties; but here it is difficult to materialise all the forms so as to designate them by geometrical names, as he was able to do in regard to the cubical systems. M. Delafosse is of opinion, that, after obtaining divers data, we can only arrive at a knowledge of the kind of molecular form of a substance, and that, in order to obtain the true type, it is necessary to add to it the atomic relations and chemical composition. He intimates that he has already obtained some results of this nature, and he proposes to present them to the Academy in another memoir.

M. Beudant concludes his report by observing, that if it be true, as has been alleged, that M. Delafosse's ideas are not altogether new, and that the germs of them may be found in different works, it ought to be added that these imperceptible germs had remained wholly unproductive; and if there be any honour to be claimed, it is due to him who has rendered them fruitful. The Academy agreed to his motion that M. Delafosse's memoir should be inserted in the *Recueil des Savants étrangers.*

Observations on the Glaciers of Spitzbergen, compared with those of Switzerland and Norway. By C. Martens, M.D. Member of the Northern Commission. (Continued from page 177.)

Causes of the annual Demolition of the Spitzbergen Glaciers.—Dr Latta* positively affirms that the glaciers of Spitzbergen always terminate at the margin of the sea. Scoresby † and

* L. c. p. 102 and 260.

† L. c. p. 97.

Keilhau* are less exclusive. "When the sea is not very deep," says the English navigator, "the glacier advances to a certain length; when it is deep and usually covered with ice, as is the case in Baffin's Bay,† then the glacier advances very far into the sea, till it reach the bottom of the water at a depth of some hundred feet, and, by breaking into pieces, it produces those mountains of ice which are so often met with to the west of Greenland." A savant, who has not visited the north, but who has attentively studied the glaciers of Switzerland, M. André du Luc, has broached a mixed opinion, founded on the data derived from the voyages of Captain Ross. It is to the following effect, as copied literally from his Memoir:‡ "Immense glaciers, formed in the gorges and valleys which open to the sea, are urged forwards, and enter the water to a great depth. For some time, the ice supports itself by its cohesive power; but when the mass which is without support becomes too great to be maintained by this power alone, it breaks off, and sinks into the sea to four-fifths of its height, and, if the water is sufficiently deep, the mass floats. The glacier continues to advance into the sea, new masses are detached, and thus, in process of time, chains of ice-mountains are formed. If the sea is not of great depth in a bay, the glacier advances, *always resting upon, and sliding along, the bottom of the sea;* and it is thus that we witness immense glaciers extending for many miles into the sea, without any portion of their mass separating. They have assumed, so to speak, the place of the sea in the bay."

Leaving for the present the consideration of the glaciers of Baffin's Bay, to which I shall afterwards return, I shall content myself by shewing,—

1*st*, That the glaciers situated at the bottom of the Spitzbergen bays do not terminate at the margin of the sea, but advance into it. 2*d*, That they never slide along the bottom of the sea, but that they slope towards it, so that the lower face of the glacier is in contact with the surface of the water.

* L. c. p. 135. † L. c. t. I. p. 108.

‡ Notice of the Ice in Baffin's Bay, Biblioth. Univ. de Genève, t. xiii. Litterature, p. 288. 1820.

I. The following are my proofs in support of the first of these two propositions.

1*st*, If we examine a plan of the bays of Spitzbergen, and in particular those of Bell Sound and Magdalena Bay, drawn up by the officers of the Recherche, we will perceive that the vertical walls of the glaciers always form nearly straight lines which transversely close up the bottom of the bays. Now, by describing a curve concentric to that which would pass by the summits of the mountains whose base is marked by the glacier, we may trace the contour of the shore-line; for in Spitzbergen there is no flat beach. We will thus see that the bottom of the bays is rounded as in every other place. The ice-wall which closes them up is the cord of the arc of a circle formed by the rounded extremity of the bay, and the segment of which is occupied by the lower part of the glacier. Thus the seven glaciers fill small bays, and the mountains which separate them are so many promontories.

2*d*, I could never discover in the *middle* of the base of the glacier's escarpement the ground on which it should rest, while the subjacent rock was quite visible at the extremities of this base. Let it not be objected to this, that the sea washed the foot of the mass; for at ebb-tide an interval of about a metre might be distinctly observed between the ice and the surface of the water.

3*d*, During our two visits to Bell Sound and Magdalena Bay, we never saw the two glaciers break or gradually retire. How could fragments of ice be continually falling into the sea, if the glacier did not advance beyond the water-line? This continual demolition becomes an inexplicable matter, if we suppose that the glacier terminates at the edge of the sea.

4*th*, These masses being unceasingly urged forward by those which are above them, why do they stop just at the margin of the water which opposes no obstacle to their progress?

5*th*, The considerable depths of the sea, varying from 32 to 123 metres, which we find near the wall of the glaciers, prove that it is very far from the margin.

II. It remains for me to shew, that the lower part of the glaciers of Spitzbergen advances *on* the sea, and does not slide along the bottom; in a word, that it forms a segment of a circle, the arc of which, as well as the two extremities of the cord, rest on the bank.

1*st*, If the glaciers descended to the bottom of the sea, we ought to meet with some of them whose upper surface was washed by the waves; this surface would form an inclined plane which would form an angle more or less open with that of the sea. But this we never witness; the glacier always terminates in a vertical wall of considerable height.

2*d*, When it is ebb-tide, we would not be able to distinguish any interval between the ice and the water; an interval which is everywhere observable along the glacier. But, it may be said, the glacier rests on the bottom of the sea, and the vertical wall which terminates it is entirely hollowed out from the level of its surface by the action of the waves. If this hypothesis were true, we would notice at the foot of the glaciers hemispherical cavities analogous to those in all the floating masses of ice, or we would always perceive the submerged ice beneath the water which was washing it away. But nothing of this nature appears in these glaciers. The line terminating their lower surface is perfectly straight, continuous, horizontal, and consequently parallel with the level of the sea. Of this I assured myself by sailing round the glacier of Magdalena Bay. I entered with the boat into the creeks formed in the glacier itself by the falling down of the ice, and in no instance did I notice submarine ice.

3*d*, When a considerable mass becomes detached from the glacier, it always extends to its entire height; it is, so to speak, a portion of the glacier separated from the rest. If it touched the bottom it would slide or tumble merely; but we see it for the most part plunge almost vertically, and remain for a few seconds under the water before it reappears at the surface.

4*th*, By ascending the terminal glacier of Magdalena Bay, I saw distinctly that it was depressed towards the middle, and very concave in all that part which slopes to the sea.

5*th*, In proportion as the glacier is broken, the escarpment in which it terminates has the tendency to describe a curve more or less parallel to that of the shore, because the central parts, which are not supported, are the first to break down.

6*th*, Another argument is derived from the height of the masses of ice detached from the glacier of either of the two bays in question, and which floated around our vessel. Their height above the water never exceeded that of the bulwarks of the Recherche, that is to say, about 4 metres. Consequently, the part submerged could not be more than 28 metres, even admitting the data of Mr Lyell,* the individual who, of all observers, gives the highest figure, by affirming that seven-eighths of the ice are sunk in the water. Thus the most elevated block of ice was in all only 32 metres high, an elevation which precisely corresponds to that of the two glaciers at the bottom of Bell Sound and Magdalena Bay, *above* the level of the sea. If they rested on the bottom, it would be entirely otherwise. M. Pacini, attached to our vessel, found soundings of 32 and 64 metres (mean 48 metres) at the very foot of the great glacier of Bell Sound. At the distance of 80 metres from that of Magdalena Bay, I constantly found between 102 and 103 metres. By supposing that the base of the glacier rested on the bottom, it would follow, in regard to Bell Sound, that floating masses would occur whose total height would be about 80 metres, viz., 10 metres above and 70 below the surface, and they would likewise be all at rest grounded on the bottom, which is not the case. At Magdalena Bay the floating ice would be 145 metres in total height, viz., 18 above and 127 below the water. They would almost all reach the height of the great scuttle of a vessel's mast, and none of them could float out of the bay. Now the floating masses of ice did not take the ground unless at depths of about from 5 to 10 metres; they often floated round the vessel, where there was never more than about 17 metres depth of water. Finally, the reflux of the tide carried them all out of the bay, and they went in every direction where the depth approached 15 metres.

7th, It is easy to explain why the glacier does not enter the sea, if we consider the temperature of the surface of the water in summer. It is in this season only that the glaciers advance, for in winter the sea is almost always frozen, and must therefore resist their progress. In proportion as they descend (and this they do very slowly) they come in contact with a li-

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* Hoffmann, Physikalische Geographie, t. i. p. 298.

quid whose temperature at Bell Sound has never been below $+1^\circ.45$ C. and whose mean is about $+3^\circ.5$. At Magdalena Bay the minimum is $+0^\circ.9$, the mean $+1^\circ.34$. As the ice is washed by water at such temperatures, it melts; however, the glacier continues to descend, supporting itself on the two sides of the bay. So true it is that the glaciers which are at the edge of a rectilinear shore either advance very little or not at all into the sea. Such were the glaciers of Entrée at Magdalena Bay, and the southern arm of that at the bottom of the bay; they never went beyond the water-line, and never broke down during our stay.

If it be asked how it is possible that these immense masses remain thus suspended, I should answer that they rest at the sides and behind on the shore, and that they are held by the upper part of the glacier which sinks deeply into the earth. Besides, when the tide is at the full, the water which flows beneath them contributes to their support. When these different means prove insufficient, the parts most advanced and separated from the others by crevices fall down; whence the continual breakings which we have witnessed.

Arches in the Glaciers of Switzerland.—The formation of those vaults or arches of ice, which are admired at the lower extremity of the glaciers of Switzerland, is a phenomenon altogether analogous to that of which I have spoken. These grottos, according to Ebel,* do not exist in winter, but in spring; the waters produced by the melting of the snow penetrate into the crevices of the glacier, then issue by its extremity, and melt sometimes a mass of ice 30 metres in height, by from 15 to 25 in breadth. These vaults remain during the whole summer. The glaciers of Spitzbergen cannot last so long; in proportion as they advance they sink and are lost in the sea. Hence the immense quantities of floating ice which cover the Icy Sea, and sometimes advance to 45° of latitude. How can we explain their prodigious number by the simple erosion of glaciers, situate for the most part at the bottom of bays where the sea is calm, and sheltered from the winds and waves of the open ocean? But if we reflect that du-

* Anleitung die Schweiz zu bereisen, t. iii. l. 120.

ring the summer a part of the Spitzbergen glaciers falls into the sea, we will easily understand from whence those fields of fresh-water ice which float in the icy ocean proceed. Every day the bays which we visited were entirely covered with new masses of ice which were carried out by the tide to the sea. When the thermometer rose a few degrees above zero, and the air was filled with fog, enormous masses then became detached from the glacier and precipitated themselves with a crash into the waves, which made our vessel roll about, and raised the waters many metres on the bank, as during a spring-tide.

Glaciers and floating Ice of Baffin's Bay.—We know that in Baffin's Bay the floating masses of ice are much higher than in the seas of Spitzbergen; they form true mountains. Scoresby* speaks of some 45 metres in height. These ice-mountains very often rest on the bottom, and M. A. de Luc thinks, with reason, that they cannot melt at the bottom because the temperature of the sea is there below zero. But, according to the observations of Captain Ross,† there are glaciers in these latitudes the height of which is about 300 metres above the surface of the water. A fragment detached from such a glacier might then form mountains 73 metres above the water. It is not, therefore, necessary to suppose, as M. de Luc has done, that the glaciers slide over the bottom of the sea, in order to explain the height of the mountains of ice in these latitudes. But considerations drawn from the temperature may lead us to incline to his opinion. In fact the temperatures in these latitudes are almost always below zero, even at the surface. Ross has given meteorological tables in connection with the account of his voyage in the Isabella and Alexander. The temperature of the surface of the sea was noted four times daily. On summing up the observations made between 63°.49′ and 75°.44′ N. Lat. during the months of June, July, August, and September, I find only thirty-one days in which the temperature of the sea was above zero; during the rest of the time it was below it. The maximum was +1°.11 C.;

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* Journal of a Voyage to the Northern Whale-Fishery. 1823.

† L. c. p. 141.

the minimum 1°.11. In the seas of Spitzbergen, even under 80° N. Lat., I never found the temperature of the open sea below + 0°.7 at the surface, and almost always it was higher at + 1°.0. We should, therefore, err, were we to draw inferences respecting the icy ocean from Baffin's Bay; and even though ulterior researches should prove that the glaciers of the latter place advance into the sea by sliding along the bottom, that would determine nothing against those of Spitzbergen, the shores of which are washed by an equatorial current of warm water, as we shall attempt to prove elsewhere.

Causes of the progression of the Spitzbergen Glaciers.—The glaciers of Switzerland descend to the plain by a progressive and continuous movement; this is a fact admitted by every one. This movement is very perceptible during the warm season, but it does not altogether cease in the winter. De Saussure* and Albert de Haller† have demonstrated this, and the destruction of the village of Randa, in the Haut-Valais, overwhelmed by a glacier from the Weisshorn, which fell from about 2000 metres, on the 27th September 1819, furnishes a lamentable proof of it.‡ But if authors are agreed as to the fact, they are far from being so with regard to the explanation of it. De Saussure,§ Escher de la Linth,‖ and André de Luc, attribute this progression to the weight of the ice and the sinking produced by the melting of the lower surface resting on the ground. Ever since the time of Gruner ¶ a third cause has been admitted, namely, the expansion of the water when passing into a solid state. Latterly this opinion has been more explicitly stated by T. de Charpentier,** and Prior

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* L. c. § 538.

† Quoted by De Luc; note on the Glaciers of the Alps (Biblioth. Univ. de Genève, May 1839, p. 142), and Jameson's Philosophical Journal, vol. xxviii. p. 15.

‡ Bericht von der Zerstoerung des Dorfes Randa von Venetz (Gilbert's Annalen der Physik, t. 64, p. 209. 1820. and Edinburgh Philosophical Journal, vol. iii. p. 274.)

§ L. c. § 535.

‖ Gilbert's Annalen der Physik, t. 69, p. 113. 1821.

¶ Description des Glaciers de la Suisse, p. 327. 1770.

** Gilbert's *Annalen der Physik*, vol. lxiii. p. 388.

Biselx,* discussed and admitted by Gilbert,† and lastly reproduced, in very recent times, by M. Agassiz.‡ The weight of the ice, the dilatation of the water freezing in its crevices, and their enlargement as a necessary consequence, appear to me to be the three causes which operate most powerfully in producing the progression of the Spitzbergen glaciers. In fact, the sinking of which Saussure speaks could not be regarded as one of the means of accounting for it, for these glaciers never melt on that surface which is in contact with the soil. The following are my proofs :—At Magdalena Bay there were, as I have already stated, two small glaciers which terminated on the top of a talus and did not descend to the sea. I have examined them closely; not the smallest streamlet of water escapes from their base. With regard to those which advance into the sea, it is impossible to assure one's self directly whether they give rise to streamlets at their lower part, since the latter would be lost in the sea below the ice. The rivulets which run laterally between the glacier and the mountain owe their existence to the melting of the snows. The experiments which we made on the temperature of the earth, appear to me equally to demonstrate that the glaciers of Spitzbergen do not melt under the influence of terrestrial heat. At Bell Sound, the earth was frozen very hard at least to the depth of a metre. We were obliged to put warm water into the hole in order to continue the bore. On the 3d and 4th of August 1838, a thermometer sunk $0^{m}.43$, and observed for thirty-three hours, afforded a mean of $+ 2^{\circ}.86$ C.; the maximum was $+ 8^{\circ}.4$; the minimum $+ 0^{\circ}.2$. At Magdalena Bay another thermometer sunk $0^{m}.35$, and, observed from hour to hour for ten days, gave a mean of $+ 1^{\circ}.55$; the maximum being $+ 2^{\circ}.40$; the minimum $+ 0^{\circ}.40$. The diurnal variations followed those of the atmospheric temperature. Thus, in Spitzbergen, the ground is frozen in the middle of

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* Ibid. t. 64, p. 183, 1820; and Biblioth. Univ. de Genève, t. xi. and xii. (*anc. sér.*)

† Annalen der Physik, t. 64. p. 183. See the notes.

‡ Bulletin de la Société de Geologie de France, t. ix. p. 443; and Biblioth. Univ. de Genève, May 1840, vol. xxvii. p. 134–5; and Jameson's Journal, vol. xxvii. p. 383.

summer to the depth of a metre, and the surface is warmed only by the direct rays of the sun. It is evident that this influence cannot act on ground covered with glaciers. The atmospheric heat cannot penetrate across masses of ice of such a thickness, and the ground on which they rest is necessarily frozen. It was our intention to assure ourselves directly of this; but circumstances, independent of our wishes, prevented.*

In his excellent work on the temperature of the earth,† Professor Bischof has fully demonstrated that an ordinary glacier cannot melt under the influence of central heat when the mean temperature of the earth which it covers is at zero. Now, he shews that,‡ among the Alps, it is at 2002 metres above the sea that this medium is found. This point being still 468 metres below the line which separates the lower and

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* M. Elie de Beaumont has inserted in the Journal of the *Institut*, of 15th June 1836, a physico-mathematical note on the *relation qui existe entre l'epaisseur que les glaces perpétuelles peuvent acquérir dans un lieu donné, et l'accroissement de température qu'on observe dans les lieux profonds.* If we apply to the glaciers of Spitzbergen the consequences which result from these formulæ, we will find that the face in contact with the ground ought to be in a state of melting, and that consequently the increase of the entire mass in thickness will be necessarily limited, if they acquire such a power that their base is comprised in a zone of terrestrial temperature above zero. In fact, the mean temperature of the air in Spitzbergen is very probably 7° C.; but temperatures above zero not being able to traverse either the ice or the snow which covers the ground during the whole year, we will take no account of them, in this general mean, from the monthly means of July and August which are above zero. The mean transmissible by the snow and ice is thus found lowered to nearly 9 metres C. If we admit, with M. E. de Beaumont, that the increase of temperature is 1° for 30 metres, in proportion as we descend vertically into the earth; if we suppose, moreover, that the conducting power of the ice for heat is equal to that of the earth, it must follow that a glacier must be 270 metres in thickness before it attain the *isogeotherme* of zero. If it pass this limit, it melts. Among the glaciers hitherto observed in Spitzbergen, the most dense, that of Horn Sound, is 121 metres of vertical height at the margin of the sea. It does not, therefore, reach the melting point, since its base is on a level with the isogeotherme of — 5° C. But if it were possible to visit the interior of the island, we would probably there find masses of ice whose height would approach to 250 metres.

† Die Waermelehre des Innern unsers Erdkoerpers, 1837; and Professor Bischof in Jameson's Philosophical Journal, vol. xxiii. p. 346.

‡ Ibid. chap. xviii.

upper glaciers, we may establish a new analogy between the Mers de Glace and those of Spitzbergen.*

The weight of the ice must necessarily draw the glaciers toward the lower parts, and it acts the more powerfully during summer, because nothing is then opposed to the progression of these lower parts. The freezing of the water in the crevices is still another force of great power, as we have seen above.

Without having recourse to any hypothesis, the following is my conception of the manner in which a glacier advances:— In summer immense transverse crevices divide its entire mass vertically into so many secondary wedge-shaped masses; consequently its surface is increased by the sum of all the spaces which the crevices leave between them at their upper part. The glacier resting firmly against the mountain cannot be pushed backward; it is, therefore, at its lower part, when nothing arrests it, that it becomes displaced, and moves forward. Winter following, these crevices are filled with snow blown into them by the wind, or falling in the form of avalanches. This snow becomes ice under the alternate influences of melting and freezing during the months of May, June, September, and October. In the succeeding summer new crevices are formed, the glacier advances, and so on successively. This progression, therefore, is neither a slipping nor a sinking (both of which it is difficult to admit, since the ice must adhere to the ground), but a successive dismemberment. A comparison will, perhaps, render my idea more intelligible: If we place a portfolio with compartments on its back, and rest one of its sides against some obstacle, such as a wall, we cannot open it but by advancing the opposite side, which is alone moveable. The obstacle is the amphitheatre of mountains which bounds and arrests the glacier on the land

* M. Bischof even tries to prove from reasoning, experiments, and facts, that the central heat of the earth has no share in the melting of glaciers in general, and that the streamlets which flow from them are formed by the springs which rise under the glaciers, and are increased by melting the ice in contact with them. This would explain in another manner why those of Spitzbergen do not produce streams, because no one has ever observed springs in that island.

side; the intervals which separate each of the compartments are the crevices; and the compartments represent the wedge-shaped masses of ice, of which a certain number are every year swallowed up by the sea. Is it necessary for me to add, that this comparison is not rigorously exact, but that its only object is to illustrate my idea? It is very evident, for example, that the wedge-shaped masses would, in the end, totally separate when the crevices are filled and covered with snow, which at a later period must be converted into solid ice.

Another analogy, and the last we shall allude to, exists between the glaciers of Spitzbergen and those of Switzerland. The latter, in consequence of their annual melting, send to the sea great rivers which maintain the uniformity of its level. The glaciers of Spitzbergen contribute to the same result by periodically throwing into it immense masses of floating ice, which lower the temperature of the northern seas, diminish their evaporation, and render rains rare and scanty in regions situate to the north of the polar circle. If in these countries, now covered with marshes which the sun is unable to dry up, notwithstanding his long presence above the horizon, rains were as frequent as in temperate zones, the line of perpetual snow would become still lower, the marshes would continually increase in extent, and these regions, already so little favoured by nature, would become altogether uninhabitable.

Additional Note.

Men of science, as well as men of the world, have long been desirous to ascertain whether the glaciers of Switzerland are continually advancing into the plains, or whether they are now in the same place they occupied many ages ago. In Norway, also, a few documents have been collected on this subject. As the progress of glaciers prevents cultivation in lands formerly subjected to it, it is more easy to find proofs of their advance than recollections of their decrease. Acts and contracts establish the existence of a house, a field, or a forest; but what interest could the inhabitants have in measuring the extent of a surface covered with blocks and sand, and doomed to eternal sterility?

In regard to Spitzbergen, the data are still more uncertain;

but the following are what I could collect on the subject. Our two pilots, who had often visited Spitzbergen, assured me that they had always seen the glaciers in the same place. We tried to obtain information of a more ancient date. The lieutenant of the Recherche, M. Delangle, had the goodness to lend me an old Dutch map, entitled *Nieuwe Afteckning van hat Eyland Spitzbergen opgegeven door de Commandeurs Giles, en Outger Rep, en in't ligt gelegt en uytgegeven Gerard Van Keulen Bock en Zeekaart, verkooper aan de Nieuwenbrug, met Privilegie, vor* 15 *Jaaren.*

This map has no date, but it cannot be earlier than 1707, the time when Commander Giles discovered the land which bears his name. The rudeness of the execution proves that it is not posterior to his time, unless by a very few years. It is ill laid down, but there is considerable accuracy in the design of the coast. Bell Sound is put down under the name of *Bel Sond of Klok Bay of Willem van Muyens Bay.* In the place now occupied by the great glacier of Bell Sound a narrow tongue of land appears, behind which is a creek with an anchorage, which bears the name of *Schoone Bay.* At the bottom of this bay there is a small river designated the *Sardammer River.* Nothing of this kind is now to be seen at Bell Sound, the whole of the part in question being occupied by the glacier. But we find other details which prove the accuracy of the Dutch hydrographers. The mountain on which we had established an observatory is called *Slaad-berg.* We recognise the small island of Eiders, and the river Sardam is perhaps the considerable rivulet which runs along the western side of the glacier, and which the latter may have diverted from its course. We cannot admit that the authors have neglected to mark the glaciers, for that of Pointe aux Renards is exactly indicated.

On the same chart the bottom of the Bay of Magdalena is semi-circular, as it would be if the glacier did not advance into the sea; and we perceive a small island to the north in a place which is now occupied by it.

It is therefore possible that the glaciers may have advanced since the period when the coast was surveyed by the Dutch. At the same time, I do not grant to these observations a confidence which they do not deserve ; but they have appeared to me sufficiently curious to be noticed. At all events, the increase of the Spitzbergen glaciers cannot be unlimited, for it will be always checked by the sea, which continually swallows them up in proportion as they advance into it by descending from the sides of the mountains.

Notice relative to Long's American Frame-Bridge. By David Stevenson, Civil Engineer, Edinburgh. With a Plate. Communicated by the Society of Arts for Scotland.*

The American bridges, as is generally known, are constructed almost entirely of timber, a circumstance which is easily accounted for by the abundance of wood and the scarcity of good materials for building bridges of stone, both in the United States and Canada. But even had good building materials been abundant, the rivers, lakes, and arms of the sea, spanned by the American bridges, are so extensive that the consumption of time and money which would necessarily have attended the building of stone-bridges, in such situations, would almost, in every case, it is to be feared, have proved too great to warrant their erection. Under these circumstances, as is natural to suppose, the erection of timber-bridges has occupied much of the attention of the American engineers; and the finest specimens of timber-bridge architecture that I have met with are to be found in that country.

To give a single instance of the extent to which bridge-building is carried in America, I may quote the dimensions of the Columbia Bridge over the River Susquehanna in Pennsylvania, which I visited in 1837. This magnificent work consists of no less than 29 arches of 200 feet span, and is supported on two abutments and 28 piers of rubble masonry, which are founded on rock at an average depth of 6 feet below the

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* Read before the Society of Arts for Scotland, 15th May 1839; when it was remarked by Mr Burn, the architect, that the principle on which the framing of this bridge is constructed might be applied with great advantage to many other purposes than bridges, where great lightness, combined with strength and rigidity, are required.—J. T. *Sec.*

surface of the water. The water-way of the bridge measures 5800 feet, and its whole length, including piers and abutments, is one mile and a quarter. The superstructure (consisting of the arches and roadway) is wholly of timber. The bridge is supported by three wooden arcs, forming a double roadway which is adapted to the passage both of road and railway carriages. There are also two footpaths, which make the whole breadth 30 feet.

In a country where so many bridges exist, it is not difficult to conceive that their construction must be very various, and it would be tedious to enumerate the different principles of construction which have been introduced and successfully practised in America. Some of these are very excellent and well adapted to many situations. So much is this the case, that having been consulted lately by H. F. Brown, Esq. as to the erection of a bridge of 150 feet span for his estate in India, I recommended the adoption of the bridge invented by Colonel Long of the United States Engineers' Corps as that which, under all circumstances, appeared to be most suitable, and a model was made by Messrs J. B. Maxton & Co. of the Leith Engine Works, from working drawings furnished by me, which was exhibited by Mr Brown's permission at a meeting of the Society of Arts.

The model was constructed on the same principle as that shewn in Plate VI. Fig. 1 is an elevation, Fig. 2 a plan, and Fig. 3 a cross section. The level of the road is indicated by letter *a*, *b* represents the "string pieces," as they are called in America; *c* the "posts"; *d* the "mainbraces"; and *e* the "counter-braces."

The string pieces are formed of three beams, in the manner shewn in the plan and cross section. The posts and mainbraces are in two pieces, and the counter-braces are formed of a single beam. Figs. 4, 5, 6, and 7 illustrate the manner in which the joining is formed, at the points where the posts and braces are attached to the string-pieces. Figs. 4 and 5 are enlarged diagrams, shewing the manner in which the posts are fixed to the strings. In Fig. 4 the strings are shewn in section at letter *b*, and the posts passing between them at *c*. In Fig. 5 the posts are shewn in section at *c*, and the strings at *b*. Fig. 6 shews the manner of fixing the main and counter braces to the upper string-piece. In this diagram *b* is the string, *c* the post, *d* the main-brace, *e* the counter-brace, and *g* is a wedge of hardwood, by which the whole woodwork is tightened up. Fig. 7 shews the fixture employed at the lower string. In this diagram *b* is the string, *c* the post, *d* the main-brace, *e* the counter-brace, *g* a wedge of hardwood, and *f* a block on which the counter-brace rests. The frames are connected at the top by cross beams, *x*, and at the bottom by the beams, marked letter *y*, which support the planking of the roadway.

The advantages attending this construction of bridge, which is called in America "Long's Frame-Bridge," are,

1*st*, A comparatively small quantity of materials, arranged as shewn in the plate, possesses a very great degree of strength and rigidity.

2*d*, The bridge exerts no lateral thrust tending to overthrow the piers or abutments, which may consequently be made in a much lighter and less expensive manner than the abutments or piers of arched bridges of the ordinary construction.

3*d*, The joinings of the different parts of the bridge are effected by the use of comparatively few bolts or spikes, a method which admits of its being very easily repaired when decay of the materials or other causes render it necessary.

White Pine *(Pinus Strobus)*, which grows in great abundance and perfection in the United States, is generally considered best suited for the construction of frame-bridges. This timber is preferred on account of its lightness and rigidity, and also because it is found to be less liable to warp or cast on exposure to the atmosphere, than most other timbers of that country.

The peculiarities in the construction of this description of bridge, and the manner in which the mortice and tenon joints and the seats for the different wedges are formed, can best be shewn by a reference to a model or working drawings on a large scale. The wedges are introduced for the purpose of tightening the truss-frames, and thereby rendering the whole structure stiff and rigid, and on the nice fitting of these wedges, and mortice, and tenon joints, the stability and rigidity of the structure in a great measure depends. In erecting a frame-bridge the several parts of the two truss-frames are, in the first place, properly fitted and put together in the work-yard; and after this has been done, and the beams and flooring timbers prepared, and the abutments built for the reception of the bridge, preparations are made for erecting it by forming a temporary bridge or support of pile-work in the bottom of the river or ravine which it is to span. This support is constructed in a substantial manner, and raised to the level of the lower part of the bridge. When this has been done, the truss-frames are taken asunder and erected piecemeal, and the lateral braces, flooring timbers, &c. applied in the manner shewn in the drawings.

The only peculiarity that remains to be noticed is the *driving home* of the keys or wedges, an operation which is done in the following manner before the scaffolding is removed:—Four workmen, two stationed on each side of the bridge, commence by driving the keys of the posts on each side of the centre post (taking care to drive the keys at the bottoms of the posts first, and afterwards those at the tops), and so advancing gradually with each post towards the abutments at each end of the bridge. The counter-brace keys are next *driven home*, and in doing this the workmen commence at the abutments and drive regularly along to the centre of the bridge, driving them as hard as they may be driven without splitting or injuring the wood, with a smooth-headed hammer weighing four or five pounds. This operation having been completed, the flooring is put on, and the scaffolding removed, when the bridge is ready for the passage of goods and passengers.

In conclusion, I may remark that bridges constructed on this principle, varying from 100 to 150 feet in span, are very commonly met with in the United States; and on the American railways I have crossed bridges of this construction, varying from 50 to 100 feet in span, in a heavily loaded train drawn by a locomotive engine, weighing from 12 to 15 tons, without observing the smallest appearance of fracture, or even of yielding in the woodwork of the bridge.

EDINBURGH, *April* 1839.

*On the Tea-Plantations in India.**

We cannot conclude our notice of the January number of the Edinburgh New Philosophical Journal, without regretting to see so much of its valuable pages occupied with a report on the manufacture of tea, and the extent and produce of tea-plantations in Assam, by C. A. Bruce, Superintendant of Tea Culture, because we think the report is calculated to mislead the public and occasion disappointment, instead of being likely to clear up any of those difficulties that are as yet to be overcome before the Assam Tea Company can expect to reap any return for the outlay of capital.

Mr Bruce states that he submits his report with diffidence, having had something more than tea to occupy his mind; nevertheless his knowledge of tea localities is much extended since he last wrote, embracing *no less than* 120 *different tracts*, some of them very extensive, both on the hills and in the plains. Mr Bruce does not state that this number includes patches of wild tea-plants found by Mr Griffith at Cujoodoo, Hookum, and other places, and those found by Captain Hannay at Jeypore, and we believe by Colonel White, Mr Bigge, and Captain Jenkins at Namroop, Jeypore, Boorthath, &c., or the nurseries cultivated at Suddyah by Captain Charlton as early as 1824. We shall merely endeavour to examine what information the report before us conveys regarding the existence of 120 tea-tracts alluded to above.

Mr Bruce, in crossing a hill 300 feet high at Jeypore, found a tea-tract which must be three miles in length, as he could not see the end of it; and at the foot of this hill he saw another tract, which he had not time to explore. He next found tea on Cheriedoo, a small hill close to the Dacca river, and again after crossing the river, at a place called Hauthoweah, near the old fort of Ghergong. Neither of these four places Mr Bruce had time to examine, with a view to the collection

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* From No. 2 of an interesting Journal entitled The Calcutta Journal of Natural History. Conducted by John M'Clelland, Esq. Bengal Medical Service.

of any further personal information than that which we have above stated. Again, Mr Bruce found tea to the south-west of Gabrew; and thus the 120 localities are reduced to five, in which he has himself seen the tea-plant growing, even supposing his experience to be such as to render his mistaking some other plant for tea unlikely, which is by no means certain, particularly as he mentions having found on the west of the Dhunseree a different species from what we use, but still tea. With this amount of new information Mr Bruce proves by argument, as well as the reports of natives "well acquainted with the leaf, having been in the habit of drinking tea," that large tracts of the Naga mountains are covered with tea-plants. On information not one whit more satisfactory than that on which Mr Bruce clothed large tracts of the Naga mountains with tea-plants, has he covered a large portion of Upper Assam with them, though we have no doubt it will be found, after all, that it is confined to a few limited patches here and there, in various parts of the forests, and by no means universally diffused and abundant, as Mr Bruce's report would lead the public to imagine.

As a specimen of Mr Bruce's way of shewing the extent of the wild tea-plants, we may quote the following:—"In giving a statement of the number of tea-tracts, when I say that Tingri, or any other tract, is so long and so broad, it must be understood that space to that extent only has been cleared, being found to contain all the plants which grew thickly together; as it was not thought worth while, at the commencement of these experiments, to go to the expense of clearing any more of the forest for the sake of a few straggling plants. If these straggling plants were followed up, they would, *in all probability*, be found becoming more numerous, until you found yourself in another tract as thick and as numerous as the one you left; and if the straggling plants of this new tract were traced, they would by degrees disappear until not one was seen; but if you only proceeded on through the jungle, *it is ten to one* that you would come upon a solitary tea-plant, a little farther on you would meet with another, until you gradually found yourself in another new tract, as full of plants as the one you had left, *growing* absolutely so thick as to *impede each other's* *growth.* Thus I am convinced that one might go on from one tract to another."

Most people in perusing this would suppose that Assam was covered with tea-plants, and that so far from Mr Bruce exaggerating in saying you might go on for miles, the reader would imagine that you might travel from one end of Assam to the other through a succession of tea-tracts. For a tract the reader must understand a patch; several patches often occur, too, in the same vicinity, and it is between these that straggling plants are found. Mr Bruce, however, calls each of these patches, tracts; and the common jungle, patches. Thus he says,—"All my tea-tracts about Tingri and Kahung are formed in this manner, with only a patch of jungle between them, which is not greater than what could be conveniently filled up by thinning those that have too many plants. At Kahung I have lately knocked three tracts into one, and I shall probably have to continue doing the same until one tract shall be made by what now consists of a dozen." Mr Bruce's substitution of the term tract for what is in reality a mere spot, is most unfortunate; and yet it does not appear to have been accidental, as he observes,—"I have never yet seen the end of Juggudoo's tea-tract, nor yet Kujudoo's or Ningrew's." Now, two at least of these localities were visited by the Assam deputation, and their extent measured and found to be very limited, and not larger than a common cottage-garden. There may be other two or three similar patches in the vicinity, but it appears to us too great a stretch of the imagination to say, that the plants of these isolated little patches "run over the hills and join, or nearly join," similar little spots in distant parts of the country; and to infer from this supposition, that the whole country is covered with tea-plants, or tea-forests, as they have been very improperly styled. It is easy to imagine how Mr Bruce makes up the number of tea-districts in Assam to 120, when every patch of jungle in which a few plants occur is considered by him a tract, however closely it may be connected with several other similar little clumps of plants in the same vicinity.

Any one rising from the perusal of Mr Bruce's report would suppose that Assam is covered with tea-plants, requiring no other cultivation than the mere destruction of the surrounding forests. Mr Bruce thinks fire is as beneficial to the tea-plant as it is destructive to all others ; and that the only cultivation or care that plant requires is merely to burn it down to the roots, by setting fire to the forest in which it is so common. In the first or second year after this, Mr Bruce is of opinion that we shall have nothing more to do than commence the manufacture of tea from an unlimited stock of plants extended over 120 tracts, which those who peruse Mr Bruce's report may consider equivalent in extent to as many districts or even counties.

Instead of finding Assam one extensive tea-garden, however, we suspect that the Tea Company will find that before they can manufacture, they must begin to plant ; and that circumspection and skill will be required in the selection of the most suitable lands. We have so poor an opinion of the extent of the wild plant, that we think it would hardly do more than afford sufficient seed for new plantations. So far, therefore, from all things being ready in Assam for the extensive manufacture of tea for commercial purposes, as the public are led to imagine from the report of Mr Bruce, we think that every thing is yet to be effected, and that some time and money have been spent in vain, and the public exposed to encounter some degree of disappointment, in consequence of Mr Bruce's report being allowed to go abroad without a few remarks from the Tea Committee to qualify what appears to us the extravagant views contained in it regarding the extent of the tea localities. With the Assam tea, as with other objects of popular interest, nothing is received with favour that does not flatter our expectations, however unreasonable and absurd these may be in reality. We always find in the long-run, however, that we have to pay pretty dearly for our indulgence ; for while few have the moral courage to express an unpopular opinion, thousands live and flourish for a time by the dissemination of popular error, until something happens to give the question another turn. With regard to the subject before us, all we will venture to recommend is, that such flattering reports as the one we have noticed be not allowed to impress us with the idea, that the present stock of wild tea-plants in Assam is of such extent as to afford anything like a return to the Assam Company. From what we have ourselves seen of the tea-plant in the Sing-Pho jungles, in the Muttack, and in Raja Parunder Sing's territory—the only three tracts in which it occurs—the whole root and branch, if converted into tea, would not make a single consignment such as would annually be expected from the Assam Tea Company; and after a careful examination of Mr Bruce's report, as it appears in the Edinburgh Philosophical Journal, we regret to find that, in our opinion, the 120 tracts with which Mr Bruce has covered the map of Upper Assam, are for the most part either imaginary, or altogether dependent on native report. Mr Bruce's adoption of the term tract for each little patch of jungle in which a few tea-plants are found assembled, is, as we have already stated, enough to lead to misconception. It is not, however, more objectionable than the term tea-forests, we believe applied in the same way by Dr Wallich. In our own report we employed the terms colony and locality; the latter term we believe was adopted by Mr Griffith, who also used the term patch in preference to colony, which was objectionable, inasmuch as it implied that the plants were introduced rather than indigenous. We think, therefore, that Mr Bruce should, according to that respect usually paid to priority in such cases, if not to avoid the appearance of exaggeration, have employed some one of the above terms in preference to *tract*, which it might be proper to confine to an assemblage of tea-patches, as the Muttack *tract*, Tingri *locality*, Sing-Pho *tract*, Ningrew locality or plantation, according as the plants may be of the wild or cultivated stock. The remainder of Mr Bruce's report is chiefly made up of details regarding the manufacture of tea; but as these are derived entirely from the Chinamen employed, for whose word Mr Bruce, as well as the public, can have no security, this part of the report is to be received with some limitation. The quality of the tea produced will be the best criterion of the merit of the process or manipulation employed. The proverbial neatness and delicacy of Chinese execution we should have thought at variance with the following part of the process of making *souchong*, as given by Mr Bruce:

"The man then stands up, holding on by a post or some such thing, and works the ball of leaves under his feet, at the same time alternately pressing with all his weight, first with one foot, then with the other."... "The tea is taken hot from the pan and packed firmly in boxes, both hands and feet being used to press it down," &c. As tea-drinkers are not the least fastidious portion of the community, we should recommend Mr Bruce to endeavour to introduce a substitute for the feet in these operations.

We think it would be a hardship to adopt Mr Bruce's advice with regard to the prohibition of opium in Assam. The Assamese have few luxuries, and to deprive them of such as they have, would be doing them a very questionable kind of service. We doubt if levying high duties on opium-land would have the effect of preventing the cultivation of the drug: if the Assamese are fond of opium they will have it, and public measures for putting down its cultivation and preventing its introduction, would be rather difficult to enforce in an isolated province, surrounded as Assam is on all sides by countries in which opium might be cultivated without restriction. Mr Bruce, however, states, that a native of Assam will steal, sell his property, and even his children for opium; and as Mr Bruce himself dealt in the drug up to the period of his employment as superintendent of tea cultivation, he had doubtless the very best opportunities of witnessing the immoral effects he describes. Let us hope, however, that the cultivation of tea will in that province be carried on to an extent that will supersede opium both as a source of profit and luxury. It will be our duty to watch the progress of this new staple; and while we shall hail with satisfaction every successful step towards its introduction, we shall freely point out whatever appears to us likely to retard or endanger the final success of the scheme.

Note.—We find the report published in the Edinburgh Philosophical Journal to be the same with that which appeared in the Journal of the Asiatic Society. How it could have passed through the Tea Committee without eliciting some observations from Dr Wallich we cannot imagine.

On Spring and River Water, as connected with Health and the Arts.

Many of our readers will doubtless remember that the lovely city of Lyons,—the ancient *Lugdunum*,—when under the sway of imperial Rome, possessed a noble aqueduct, which, from the spring-head of Mont Pilate, furnished a superabundant supply of the first necessary of life to its inhabitants—a supply which, when reduced to a modern standard, amounts to not less than five millions of imperial gallons a-day. That invaluable boon has now, for many ages, been suspended; the colossal work has long lain a ruin, and the citizens have been forced to resort to the waters of the Rhone and the neighbouring springs. This exchange has very generally been believed to be attended with numerous disadvantages and evils; and this circumstance has fortunately induced Professor Dupasquier, one of the physicians of the *Hotel Dieu*, and a member of the Council of Health in the department of the Rhone, to undertake an extensive and laborious investigation of the whole subject, bringing to its elucidation all the appliances which modern science can suggest. The results of this investigation he has published in a large and learned treatise which lies before us,* and in which he has arrived at the conclusion, that it would be greatly for the benefit of the Lyonnais again to resort to an aqueduct—which has been proposed—and afresh to obtain from springs, on the banks of the Saone, supplies which, he believes, would be scarcely less productive than the ancient ones. Much of the interest connected with this important investigation is necessarily special and local; but, on the contrary, many of the results which have been obtained are new and weighty, and, as connected with Hygiène and the arts, cannot fail to be interesting to the physician and chemist, to those connected with the arts, and the general reader; and hence we shall present a short analysis of the work.

As remarked by the author, the vast progress which has recently been made in the art of chemical analysis has greatly

* Des eaux de Source et des eaux de Rivière, comparées sous le double rapport Hygiènique et Industriel, &c. 8vo, pp. 414. Paris et Lyon, 1840.

augmented our knowledge of the composition of water. It has, however, been almost exclusively *mineral waters* which have attracted the attention of chemists; and the careful study of the more ordinary kinds of water, whether for drinking or employment in the arts, has been nearly wholly neglected. Hence, in the accomplishment of the task he had undertaken, M. Dupasquier soon found that the most valued opinions concerning the properties both of river and spring water were altogether vague and unsatisfactory; that so far as Hygiène was concerned, no progress had been made since the days of Hippocrates; and that as it regards their employment in the arts, and especially in dyeing, not only has the subject been studied in the most superficial manner, but many gross errors are still prevalent, not only among the common workmen, but among those scientific individuals who direct and assist them.

In endeavouring to supply the many deficiencies which exist on the subject, the author undertook long and laborious researches, which present original views; and he directs attention to the following points: 1*st*, To the influence, upon the animal economy, of the temperature of the water which is drunk; 2*d*, To the utility, in water, of certain substances which are foreign to its atomic constitution; 3*d*, To the difference which exists between the various salts of lime found in potable waters, as it respects their wholesomeness, and the arts; 4*th*, To the comparative action of the different calcareous salts upon soap; and, 5*th*, To the part assigned by nature to the carbonate of lime in the process of digestion, and its influence upon the colouring principles of dye-stuffs.

We must here omit all that the author has said respecting the historical portion of the subject, and also his analysis of the various springs of the different wells and fountains of the city of Lyons; but shall introduce a few of his statements concerning the waters of the Rhone. This river, taking its rise from one of the largest glaciers in Switzerland, previous to reaching the Lake of Geneva, receives a large quantity of water, which is produced by the melting of the snow and ice which cover the summits of those Alps through whose valleys it wends its way. It is reckoned there are about forty-two important glaciers which supply their contributions to this river, including in this enumeration those which pour their waters into the Arve, a tributary joining the river a little below the Lake of Geneva, and which receives the waters of the glaciers of the valley of Chamouni. The waters of the Rhone are considerably augmented during the heats of summer, by the melting of the snow ; and during this season, the waters of the Lake of Geneva rise, notwithstanding its vast extent, to between six and eight feet, as the maximum. This circumstance distinguishes the rivers of high mountains from those which take their rise in less elevated regions ; whose increase occurs in the rainy seasons, and their greatest subsidence at the period of the summer's heat.

The Rhone, at its egress from the Lake of Geneva, where it deposits all the sand and mud with which it is loaded during its impetuous course in *Le Valais*, is perfectly pure and transparent. Its colour,—a fine blue, somewhat tinged with green,—as is well known, has often engaged the attention of philosophers, and several explanations of the phenomenon have been attempted. This lovely hue it soon loses. Immediately after passing the town of Geneva, it receives the waters of the Arve, the torrent which, as we have already hinted, comes from Mont Blanc, and which, in its rapid course, passing over a bed of rather decomposible schist, is soon loaded with fine mud, which, in summer especially, darkens its waters, and confers on the Rhone a very deep greyish tint. Somewhat further down, the Rhone receives also the Ain, which, during its floods, brings down the marls and clays of *La Bresse*, and contributes not a little to alter its original limpid purity. According to the appearance and the quantity of these different tributaries, the Rhone, in passing Lyons, varies in its physical qualities. If the Arve predominates, it holds suspended a greyish sediment ; on the contrary, when the Ain, swollen by the rains, manifestly augments the size of the river, its waters contain a calcareous argillaceous earth, which communicates to it a yellowish tint.

In winter, the water of the Rhone is clear and almost limpid ; in spring, as soon as the melting of the Alpine snows commences, it increases in size and becomes more muddy every day till well on in autumn, from which time it again decreases, and gradually becomes clearer. But its chemical constitution, as we shall by-and-by remark, supplies a result which contradicts those conclusions which would be naturally drawn from its physical condition. In winter, the water of the Rhone, when at its minimum as to quantity, contains, all limpid and blue as it is, more of the salts and gases in solution than in the greatest heats of summer, at a time when it is grey and muddy to the eye, but really at its maximum in chemical purity.

The temperature of the Rhone water rises in summer to 77° Fahr., and more. It has then an insipid and disagreeable taste, which does not leave it till it falls to 64°, or even to 61°. In winter, the temperature is sometimes as low as 34°, and in the year 1837, when the temperature at Lyons ranged from 17° to 23° Fahr., the Rhone water descended to 33°, so that it was within 1° of the freezing point.

Comparing its composition in winter and summer, the following results were obtained.

	In July 1835, there was in somewhat more than 3 gallons (15 litres),	In February 1838,
Gases.	*Centilitres.*	*Centilitres.*
Carbonic acid,	9.8	27.3
Oxygen,	9.8	18.6
Nitrogen,	17.3	10.
Solids.	*Grammes.*	*Grammes.*
Carbonate of lime,	1.51	2.260
Sulphate of lime,	0.10	0.293
Chloride of sodium,	a trace	0.101
Chloride of calcium,	do.	0.101
Sulphate of soda,	do.	0.103
Sulphate of magnesia,	do.	0.103
Organic matters,	do.	a trace.
Chloride of magnesium,	0.00	do.

In this table it will be observed that the water of the Rhone, in approximating in winter to the good qualities of spring-water in its physical properties, also approximates to its chemical combination by a considerable augmentation of its gases and soluble salts.

After these minute researches concerning the spring and river water which supply the wells and fountains of the town of Lyons, M. Dupasquier proposes to resolve the question, Whether the water of springs is more valuable than that of rivers? And on this point he regards it impossible to come to any general conclusion, as every thing depends upon the peculiar chemical and physical nature of each separate example. Thus, the water of *La Bièvre* and of *La Beuvrone*, at Paris, contain respectively eighteen and twenty-nine times more sulphate of lime and of chlorate of calcium and magnesium (those salts which render water *hard*—in other words, improper for the dissolving of soap and the cooking of vegetables), than the water of the springs which rise in the neighbourhood of the Saone, and which, it is proposed, should supply the wells of the city of Lyons. The water, then, of the specified rivers must be much inferior in value to that of the fountains. On the other hand, the waters of other springs—and the author cites many examples taken in the neighbourhood of Lyons—contain almost a fourfold quantity of these earthy salts, and are, in this respect, as bad as the worst water of wells. He concludes, therefore, that you may have good and bad spring-water, as well as good and bad river-water, and that the question of the comparative excellence can be decided only *a posteriori*, and this, too, after you have first established the character of good potable water.

The qualities which are universally esteemed in drinking-water are perfect freedom from all disagreeable taste or flavour, and clearness, and coolness. The *Dictionnaire des Sciences Medicales* has the following definition: "Water may be considered good and potable when it is cold, clear, without odour, when its flavour is neither insipid nor disagreeable, nor sharp, nor salt, nor sweetish; when it contains little foreign matter, when it contains air in solution, and when it readily cooks dry vegetables."

The *odour* which water contracts, in those cases in which it is not owing to sulphur or some other active mineral substance, arises from the organic matters, more or less in a state of decomposition, with which it has been in contact; sometimes also from turf, or even, as is seen in many localities, from a certain quantity of carburetted hydrogen gas, gradually disengaged from certain clays. If this odour is persistent, the water should not be considered potable, and it should be rejected from all domestic uses; often, however, its presence is only accidental, and owing to some casual admixture or infiltration. The use of quicklime, thrown in small quantities into the wells, is often found to remove this disagreeable adjunct.

When the *flavour* is acid and sharp, and owing to carbonic acid, the water need not be rejected as impotable, for it is in no way injurious to the health. Every other flavour, however, indicates the presence of something noxious; and it should not be forgotten, that the total absence of all flavour is no certain guarantee of the excellence of water.

Every kind of strange *colour* should exclude all such water from being employed for domestic purposes, except in cases of absolute necessity, and, in these instances, it should be filtered through charcoal. Thick and muddy water, however, often becomes of excellent quality after it has remained in a state of repose, or has been filtered through a bed of sand or gravel.

As to *temperature*, Hippocrates says that the best water is that " which is warm in winter and cold in summer." In fact, nothing is more disagreeable and more hurtful during hot weather, than the use of water whose temperature approaches nearly to that of the atmosphere. It appears that, however excellent it may otherwise be, it is disagreeable to the taste and sickening; and a draught of it is not followed by that sensation of refreshment and comfort, nor with that renewal of strength and activity, which follows drinking cold water. This enervating influence of warm water enfeebles the stomach; and a great number of the diseases which are common during the summer may be attributed to it. Fresh and cold water may be considered, therefore, during the hot season, as one of the necessaries of health for the population of temperate climates, who do not, as in India, use active stimuli for the support of the digestive powers; and, in this respect, spring-water, as maintaining a more equal temperature, is preferable to river-water, which is much more readily affected by atmospheric influence.

As to *chemical composition*, water is *light* when it contains a suitable quantity of atmospheric air and carbonic acid; it is *soft* when it does not hold in solution a quantity of calcareous salts sufficient to curdle soap in water, nor to harden the vegetables which are boiled in it. In this latter alternative, it is *hard*, and is usually regarded as unwholesome and indigestible.

It must not, however, be supposed that water, the more it is chemically pure, that is to say, freed from all foreign admixture, is therefore proportionably salubrious and agreeable to the taste. This is far from being the case. Distilled water, which contains neither salts nor gas, is both insipid and disagreeable, and feels heavy upon the stomach. In water, therefore, there are certain foreign bodies which are beneficial, and which improve its virtues as a refreshing beverage ; and there are others, on the contrary, which are hurtful ; and hence the importance of our being able to discriminate between the two classes. To the former class belong atmospheric air, carbonic acid, the chloride of sodium, and, according to M. Dupasquier, a small proportion of the carbonate of lime ; to the latter, the sulphate of lime, and the nitrate, also the chloride of calcium, so soon as it is in considerable quantity, and all organic matter, especially when in a state of decomposition.

It is scarcely necessary to insist upon the utility of air, or at least of oxygen, in water, and of carbonic acid : the change which boiling or distillation produces in the agreeable and wholesome qualities of this fluid is known to every one. Not so, however, with regard to the presence of carbonate of lime, which has hitherto been supposed to be hurtful ; this opinion the author conceives is a popular error, and on this point his views are new. Here it is scarcely necessary to remark, that the author excludes from this favourable judgment all those waters which, like those of *Saint Altyre* near *Clermont*, contain as much as 15 grains of the carbonate of lime and magnesia in 1.76 imperial pints *(deux grammes par litre)*. The proportion in which he esteems them useful is from 3 to 4 grains *(de* 0,20 *à* 0,25 *grammes)*, in the same quantity of water. He reposes this conclusion upon the well-known properties of the bicarbonatis, the alone state in which the great insolubility of the carbonate of lime will allow him to view it in any water which promotes digestion ; and he believes that the bicarbonate of lime will excite the digestive functions, as does the bicarbonate of soda, in water, and other vehicles.

Respecting hurtful substances, the most common is gypsum, or sulphate of lime, which curdles a solution of soap, hardens vegetables, and renders digestion painful and difficult. The chloride of calcium and the nitrate of lime have the same hurtful properties.

Organic substances are so difficult to demonstrate by the use of chemical tests, that we must not always conclude from the absence of appreciable characters diluted by this agency, that the water is pure. On this point, the author mentions a fact which was observed at Lyons, in which the use of a well whose waters, on analysis, shewed nothing remarkable, induced an endemic disorder in the garrison, and which ceased, as soon as they were discontinued. Experiments, therefore, in such cases, must be tried; and if, as a general rule, the water of ponds and marshes should be at once rejected, still more anxiously should we abstain from those whose use appears to injure the health of the inhabitants who live in the neighbourhood.

As it regards the arts, that water is usually considered unobjectionable which is esteemed a good drinking water; but, moreover, certain considerations should be kept in view, depending upon the particular art in which it is employed.

In dyeing, for example, clearness is an essential quality to the purity of colours, and especially of the more delicate hues. It has been remarked at Lyons, that those white silks which are prepared with the river-water are inferior, as to brightness and purity, to those which have been bleached with spring-water. It is also regarded as indispensable that the water intended for bleaching, should not contain too great a quantity of calcareous salts; as then they will cause the soap to curdle, and thus not only will occasion waste, but these flakes, by attaching themselves to the silk, wool, or cotton, will hinder the bleaching of the parts which are thus enveloped, and render the influence of the soap incomplete.

One remark made by the author is alike curious and interesting: it is, that the carbonate of lime held in solution in water, in those proportions in which it is found in good drinking waters, has not, like other calcareous salts, the property of precipitating the solution of soap. M. Dupasquier attributes this effect to the excess of carbonic acid, which hinders the reaction of the calcareous carbonate upon the stearate and oleate of soda, which form soap. He has proved this interesting fact by numerous experiments, which he describes in detail, and from which it results that there must be in water a proportion of this salt almost six times greater than that which exists in common spring water, before a sensible effect will be detected in the solution of soap. Another important remark is this, that for dyeing it is serviceable to employ water whose inherent temperature remains throughout the winter as far as possible from the freezing point. In this state it is more active in its dissolving powers, both in washing and bleaching, and also in removing the soluble colours which are intended to be retained by means of a mordant. The influence of the calcareous salts contained in water upon colours is remarkable. They heighten their intensity without altering their freshness and tint. Hence the dyers at Lyons prefer spring-waters, and ascribe to their virtues a considerable diminution of the required dyes. It is striking that they have no such influence upon the different shades of black, and that the softest waters are accordingly preferred for this hue.

In the following chapters, the author compares the advantages and disadvantages accruing to Lyons from the employment of the water of the Rhone, and that of the springs whose properties he has so carefully examined. He does not hesitate to give the preference, as it regards clearness, to the spring-water, for it has it in perfection; whilst the water of the Rhone, after ten days' repose, has not deposited all its sedimentary matter; and none of the known systems of infiltration can effect, without enormous expense, a sufficient purification of the 1,500,000 or 2,000,000 gallons of water, which the city requires. The author has even remarked that the Rhone water, filtered in an apparatus at the Great School, contains more earthy salts than the water taken from the stream, and filtered through paper. Hence, it is probable that in the pressure employed in the operation, the earthy matters which are deposited become more soluble. The foreign matter which the Rhone contains at Lyons is very considerable in amount. Experiments made in the year 1839 gave the following results.

	Quantity of dry residuum, per 1.76 lb. (per litre).
First Experiment—The flood of the river being advancing,	12 grains.
Second Experiment—The flood of the river being at its maximum,	18 grains.
Third Experiment—The flood past and the river diminishing,	7 grains.

Thus, at the maximum of the flood, the quantity of mud to be separated from the 1,500,000 gallons of water regarded necessary every twenty-four hours, would be about 22,000 pounds (8800 *kilogrammes*); and it is not easy to comprehend how the filters could be cleared out. If estimated by volume, it would amount to about 1250 cubic feet of solid matter *per diem*.

The process of natural infiltration across the soil on the margin of the river, and of the great drain-wells whence the water is procured, which appears at first sight as the most convenient, has the disadvantage of decidedly diminishing the purity of the water. Thus, the author has found that the wells which are at the distance of from fifteen to twenty yards from the bed of the Rhone, supply water which contained calcareous sulphates and chlorides, and decomposed the solution of soap.

A preference is also due to spring-water on the score of *temperature*. Such water may run for a great distance over the surface, without losing much of that equality of temperature which makes it cool in summer and warm in winter. The author quotes, in reference to this curious subject, the *Lavosne* spring at *Neuville*, which takes two hours and forty-four minutes to reach the Saone, and whose waters, nevertheless, never freeze, even under the most intense cold; and, on the other hand, are never heated more than five or six degrees Fahr. above their original temperature.

The water of the Rhone, which at Lyons varies very much as the atmosphere does, never seems decidedly to cool down in its passage through the pipes of the conduit, by which it reaches the wells and fountains. Thus in 1839, from the

17th to the 25th of June, the mean temperature of the water of the Rhone had been, in the stream, 74° Fahr.; in the well of the Botanic Garden, a covered reservoir, where it continues for a time, it was as high as 77°; and in the fountains which this reservoir feeds, it was found, according to the distances, at 72°, 70°, and 68°, the distance in the last case being about 1600 yards. Hence, it can scarcely be expected that, by any subterranean transit through pipes or otherwise, the temperature of water will be materially improved.

From the marked superiority which the spring-water on the margin of the Saone possesses over the water of the Rhone, in the important points both of clearness and temperature, and the trifling importance he assigns to the presence in these springs of a small quantity of the carbonate of lime, which, in fact, is advantageous, M. Dupasquier gives it as his mature and decided opinion that the former should be employed for the supply of Lyons. This leads to a discussion on the probability of the obstruction of the conduits by ferruginous depositions or calcareous incrustations, and he concludes that all this might be prevented by constructing great earthen tanks, through which the water would needs pass before it entered the pipes; or that, by effectually excluding the atmospheric air, the water might maintain its primitive composition and have little or no tendency to deposit foreign matter.

Desirous of elucidating, as far as possible, the comparative value of spring and river water, in so far as the question involves the public health, M. Dupasquier thought of more particularly examining the organic matter which they contained. In all the specimens submitted to examination, chemical investigation exhibited traces of an animo-vegetable substance, but without yielding any more satisfactory result. This suggested the propriety of having recourse to the microscope; and for the avoiding of the many illusions inseparable from such investigations, the author obtained the assistance of Dr Donné, already favourably known for his microscopic labours. The following are the principal results he obtained.

1*st*, In winter 1839, at a temperature below the freezing point, the Rhone being at its minimum of volume and in its state of greatest clearness, water taken from the current exhibited only a few rare infusoria and a small quantity of matter of vegetable origin. The substances belonging to both kingdoms augmented rapidly, however, with the thaw, and this when the specimens were taken from the current, as well as from the wells fed by natural infiltration. 2*d*, At this epoch, spring water contained much less of the organic substances than the water of the Rhone, and less, too, the nearer it was procured to the spring-head itself. 3*d*, The same results were obtained in the season of spring, with the temperature varying from 54° to 59° Fahr. 4*th*, In summer, in the month of June, with the temperature ranging from 77° to 86°, the waters of the Rhone and those procured from springs contain, nearly to the same extent, an immense number of infusoria and microscopic vegetables.

Hence it follows, 1*st*, That the Rhone is at its maximum of purity, in respect to the organic matter it contains, when the air has been for some time at a temperature below zero. 2*d*, That the water of the several springs is in its best condition, so far as it relates to this matter, in the springs where they first see the light, and that, in all seasons, they are as free of animo-vegetable matter at their origin, as is the water of the Rhone in the depth of winter. And 3*d*, That on this point, there is an evident superiority of spring-water over river-water, a circumstance which is very readily explained by the innumerable causes of contamination which occur in many localities through which streams flow.

In respect to *chemical composition*, M. Dupasquier maintains that there is an advantage, both for the preservation of health and the successful prosecution of various arts, in water not undergoing great and sudden changes. He conceives that the health of many may be injured, and the success of various delicate operations in the arts prevented, by a change in the chemical nature of potable water. Spring-water, therefore, which at all seasons has presented a great uniformity of composition, is, in these respects, preferable to the water of the Rhone, whose chemical composition varies rapidly, according as its stream is principally supplied by its different feeders, taking their origin in such different localities, and more especially as manifested in the changes of winter and summer, as has been above explained. On all these grounds, whilst the author admits that the Rhone water is, at Lyons, a very potable water,—especially when it has undergone the process of purification,—yet he must assign a decided preference, for all domestic purposes, to the springs on the margin of the Saone.

The important subject of the application of water to various arts, and more especially to the dyeing of silks, the chief of the manufactures at Lyons, leads to a number of not less curious remarks. M. Dupasquier publishes a letter, signed by seventy-five of the dyers of Lyons, who have all remarked that there is a marked superiority in good spring-water over the Rhone water in the dyeing of silk. The calico-printers, also, whose establishments are in the vicinity of copious streams, have stated that their dye-stuffs were not only more beautiful, but went much farther than they did on the banks of the Rhone, where they had formerly been situated. They also remark that, in their new circumstances, bleaching is improved, and that the steadiness of temperature is much more favourable to dyeing and the printing of goods, than the variety of execution which attended upon the changes of the river-water when frozen in winter and parboiled in summer. This last advantage is so great, that some of the calico-printers on the banks of the Rhone carry, in summer, the valuable goods they have to dye to a distance of about two leagues, that the operation may be carried on with spring-water, whose temperature and clearness are not liable to change. We may add, that the presence of calcareous salts in the water employed for the dyeing of silk is so indispensable,—and especially for the preservation of various shades of white, that the eminent dyers of Saint Etienne, who, on their premises, can command only the very pure waters of Furens, send to Lyons, a distance of fifteen leagues, to the establishments close to the calcareous springs, all the silks which are intended for white fabrics; and for this kind of work these establishments have a kind of monopoly.

Wholly independent of these facts founded upon the practice and observation of the manufacturers, M. Dupasquier has made, both personally and in the presence of many practical and expert dyers, comparative experiments upon the colouring and dyeing properties of decoctions of various dye-stuffs, in equal quantities of spring-water, Rhone-water, and distilled water; and has invariably found that the result of the boiling during the same length of time, and in the same proportion of the dye-stuff, was of a deeper and more beautiful hue in the spring-water than in the Rhone-water, and in this latter than in distilled water. The quantity of the colour extracted seemed to be in inverse ratio to the purity of the water used.

It was only natural to infer that this interesting and remarkable event was owing mainly to the presence of the calcareous salts of the different specimens of water subjected to trial, and it was not a matter of much difficulty to determine by direct experiment to which of the salts contained in these waters the effect was principally to be attributed. Accordingly, M. Dupasquier undertook a careful investigation of the subject. He prepared solutions in distilled water of each of the salts contained either in the waters of the springs, or of the Rhone, and after having boiled each of the dyes previously examined in distilled water, he observed the effect which the solution of each of the salts produced upon these decoctions. After all these experiments, of which the author furnishes a detailed table, it most decidedly appears that the carbonate of lime is the only substance which increases the intensity of the colour of dyes; that the sulphate of lime, the chloride of calcium, and the sulphate of magnesia, are either inoperative, or only more or less change the peculiar colouring properties of the dye; and, finally, that carbonic acid, when it acts—and it does so only on a few colours—has only a trifling agency, slightly lightening the shade.

But not only does the carbonate of lime act upon the colour by brightening its hue, but it also appears to facilitate its solution in water, for this solution is, upon its addition, effected more quickly and in greater abundance. This result, we need scarcely add, perfectly harmonizes with the practical experience of dyers, who reckon that to obtain a given hue, a fifth less of the dye suffices when spring-water is employed instead of river-water.

In a final summary, in which he briefly recapitulates all the considerations which point out the marked superiority of the spring-water on the banks of the Saone to the Rhone water, as a potable beverage, fair, on the whole, as this may be, M. Dupasquier expresses his ardent desire that the project, alluded to at the commencement of this paper, of introducing these springs by means of an aqueduct, which would supply the wells of Lyons, might be carried into execution; for he considers the scheme as highly calculated both to improve and corroborate the health of all classes of his fellow-citizens, and also to contribute greatly to the maintaining of the reputation and advancing the superiority of their valuable manufactures.

On an Erroneous Deduction drawn by the late Captain Henry Kater *from his Experiment on the Flexure of Thin Bars.* By Edward Sang, Esq. Actuary, Edinburgh, M. S. A. Communicated by the Society of Arts for Scotland.*

In June 1830, Captain Kater read before the Royal Society of London a paper *on the Error in Standards of Linear Measure, arising from the thickness of the bar on which they are traced.* This error he states to have been discovered by him during the adjustment and verification of the copies of the imperial standard yard destined for the Exchequer, Guild hall, Dublin and Edinburgh; and he also states his belief, that the source of this error was previously wholly unsuspected.

On perusing the paper, it is found that the *error* in question arises, not from the thickness of the bar, but from its flexure or change of shape; and, with all due deference to the authority of Captain Kater, and the silence of the Royal Society of London, it is my distinct conviction that the change of dimension induced by mechanical agency was known very long ago, and particularly, that the very amount of change in the case before us could have been computed beforehand by help of formulæ known since the days of the Bernouillis. The anachronism of the discovery, however, is not that point to which I wish to draw the attention of the Society of Arts: I notice it merely because its occurrence may serve to explain

* Read before the Society of Arts for Scotland, 8th February 1841.

other errors more vitally affecting the progress of mechanical science.

When a rod is slightly bent, one side of it is distended, and the other side is compressed, so that between the two there must exist a locus of no compression and of no distension: it had hitherto been believed that this locus is in the middle of the bar, but Captain Kater has in this paper announced a new doctrine, viz., that the neutral plane is nearer to the convex surface of the bar, its distance from that surface being scarcely one-third part of the whole thickness.

If this inference be correct, it must follow that solid bodies resist distension with an energy eight times greater than that with which they resist compression. Yet all the experiments which have been made on this subject go to shew that, to moderate compressions and distensions, the resistances are alike, but that, on being pushed nearly to the destruction of the substance, the resistance to compression seems to be rather the greater of the two.

The discrepancy between these results and that of Captain Kater is so great as to leave no doubt on the mind that, in one set of observations or in the other, some important oversight has been committed; and I now proceed to shew that the oversight has been entirely on the side of the Captain.

When a thin bar is slightly bent, a shortening must take place in the distance between the two extremities of the neutral plane, since the straight line is shorter than the curved one; but the distance between two points on the upper surace will be augmented or diminished according to circumstances. Thus, if the upper surface be distended, and if its distension be greater than the shortening due to the curvature merely, an increase in the distance will be observed; while, when the upper surface is compressed, a shortening will appear equal to the sum of the shortening due to curvature, and of the shortening due to compression. So that, in order to obtain the effects due to compression or distension alone, the shortening arising from curvature, or, as it is called, the reduction of the arc to its chord, must be allowed for. This allowance has been made by Captain Kater, and although in this he has committed a treason against the laws of geometry, the amount of the allowance, in any case, is too small to have led to the mistake with which I have immediate concern. The mere effect of curvature, then, being supposed to be eliminated, there will remain the effects of compression or distension.

Taking a very crude view of the state of a bent bar, we may assume, that lines drawn originally perpendicular to its length when straight, remain normals to its surface when bent. Crude though this view may be, it affords a sufficient approximation for my present purpose. We may then regard the two dots on which the observations are made, as supported on two pillars resting on the locus of neutrality; and the distance between those dots (the reduction of the arc to its chord having been allowed for) will be changed by a quantity, the product of the height of the pillars by the sine of the inclination of each end of the bent bar.

Now, when the bar was supported in the middle, and its ends allowed to hang down, the lengthening was found to be scarcely half of the shortening obtained by supporting the ends and allowing the middle to be depressed, the versed sine of the arc being the same in both cases. Therefore, concludes Captain Kater, the height of the pillars in the one case must be scarcely half of the height of the pillars in the other: that is to say, the neutral plane must be twice as far from the concave as from the convex surface.

It must, however, be clear to any one who bestows a single thought upon the subject, that, since the half shortening or lengthening is the product of the distance of the neutral plane by the sine of the inclination, the comparison of the shortening with the lengthening cannot exhibit the relative distances of the neutral planes in the two cases, unless the inclinations of the curve be alike. Captain Kater has reasoned as if the curve in the one case were a copy of the curve in the other; but unfortunately for his logic, the two forms of the curve are essentially different, as may be known from the perusal of the works of any respectable writer on flexure. When a bar whose half length is l, is supported by its middle, the depression of the end is $\frac{1}{8}l^4$, multiplied by a certain constant depending on the nature of the material and thickness of the bar, while the inclination of the extremity is $\frac{1}{6}\,l^3$ multiplied by the same constant. But when the same bar is supported at the ends, the depression of the middle is proportional to $\frac{5}{24}l^4$, while the inclination at the extremity is as $\frac{1}{3}l^3$. Hence, from what was previously well known, it turns out that the inclination in the one set of experiments is exactly double that in the other, so that, supposing this ratio of one to two to have been found experimentally, we ought to have concluded that the plane of neutrality is equidistant from the two surfaces of the bars. Captain Kater's experiments, therefore, so far as they go, confirm the truth of the old doctrine, and refute this new one propounded by himself.

This result has been obtained, by taking, as I have said, a very crude view of the state of a bent rod. Let us conceive the rod, while straight, to have been marked off into a multitude of rectangular portions : these rectangles have been supposed to change into trapeziums by the flexure. This supposition is, however, but a rough approximation, barely admissible in the case of a thin bar, very slightly bent. The true form of the rectangular portions of a solid, after that solid has been subjected to pressure, is yet unknown ; the want of that knowledge being one of the obstacles to the farther advancement of this branch of mechanical science. Seeing, then, the nicety of the instruments with which Captain Kater wrought, and the evident care which he had taken, I was in the hope of eliciting some information on this point from his experiments ; and, with this view, commenced a minute discussion of them. But at the very threshold of the inquiry, difficulties, most unexpected in such a quarter, at once arrested my progress.

In making the reduction of the arc to its chord, I found that Captain Kater had used some processes quite as startling as the proposition, the fallacy of which I have just exposed.

Some of the bars on which he experimented were longer than others, while the distance between the dots was in all cases nearly the same, viz. 36 inches. The difference between the whole arc and its chord having been computed, the reduction of the observed arc to its chord was thence found by direct proportion. Thus the reduction on an arc of 60 inches having beencomputed at .0004000, that of a portion of the same arc 36 inches long, is put down .000240; by the *golden rule* 60 : 36 : : 400 : 240. Admitting the truth of this *entirely new doctrine*, it would be very easy to prove that a crooked line is no longer than a straight one having the same terminations. The question might here present itself to the anthropologist, how it is possible that an intellect capable of computing the reduction of any arc to its chord, could have entertained for one single instant such an idea? But, seriously, every tyro in trigonometry is acquainted with the equation, $\sin x - x = \frac{1}{6}x^3 - \frac{1}{120}x^5 + \&c.$, from which it is pretty clear that the reductions are as the cubes of the lengths of the arcs at least, and that, therefore, the reduction .000240 ought to have been .000086. But even this method of reduction is inapplicable, for the curve is not circular, and the reduction on one part of it is not equal to that on another; neither indeed is the reduction when the curvature is upwards necessarily equal to the reduction when the curvature is downwards.

From this specimen, it is quite clear that no dependence can be placed in any results at which Captain Kater may have arrived by computation, an error of this kind in a matter so purely elementary leaving room for the expectation of other blunders in simple matters. Perhaps, in the estimation of the value of his micrometric divisions, similar mistakes may have been committed. Nor, in looking more closely at the management of the experiments, is this feeling of insecurity at all lessened, for it is found that Captain Kater's idea of straightness led him to adopt, as the best attainable straight rule, a steel harpsichord wire stretched by a bow; and that he placed the whole weight of the micrometer apparatus upon the bar whose flexure he was examining.

It is almost needless for me to add, that, after pondering on these matters, the idea of attempting to draw any safe result from the recorded observations was abandoned.

*On Trigonometrical Surveying and Levelling, and on the Effects of a Supposed Local Attraction at the Calton Hill.** By William Galbraith, M.A., M.S.A., Edinburgh; F.R.A.S., London.

In the course of some years past I have laid before the Society of Arts for Scotland a few remarks relative to the imperfect state of those usually reckoned our best maps of the country, and pointed out some instances of the amount of error.†

I also proposed formulæ and rules to be observed in the practice of accurate trigonometrical surveying and levelling, which, in my opinion, might be easily understood and readily applied by any one possessing a moderate share of scientific knowledge. As a continuation of these, I shall, with permission of the Society, read the following few remarks on what has already been written, as well as on some connected observations which I have been enabled to make during the months of August and September last in the immediate vicinity of Edinburgh.

I am gratified to find that the data on which the geodetical tables previously given were formed have been confirmed by a recent memoir of M. Bessel, the learned Astronomer of Koenigsberg, who had the benefit of the perusal of the results of several arcs of the meridian measured in Germany and Russia, communicated to him in manuscript, which were, consequently, unknown to me. It is fortunate, therefore, that M. Bessel's value of the radius of the equator deduced from these, combined with others formerly known, exceeds mine by 166 feet, and his value of the polar semi-axis exceeds mine by 334 feet only—small quantities in about twenty millions of feet.

He also determines the most probable value of the French metre to be 443.321 French lines instead of 443.296 lines,

* Read before the Society of Arts for Scotland, 14th December 1840.

† Through the errors in our charts, or the ignorance of our pilots, or both combined, the Lords of the Admiralty themselves have lately been put in peril. Will this contribute in any degree to hasten the survey of the British Isles?

that adopted by the French commission of weights and measures, as the ten-millionth part of the quadrantal arc of the meridian. The difference of these amounts to 0.025 of a line, or about $\frac{1}{450}$ of an English inch—a very considerable quantity in nice operations. Hence the French metre, if lost, cannot be found again by taking accurately the ten-millionth part of the quadrant of the meridian. It must be taken from the standard metre of the French Archives, just as our yard must be taken from our standard yard, or from its copies, since the original was lost when the late Houses of Parliament were burned.

It is certainly of great consequence to possess a uniform standard of weights and measures, which should be adopted with caution and enforced by legal penalties, otherwise these changes tend to render confusion worse confounded. Previous to the late act relating to weights and measures, we had one generally admitted Scotch acre, containing in round numbers 6084 square yards, which, since the passing of that act, has been accounted 6104 square yards, especially about Glasgow and the west of Scotland. Hence the direct consequence resulting from the passing of that act has been to augment the variety of our measures instead of reducing them.

Mr Airy, indeed, the Astronomer-royal, determined also some time ago the values of the earth's semi-axes, in an article on the magnitude and figure of the earth in the Encyclopædia Metropolitana, and these, though exceeding all others of the greatest authority by about 1000 feet, have been adopted by Sir John Herschel in his Treatise on Astronomy. Notwithstanding my respect for the authority of both these names, I believe they can hardly be put in competition with that of M. Bessel on such a question, and as my results agree nearly with his, I may be excused, in the present state of our knowledge, for still adhering to them.

In determining the heights of mountains trigonometrically, these tables must be employed either singly or in combination with the effects of terrestrial refraction. For most ordinary purposes the mean effect of refraction, generally taken at 0.08, or $\frac{1}{12.5}$ of the intercepted arc, may be thought sufficient, when simultaneous and reciprocal observations cannot be obtained from which it is usual to deduce it by experiment. Indeed the latter method cannot be generally followed without great expense both in money and time. If, for example, an observer on the summit of Benlomond should proceed to take the elevation or depression of a hundred mountains round his range of the horizon, then to make reciprocal and simultaneous observations on Benlomond from all these hundred mountains, it would require one hundred surveying parties placed on their summits provided with all necessary instruments and accommodations, thus entailing an expense upon the country far surpassing its finances, great as they may be, or otherwise causing a corresponding loss of time to enable two parties to accomplish the same end. It is clear, therefore, that, though in some peculiar situations this mode may be occasionally followed to serve particular purposes, such as determining the amount of terrestrial refraction now and then, which may be sufficient for ordinary practice, yet it cannot be generally followed.

From these considerations it has been an object with me for a considerable number of years past, during which I have, in conjunction with others, made numerous observations, barometric and thermometric, on many of the mountains of Scotland, to deduce, among other results, from these, combined with known laws regulating the refringent power of the atmosphere, such as pressure, temperature, elastic force of vapour, &c. a formula depending upon the employment of the usual meteorological instruments, that would give the co-efficient of terrestrial refraction independent of reciprocal and simultaneous observations, which, from what has been just said, are frequently, and at least generally, impracticable in an extensive survey.

In order to determine the co-efficient of terrestrial refraction for pressure and temperature at the place of observation, we have from the known laws of hydrostatics

$$n = \mathrm{R\,D}\frac{r}{4l} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

in which n is the co-efficient of refraction, R the refringent power of dry air, D its density, r the mean radius of the earth, and l the height of the homogeneous atmosphere.

Introducing the effect of aqueous vapour upon the density of dry air

$$D' = D\left(1 - \frac{3f}{8B}\right) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

where f is the elastic force of aqueous vapour, and B the height of the barometer.

In like manner, if R′ be the refringent power of the air under the same circumstances, while R is that at the pressure B, the standard at which most of our experiments have been made, that is, about $0^{m}.76$ of the metrical barometer, or 29.9218 inches of the English barometer,

$$R' = R\left(1 + \frac{f}{12B}\right) \cdot \frac{1}{1 - \frac{3f}{8B}} \quad . \quad . \quad . \quad . \quad (3)$$

Now if Δ be the density of mercury at the freezing point when B is the height of the barometer,

$$l = \frac{\Delta}{D} \cdot B = 10466.8\,B = 26100 \text{ English feet} \quad . \quad . \quad . \quad . \quad (4)$$

when B $= 0^{m}.76$, or 29.9218 inches, the pressure at which $\frac{\Delta}{D} = 10466.8$ was determined.

The expansion of dry air for 1° of the centigrade thermometer here designated by β is generally estimated at 0.00375 ; hence

$$B' = B(1 + \beta t) \quad . \quad . \quad . \quad . \quad . \quad (5)$$

therefore by substitution for B and D in formula (4),

$$l' = \frac{\Delta}{D} \cdot B \cdot \frac{1 + \beta t}{1 - \frac{3f}{8B}} = l \cdot \frac{1 + \beta t}{1 - \frac{3f}{8B}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

Putting these in equation (1) and $n = R'\,D'\,\frac{r}{4l'}$ becomes

$$n = R\,D\frac{r}{4l}\left(1 + \frac{f}{12B}\right)\frac{1 - \frac{3f}{8B}}{1 + \beta t} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

The value of f, the elastic force of aqueous vapour, may be taken from the well-known table of Dalton, or from one given in my Mathematical Tables from a formula of Mr Ivory, founded on the experiments of Dr Ure. If thought necessary, in such cases as this the true elasticities may be determined under given circumstances by formulæ (A) and (B), page 60 of the same work.

From various experiments on the refringent power of the air, and by numerous astronomical observations, the co-efficient of the astronomical refractions at the freezing point and 30 inches of the English barometer, is

$$\alpha = 0.000294144 = \tfrac{1}{2}\,\mathrm{R\,D} \text{ nearly} \quad . \quad . \quad . \quad (8)$$

This value of α becomes at 30 inches of the English barometer and 50° of Fahrenheit's thermometer

$$\alpha = 0.000283003 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (x)$$

This is the standard pressure and temperature of Ivory's table of refractions, which, on account of their extreme accuracy, I generally prefer, and to whose standard, for the purpose of employing the auxiliary tables accompanying it, the following formula will be adapted. The co-efficient of astronomical refraction here assumed at 45°, or $\alpha = 0.000283003$, equal to the length of the circular arc to radius unity, or equal to 58″.38, agrees closely with 58″.36, that adopted by Ivory for his table.

But this co-efficient varies directly as the pressure, and inversely as the temperature, of the atmosphere, consequently

$$\alpha' = \alpha \frac{b}{\mathrm{B}} \cdot \frac{1}{1 + \beta t} \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

in which b is the given pressure, B the standard, and t the given temperature.

In like manner, the barometer must be corrected for the expansion of mercury by heat. Let this expansion for 1° of the centigrade thermometer be denoted by β', while τ is the temperature of the mercury indicated by the attached thermometer, and equal to 0.00018, then

$$\alpha' = \alpha \frac{b}{\mathrm{B}} \cdot \frac{1}{1 + \beta\, t} \cdot \frac{1}{1 + \beta'\, \tau} \quad . \quad . \quad . \quad . \quad (10)$$

Combining the results in (8), (9), and (10), in formula (7), and it becomes

$$n = \frac{\alpha\, r}{2\,\mathrm{B}} \cdot b \cdot \frac{1}{1 + \beta t} \cdot \frac{1}{1 + \beta' \tau} \left(1 + \frac{f}{12 b}\right) \frac{1 - \frac{3 f}{8 b}}{l\,(1 + \beta\, t)} \quad . \quad . \quad . \quad (11)$$

By observation it has been found necessary to modify this expression by considerations depending upon the difference of altitude, and the expansion of moist air. From barometric measurements of heights, and experiments on sound about the years 1827, 1828, &c., I found that a given volume of moist air in its mean state expanded from 1 to 1.4112 when heated from the freezing to the boiling point. I shall call this expansion for 1° centigrade β'', and equal to 0.004112. The barometric measurements to which I here refer were made on the Cheviot range, on the Pentland range of hills, on Benlomond, Ben Nevis, &c., and the experiments on sound were made in Fife by taking advantage of the reports of the guns fired on Edinburgh Castle in honour of the royal birth-days, coronation anniversaries, &c., thus turning, as far as I could, these holiday rejoicings to the benefit of science, since I had neither the means nor the influence to procure experiments to be made expressly to serve my purposes.

This co-efficient was introduced by me into a formula, which I gave about that time in one of our public journals, to determine the velocity of sound, and it almost uniformly brings out results conformable to experience. It becomes 0.00230 for 1° of Fahrenheit's thermometer, and this was also introduced into a set of tables published by me in 1838 for the purpose of calculating heights by means of the mountain barometer, which give correct results for all heights when the barometric measurements have been accurately made under favourable circumstances.*

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* Mr Francis Baily gave, among his collection of Astronomical Tables, printed in 1827, Table xxxvi. to compute heights by the barometer. In its application he neglected to state, that when subtracting the temperature at the higher station from that at the lower, if the result become negative, which it will be when the temperature at the higher station is greater than that at the lower, a circumstance that may occur on some rare occasions in small heights, then the logarithmic correction must be applied to the higher barometer with the same or a *negative* sign, instead of an additive as commonly happens.

Though to a mathematician this could be of little consequence, as he would naturally apply this correction according to its proper sign, yet to guard against any misapprehension on the part of those little acquainted with the use of signs, I carefully warned computers against any error from this cause in the last sentence of Rule 3, page 10 of the explanation of these tables.

The same thing was done by Mr Howlett of the Ordnance Map Office some years afterwards, among the professional papers published by the Royal Engineers in their first number.

M. Poisson, in his excellent Traité de Mecanique, second edition, published in 1833, gives, on the authority of Dulong, the quantity 1.421, a number exceeding mine slightly. I therefore conclude that the data thus determined possess the requisite accuracy.

I have been very minute in stating the foundation of the constants employed in my formula, because writers of celebrity have introduced generally too small co-efficients, and consequently all the results from their formulæ are defective. This is the case with respect to Sir John Herschel's formula for the velocity of sound in the Encyclopædia Metropolitana, in which the expansion of moist air in its mean state is assumed at 0.00375 for 1° centigrade, instead of 0.004112 which I have found, or even 0.00421 from the experiments of Dulong, as reported by Poisson in chapter vi. Book Fifth, of his Mecanique, entitled, "Of the elastic force, and of the heat of gas."

For this cause it is that the formula of Sir John Herschel gives results all considerably smaller than those from actual experiment.

By these remarks I by no means design to throw discredit on any of the writings of that distinguished individual. They are merely introduced to guard those who trust the authority of names from condemning the formula which I am now investigating, because the constants employed are different from his.

From numerous observations on the mountains of Scotland already quoted, I have found that the last factor in formula (11), namely, $\frac{1 - \frac{3f}{8b}}{l(1+\beta t)}$ should be diminished by a quantity represented by $\frac{\beta'' \delta t}{\delta h (1 + \beta t)}$ in which, as already stated, $\beta' =$ 0.004112, and δt is the variation of temperature corresponding to δh, the variation of height.

Introducing this into formula (11), it becomes

$$n = \frac{\alpha r}{2 \mathrm{B}} \cdot b \cdot \frac{1}{1 + \beta t} \cdot \frac{1}{1 + \beta' \tau} \left(1 + \frac{f}{12b}\right) \left\{ \frac{1 - \frac{3f}{8b}}{l(1+\beta t)} - \frac{\beta'' \delta t}{\delta h (1 + \beta t)} \right\} \quad (12)$$

Now the same observations in a mean state of the atmosphere give an elevation of 240 feet to depress Fahrenheit's thermometer 1°, or 430 feet for 1° centigrade. From these considerations the last term of formula (12) becomes $\frac{0.004112\,\delta t}{\delta h(1+\beta t)}$, and since $\delta t = \frac{\delta h}{430}$, the preceding becomes by substitution

$$\frac{0.004112\,\frac{\delta h}{430}}{\delta h(1+\beta t)} = \frac{0.0000096}{1+\beta t}.$$

Combining the two last terms of formula (12) by means of the last expression, and they become in a common denominator

$$\frac{1-\frac{3f}{8b}}{l(1+\beta t)} - \frac{0.0000096\,l}{l(1+\beta t)}$$

Now since $l = 26100$ feet by (4)

$$\frac{1-\frac{3f}{8b}-0.0000096\,l}{l(1+\beta t)} = \frac{1-\frac{3f}{8b}-0.25}{l(1+\beta t)} = \frac{0.75-\frac{3f}{8b}}{l(1+\beta t)}$$

Replacing the factor consisting of the two last terms in formula (12) by this equivalent expression, it becomes

$$n = \frac{\alpha r}{2\,B}\cdot b\cdot\frac{1}{1+\beta t}\cdot\frac{1}{1+\beta'\tau}\left(1+\frac{f}{12b}\right)\frac{\frac{3}{4}-\frac{3f}{8b}}{l(1+\beta t)} =$$

$$\frac{\alpha r}{2\,B\,l}\cdot b\left(\frac{1}{1+\beta t}\right)^2\cdot\frac{1}{1+\beta'\tau}\left(1+\frac{f}{12b}\right)\left(\frac{3}{4}-\frac{3f}{8b}\right)$$

If the two last factors be multiplied together, the product is $\frac{3}{4}-\frac{5f}{16b}-\frac{f^2}{32b^2}$. Because $\frac{f^2}{32b^2}$ must always be a very small quantity, it may without sensible error be neglected, and the preceding expression becomes $\frac{3}{4}-\frac{f}{3b}$ very nearly. By means of these simplifications equation (12) finally becomes

$$n = \frac{\alpha r}{2\,B\,l}\cdot b\cdot\left(\frac{1}{1+\beta t}\right)^2\cdot\frac{1}{1+\beta'\tau}\left(0.75-\frac{f}{3b}\right) \quad . \quad . \quad . \quad (13)$$

Adapting this formula to 30 inches of the English barometer, and 50° of Fahrenheit's thermometer, it becomes

$$n = \frac{\alpha r}{2\,B\,l}\cdot b\cdot\left(\frac{1}{1+\beta(t-50^\circ)}\right)^2\cdot\frac{1}{1+\beta'(\tau-50^\circ)}\left(0.75-\frac{f}{3b}\right) \quad . \; (14)$$

Taking the values of α, B, l as formerly stated, and $r = 20922642$ feet $= a$, the radius of the equator equal to half the sum of the radius of curvature of the meridian, and of the arc perpendicular to the meridian at latitude 45°, which may for this and similar purposes be reckoned the mean radius of curvature of the earth very nearly.

To simplify the calculation of n, the log. of $\frac{\alpha r}{2\,B\,l}$ may be used as a constant log. where B = 30 inches and $t = 50°$ Fahrenheit, the standard pressure and temperature of Ivory's refractions. Whence the auxiliary tables accompanying them to correct for pressure and temperature may be employed, which greatly facilitates the application of my formula.

Log α = log of 0.000283003 . .	6.451791
Log r = log of 20922642 feet log . .	7.320616
$a . c .$ log 2	9.698970
B = 29.9218 $a . c .$ log . . .	8.524012
l = 26100 feet $a . c .$ log . . .	5.583379
Log $\frac{\alpha r}{2\,B\,l} = \bar{1}$ const log . . .	7.578768

The formula will now be applied to the determination of heights from observations which I lately made in the Firth of Forth.

Having procured, through the favour of Colonel Colby, from the Ordnance Map Office, such lines and angles as I thought desirable for this and other purposes, I made such additional observations as gave the distance between Inchkeith Lighthouse and my station on Inchcolm, from which, as a base, triangles were extended to several eminences in the vicinity of Edinburgh, such as Carnethy among the Pentlands, the height of which, in conjunction with Professor Henderson, I had determined with great care, in 1828, by the mountain barometer, from numerous observations taken every ten minutes during some hours. The same barometer was employed both at the sea-shore and at Carnethy Cairn, thus rendering the barometer employed at the Calton Hill merely one of comparison, thereby avoiding any error that might arise from the use of different barometers. The results were published in the Edinburgh New Philosophical Journal for October 1831. The height is computed there by two different methods, the mean of which is 1880 feet. I have repeated the calculations with my new tables, which give 1880.7 feet, agreeing almost exactly with the former. This height was verified by Mr

Jardine, civil-engineer, by direct levelling, and his results, I believe, differed little from the above, to which, as very near the truth, I shall still adhere.

As this height may therefore be considered well determined, I was desirous of performing the measurement of it trigonometrically with great care by means of the best instruments I possessed. That for angular measures was my altitude and azimuth astronomical circle, and for meteorological observations Mr Adie's portable sympiesometer. As the thermometer is a good one, the temperature must have been accurately determined, and though its indications of pressure are not quite so accurate as those of a good mercurial barometer, yet the differences will seldom exceed a few hundredths of an inch when in a fair state of adjustment, which ought to be repeated every two or three years.

I adopt this instrument for all such operations, geodetical and astronomical, in my travelling excursions, on account of its being less liable to accidents than the barometer, and its greater portability, while its indications are fully sufficient for all ordinary purposes.

This instance, then, affords me a fair criterion to judge of the relative accuracy of direct levelling, of barometric measurement, and of trigonometrical operations. When I find, as will immediately appear, that I cannot avoid considerable discrepancies in my own results, even by taking all possible care, while, at the same time, I employ the best data I can procure, combined with the co-efficient of terrestrial refraction by the formula just investigated, I shall then be compelled to form a more candid and charitable opinion upon the analogous operations of others.

I. At my station on Inchcolm on the 15th of August 1810, the observed zenith distance of the horizon of the sea down the Firth of Forth to the eastward was $90^\circ\ 8'\ 21''.2$, while the pressure of the atmosphere was 29.75 inches, and the temperature 64° Fahrenheit, the height of the station above the sea will be determined by the usual formula

$$d\,h = \tfrac{1}{2}\,\varrho\,(1 + n)^2 \tan^2 D \quad \ldots\ldots\ldots \quad \text{(A)}$$

in which ϱ is the radius of curvature in the given direction, n

the co-efficient of terrestrial refraction computed by formula (14), and D the observed depression or zenith distance, diminished by 90°.

In like manner, the height of one point above another will be obtained from the formula

$$d\,h = \text{K} \sec \tfrac{1}{2}\, u \cot \{\delta + (n - 0.5)\, u\} + \&\text{c. or}$$

$$d\,h = \text{A} \cot \{\delta + (n - 0.5)\, u\} \quad . \quad . \quad . \quad . \quad \text{(B)}$$

sufficiently near for moderate heights, such as those found in this country, in which K is the chord between the stations, u the angle between their verticals, and n the co-efficient of refraction, or, even using A, the arc between them in feet, and rejecting the other terms as insensible in our observations.

Constant log for (14)		7.57877
$t = 64°$ log × 2 (table xviii.)* . . .		9.97502
$\tau = 64°$ log (table xx.)*		9.99940
$\text{F} = 0.75 - \dfrac{f}{3\,b} = 0.75 - \dfrac{0.5904}{89.25} = 0.7434$ log .		9.87122
$b = 29.75$ in. log		1.47349
$n =$ 0.07905	log	8.89790
1.0		
$1 + n =$ 1.07905 log × 2 = ...		0.066082
$\tfrac{1}{2}\,\varrho = \dfrac{\tfrac{1}{2}\,\text{R}''}{\text{O}}$ log from geodetic tables . .		7.020582
D = 0° 8′ 21″.2 tan² log		4.771174
$d\,h = 72.1$ feet log		1.857838

The height of the station above the level of the sea at high water nearly by formulæ (14), and (A).

II. By a mean of several observations, the observed zenith distance of Carnethy Cairn was 88° 34′ 16″ from the same station, while the barometer stood at 29.725 inches, and the thermometer at 63°.5.

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* These are the numbers of the auxiliary tables accompanying Ivory's tables in my collection of Mathematical and Astronomical Tables. The value of f is taken from the same table, iv., page 64 of the introduction to them, while log. O is taken from my previous paper, in No. LI. of the Edinburgh New Philosophical Journal.

Const log as before	. . . . .	7.57877
$t = 63.5$ log × 2	. . . . .	9.97590
$\tau = 63.5$ log	. . . . .	9.99942
$F = 0.75 - \frac{f}{3b} = 0.7435$ log	. . . .	9.87128
$b = 29.725$ log	. . . . .	1.47312
$n =$ 0.07916 log	. . . . .	8.89849
-0.5		
$n - 0.5 = -0.42084$ log	. . . . .	-9.624117
Log M to lat 56° (see last paper)	. . . .	7.993722
K or A $=$ 67982.5 feet log	. . .	4.832397
$-u'' = -$ 0° 4′ 42″ . . . . . . — 282″ log	.	-2.450236
$\mathfrak{z} =$ 88 34 16		
$\mathfrak{z}_{,} =$ 88 29 34 cotangent	. . .	$+ 8.420155$
A $=$ 67982.5 log	. . . .	4.832397
$dh = + 1788.8$ feet	. . . log	$+ 3.252552$

Collecting these numbers, and allowing 8.5 feet for half the rise of spring tides, there will result

Half the rise of tide,	. . . .	8.5 feet.
Height of station above high water,	. . .	72.1 ...
Height of Carnethy above station,	. . .	1788.8 ...
Total height above mean level of sea,	. .	1869.4 ...
Barometric height verified by levelling,	. .	1880.7 ...
Defect of trigonometrical height,	. . .	11.3 ... *

Hence the probable error of this determination is about 11 feet too small.

III. The height of the summit of Carnethy was determined in a similar manner from Inchkeith.

> Mean zenith distance, ... 88° 46′ 51″.5
> B $=$ 29.78 in. $t = 60°$, and $\tau = 60°$ Fahrenheit.
> A $=$ 74701.5 feet, bearing about S. 22° W.
> Whence $n = 0.08055$, and $dh = 1701.5$ feet.

Here, however, the height of the station was got from Mr Robert Stevenson some years ago, who stated the height of the ground above high water by levelling to be 175 feet, above which the axis of the circle was 4 feet.

* This defect may in part be owing to the difficulty of determining the first height accurately by the depression of the horizon of the sea, especially in a narrow firth.

Now, allowing 8.5 feet for the half tide, as before, there will be obtained from this series

Half the rise of tide, ...	8.5 feet.
Height of ground at station, ...	175.0 ...
Height of circle, ...	4.0 ...
Height of Carnethy above circle, . . .	1701.5 ...
Total height, ...	1889.0 ...
Barometric height verified by levelling, . .	1880.7 ...
Excess of Trigonometrical height, . . .	8.3 ...

These differences of 8 and 11 feet only serve to shew that, by the new method of computing the co-efficient of refraction from the state of the barometer and thermometer, both more exact and more consistent results are obtained than by using a mean value generally, especially upon the summits of distant high mountains, where the pressure and temperature differ considerably from the mean.

It is long since a table of mean refractions has been rejected in astronomy where any attention is paid to accuracy, and after this example, it is to be hoped that the actual co-efficient of terrestrial refraction will be computed for pressure and temperature uniformly in the same manner as the astronomical refractions generally are. It was with this view that I have transformed my equation so as to be enabled to employ the same tables for both. Indeed, if the mean of our present deductions be taken, the first series gives . 1869.4 feet.

The second series, . . .	1889.0 ...
Of which the mean is, . . .	1879.2 ...

agreeing almost exactly with the barometric measurement, confirmed by Mr Jardine's levelling, though the difference between the two amounts to 19.6 feet, a difference likely owing to small unavoidable errors of observation and atmospheric irregularities which no care and address can obviate. From this conclusion, it also appears that all the three methods concur in bringing out the same conclusion when performed with equal care, though the same result could not be expected when any of them is hastily executed.

It has been known to several individuals for some years past, especially to those taking an interest in trigonometrical surveying, that the latitude of Edinburgh Observatory determined from actual observation there, and that deduced geodetically from observations made with the Ordnance zenith sector on Kellie Law in Fife, differ from each other by a considerable number of seconds. It occurred to me that this might be owing to local causes, and was of the same nature as that at Arburyhill in England, which at one time caused considerable speculation. Don Joseph Rodriguez, a Spanish mathematician, associated, I believe, with Messrs Biot and Arago in the prolongation of the French arc of the meridian along the eastern coast of Spain, attempted to shew geodetically that this was owing probably to errors made with the Ordnance zenith sector. It was then contended by the defenders of our survey, and admitted by Delambre, that geodetical deductions could never be employed to test effectually astronomical observations.

The Ordnance zenith sector has been lately tested by a comparison with the Greenwich mural circles, and I understand the comparison has proved satisfactory. Besides, much greater discrepancies have been recently detected on the Continent, as might naturally have been expected from the action of greater mountain-masses, such as the Alps, Pyrenees, &c.

It might have been supposed that one of the points in our survey most to be depended upon would have been the observed latitude of Greenwich, where astronomical observations have been made for a hundred years with the best instruments which art could contribute to science.

The result given to the late General Mudge by Dr Maskelyne was 51° 28′ 40″ N., while that lately given by Mr Airy in the Greenwich Observations is 51° 28′ 38″ N., less than the preceding by 2″. How are our surveyors to dispose of these discrepancies, proceeding from the highest authorities? Again, Professor Henderson gives the latitude of the Calton Hill Observatory at 55° 57′ 23″ N., while the zenith sector observations on Kellie Law, reduced to the Calton, give 55° 57′ 16″ N., less than that by direct observation by 7″.

Now, I entertain no doubt that both these results are fair deductions from very accurate observations reduced with great precision. In making the usual deductions of latitudes geodetically for other points connected with these, what ought our surveyors to do? In my opinion, an inquiry ought to be instituted to discover whether one or both of these places are likely, from their geological position, to be affected by the irregularities of local attraction. If so, from the sudden fall of the ground north of the Calton towards Leith, with the Firth of Forth following in succession, the Calton is likely to be more affected than Kellie Law, situated nearly in the centre of the east *neuk of Fife.*

From these considerations, it occurred to me that Inchkeith would probably be a good point to test the accuracy of these views. I therefore applied to the Ordnance Map Office for their results, which, with the permission of Colonel Colby, were readily granted me, while my friend Mr Stevenson very kindly gave me an introduction to the principal light-keeper, to allow me such accommodation as the lighthouse afforded. I am consequently under great obligations to both these gentlemen for giving every facility to me in their power, to enable me, as far as possible, with the instrument which I possessed, to examine this curious anomaly.

I had, during some years past, determined the latitude of my own residence to be 55° 56′ 57″.89 N. from some hundreds of observations by the same circle I was now about to use. By a trigonometrical operation compared with, and checked by, the results in Professor Wallace's Treatise on Geometrical Theorems and Formulæ, lately published, page 140, I reduced this latitude to the Observatory, by adding 25″.67 to it, which gives 55° 57′ 23″.56 N. for the latitude of the Observatory, exceeding Professor Henderson's result by a small fraction of a second only. This circle, then, gives results, when the observations are very numerous, that accord with those of the mural circle when employed in its vicinity, and the question with me was, Would it likewise do so at Inchkeith, where it is probable local attraction would be insensible? For this purpose, I resided on the island a few days, during which I made several observations on the heights in the vicinity of Edinburgh, already partly given in the preceding portion of this paper, as well as some on the latitude, but I have chiefly trusted those made on the 21st of August on the pole-star, which I continued to observe from about 10 o'clock in the evening to 1 o'clock next morning. During this period I completed eight series of double observations, reversing the circle each time, or sixteen single observations, comprehending forty-eight readings of the verniers, accompanied by the times of observations, and the indications of the level.

The circle, indeed, is rather small for examining so delicate a question, being only six inches in diameter, having three verniers, each shewing 10″, and a level whose divisions each indicate 2″. I shall, however, claim no more for it than what competent judges may be inclined to grant; and they may put such confidence in my final result as they conscientiously believe it deserves. I may add, however, that I believe it to be in contemplation by Colonel Colby to examine this point carefully at no distant period, by means of the Ordnance zenith sector, which will be set up somewhere in this vicinity expressly with that view.

For ordinary purposes the tables to determine the latitude by the pole-star at the end of the Nautical Almanac are very convenient when no great precision is wanted, and the interpolation made by even proportion. I used these chiefly as a check, however, and made my computations by the following formulæ, easily investigated, and in the application of which I used proportional logarithms to five places of decimals, which, for this purpose, give results in general sufficiently accurate.

Let t be the sidereal time after the upper transit of the pole-star, and p its polar distance.

1. Log secant t + prop. log p = prop. log u. Now if λ be the latitude of the foot of the perpendicular arc from the star upon the meridian, and z the true zenith distance,

2. Log sin λ = log sec p + log cos u + log cos z. Now making l the true latitude,

3. $l = \lambda \pm u$

where it must be observed that in the 1st and 4th quadrant after transit the sign of u is $-$, while in the 2d and 3d it is $+$.

4. Prop. log m = log cosec t + log cos λ + prop. log p, in which m is the azimuth reckoned from the north reduced to the horizon, which, compared with a referring lamp* by means of the horizontal circle, gives the azimuth along with the latitude at any point of the star's revolution. The usual formula for computing the effect of a level reading from a central zero upon the zenith distance is

$$l = \frac{(e - o)\,a''}{2\,n} \quad . \quad . \quad . \quad . \qquad \text{(C)}$$

in which l is the effect, e the sum of the readings of the scale at the eye-end of the telescope, o that of the object-end, a'' the value of one division of the scale of the level in seconds, and n the number of observations. The scale of my level originally read to three seconds, but on considering the preceding formula (C) should it become 2″, then the formula itself becomes

$$l = \frac{e - o}{n} \quad . \quad . \quad . \quad . \qquad \text{(D)}$$

Whence, in this case, it is neither necessary to multiply by the value of the divisions of the scale, nor to double the number of observations, to get the effect of the level, but merely to divide the difference of the sums of e and o by the number of observations simply, at the same time taking care of the sign to correct the zenith distance for the effects of the level. On this account I got Mr John Adie to redivide the scale of my level in accordance with these views previous to commencing my observations this season.

Having made these preliminary remarks so that every thing relative to my operations may be fully understood, I shall record the first series of observations, and perform the computation at full length, so as to render the whole operation clear and distinct to every one having a very ordinary knowledge of such subjects.

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* A distant lighthouse will frequently answer the purpose of a referring lamp very successfully.

Polaris.

1840, August 21. B = 29.70 in. $t = 64°$

Error of Chron. at 10¼ P. M. Fast 1^m $58^s.4$, Rate $19^s.7$ gaining.

	Times.	Ver.	Z. D.	Level. + —
	h. m. s.		° ′ ″	e o
1.	10 9 5	A	33 34 30	24 22
		B	34 25	
		C	34 30	
2.	10 19 35	A	33 27 20	22 23
Mean	10 14 20	B	27 20	46 45
Error Cr.	— 1 58.4	C	27 30	45
M. T. I.	10 12 21.6		33 30 55.8	2|1
Long.	+ 12 32.0	level +	0.5	0″.5
G. M. T.	10 24 53.6		33 30 56.3	
		refr. +	37.2	

Z. D.	33° 31′	log 1.5876
B =	29.70 in.	log 9.9956
t =	64°	log 9.9875
τ =	64°	log 9.9994
r =	37″.2	log 1.5701

True Z. Dist. = 33 31 33.5

	h. m. s.
Sidereal time at Greenwich, M. N.	9 59 29.28
Mean time at place of observation,.	10 12 21.60
Reduction to Greenwich M. T. 10^h 24^m 54^s . . .	+ 1 42.65
Sidereal time of observation, ...	20 13 33.53
Star's Right Ascension, ...	1 2 36.78
t = Sidereal time after transit, ...	19 10 56.75

By the preceding formulæ,

t =	h. m. s. 19 10 56.75	sec 0.51622	cosecant . .		0.02115
p =	1° 32′ 33″.80	P.L. 0.28884	sec	0.000158	P.L. 0.28884
u =	— 0 28 11 .8	P.L. 0.80506	cos	9.999986	
		Z.D. 33° 31′ 33″.5	cos	9.920976	
λ =	+ 56 30 9.2	. . .	sin	9.921120	cos 9.74186
l =	56 1 57.4 N.		m' =	N. 2° 39′ 44″.6 E.	P. L. 0.05185

Table III. Surveying gives $\frac{2}{3} \Delta \tan a$ = cor = — 4 .6

m = N. 2 39 40 .0 E.

Hence the latitude l, deduced from this series, is 56° 1′ 57″.4 N., and the azimuth of the star referred to the horizon, m = N. 2° 39′ 40″.0 E.

Now, the reading on the horizontal circle being recorded at each observation, and referred to a lamp in a given position, the azimuth of the lamp becomes known, as well as those of any number of points whose angular bearings are compared with it by observation. The latter, not being my present object, are omitted. The following are the results of all my observations for latitude computed in this manner.

Obs.		Particular Values.	Successive Results.
		° ′ ″	° ′ ″
1.	l = .	56 1 57.4 . .	56 1 57.40 N.
2.	. . .	2 1.0 . .	1 59.20
3.	. .	2 1.9 . .	2 0.10
4.	. .	2 1.8 . .	2 0.52
5.	. .	1 57.0 . .	1 59.82
6.	. .	1 59.3 . .	1 59.73
7.	. .	2 0.2 . .	1 59.60
8.	. .	2 0.3 . .	1 59.86

Hence the final latitude of the point observed is,	56 1 59.86 N.
Reduction to centre of tower or lantern, .	0.24
Latitude of light, ...	56 1 59.62
Colonel Colby's results from Kellie Law observations,	56 1 59.00
Difference, ...	0.62

In the first six series of observations the star was bisected as accurately as possible, but in the two last the wire was made a tangent to the star.

In the seventh the star was apparently above the wire, or really below it, and the latitude came out, ...	56° 1′ 55″.2
In the eighth apparently below, . . .	56 2 5 .3
Making the mean, ...	56 2 0 .25
Difference, ...	10 .1
Half difference, or half the thickness of the wire,	5 .05

Which, applied to each of the observations, brought out the results of seventh and eighth above.

Mechain, as stated in the "Base du Système Métrique, tom. ii." p. 193, always supposed the thickness of the wires of the telescope belonging to his repeating circle to be 6″, though Delambre found the thickness of his to be 8″, 10″, or 12″. The thickness of the wires of my telescope, however, ought to have been greater, unless they be finer, since it is less powerful than theirs.

To conclude, in the Calton Hill Observations, volume i. Introduction, page xxxviii., the latitude of the Observatory is stated at 55° 57′ 23″.2 N., which, combined with the bearing of Inchkeith, page xii. and known distance 30272 feet, gives 56° 2′ 5″.8 N. for the latitude of Inchkeith Lighthouse, exceeding Colonel Colby's by 6″.8, and mine by 6″.2. To what cause to attribute this I cannot positively say, though partial local attraction is sufficient to account for it, since, in other situations, it has been known to produce greater discrepancies between the observed and geodetic latitudes than what has been here determined. No doubt, in the course of time, future observations will be made in greater number, and with superior instruments to mine, in order to explain this anomaly in a satisfactory manner. I cannot, however, understand how, on any other principles than those of local attraction, my circle should give results agreeing with those of the mural circle at one place, and disagreeing at another. Till then, I may be allowed to infer from the observations I have made and detailed in the preceding paper, that the anomaly *is owing to a partial local attraction at the Calton Hill*, whereby the plumb-line is deflected towards the south, thereby throwing the zenith point towards the north, making the observed latitude exceed the true by about 6″ or 7″.

1. In assuming the decrement of heat at 240 feet for 1° Fahrenheit in this country in the preceding investigation, as being sufficiently accurate for my present purpose, I still adhere to my conclusions in a former paper on that subject read to this Society, namely, that the law of decrement involves functions of the height and latitude, which is borne out by the fact, that the decrement for considerable heights, and for high latitudes, is greater than that derived from small heights and low latitudes. It is, generally speaking, greater from Ramond's great heights than from smaller, and that from the barometric observations of Captains Sabine and Foster to determine a height at Spitzbergen, in latitude 80° N., there was no sensible change of temperature for an elevation of 1640 feet.* From these remarks it would also follow that the decrement will also change with the season, and also with the hour of the day. This is partly the reason why even the astronomical refractions in the Arctic regions were found by the late Captain Foster to be very different from those in more temperate climates.

* From a mean of the whole. Some of them indeed indicate a greater temperature at the top than the bottom.

2. In carrying out these views, will the Koenigsberg Astronomical Refractions, given by M. Bessel in the Tabulæ Regiomontanæ, be conformable to the climate of Greenwich? To me there appears to be some doubt, even though they have been adopted by Mr Airy, the Astronomer-Royal there.

3. If the formula (14) be restricted to primary triangulation, by assuming a mean value for

F = 0.7456, there will be obtained for secondary,

$$n = \frac{\alpha r \mathrm{F}}{2 \mathrm{B} l} \cdot b \left(\frac{1}{1 + \beta (t - 50^\circ)} \right)^2 \cdot \frac{1}{1 + \beta' (\tau - 50^\circ)} \quad . \quad . \quad . \quad . \quad (15)$$

II. Const $\log = \log \frac{\alpha r \mathrm{F}}{2 \mathrm{B} l} = 7.451274.$

4. If, instead of using proportional logarithms, the usual tables of sines, tangents, &c. be employed, the following formulæ must be substituted in place of those at page 341.

1. $\tan u = \tan p \cos t.$
2. $\sin \lambda = \sec p \cos u \cos z.$
3. $l = \lambda \pm u.$
4. $\tan m = \tan p \sin t \sec \lambda.$

Here u is *minus* in the first and fourth quadrants of t, and *plus* in the second and third.

In like manner m is west in the first and second quadrants of t, and east in the third and fourth.

These formulæ may all be included in the same operation as in the example in page 343, and they will be found very useful in finding latitudes and azimuths by the pole-star.

P. S.—Since the preceding paper was drawn up I have applied the small correction to the latitude, on account of the irregularity of the motion of the star between observations, first pointed out by Soldner, and estimated the probable error by the formula of Baron Fourier, and the final latitude of Inchkeith Light is 56° 1′ 59″.82 N., with a probable error of ± 0.″5. The Baron's formula, however, shews merely the deviation from consistency, and not the absolute error, because it takes no account of constant errors in the instrument, &c.

Notice of a New Signal-Light for Railways. By ALAN STEVENSON, LL.B. Civil Engineer, Edinburgh. Communicated by the Society of Arts for Scotland.*

THE numerous accidents attended with fatal consequences which have lately occurred on railways have excited much alarm in the public mind, and the prevention of these casualties is unquestionably a matter of great importance. The object of this communication is, to point out one source of danger to which several of the late accidents may be attributed, and to suggest the means of its removal; and from the personal interest which all must have in the improvement of railway-travelling, both as regards its speed and, what is of much greater importance, its safety, I venture to hope that the following observations, although limited to one part of the subject, will not be found to have been unsuitably addressed to a society whose province it is to improve the useful arts.

One of the most imperfect parts of the railway system is undoubtedly the uncertainty of the night signals, and to this it is well known many of the most fatal of the accidents which have occurred must be traced. The great object of these signal-lights is, to announce that the train has reached a certain point of its course, and to forewarn the engineman of his approach to a station, or the junction of a branch railway, so that the speed of the engine may be checked in proper time to prevent collision. The lights used for this purpose are generally exhibited at the place the approach to which they are intended to announce; but the distance at which light projected horizontally, may be seen by a person approaching in the line of its transmission is very variable according to the state of the atmosphere, which in our climate is subject to great and sudden changes, in regard to clearness and fog. These variations in the visibility of lights of extensive range are by no means confined within narrow limits, as experience too amply demonstrates in the case of lighthouses, whose range has been known to vary with the state of the atmosphere, from sixty

* Read before the Society of Arts for Scotland, 22d February 1841.

miles down to two or three miles ; and this evil is unhappily one of those which, in the present state of chemical and optical science must, we fear, be pronounced irremediable. This defect, great as it is in regard to lighthouses, is, in the case of railways, materially aggravated by the excessive velocity of railway-travelling. Any variation in the distance at which a signal-light is first seen, must lead to great misconceptions as to the time of reaching a station, and all such misconceptions are fraught with the worst consequences, owing to the numerous sources of danger from the crossings of branch lines, the meeting of carriages on the rails, or the occurrence of other accidents, which may render a railway impassable. It is therefore obviously indispensable to safety that the signal-lights should be so constructed, that in all states of the weather they shall be constantly visible at the same point, and that this point shall be sufficiently distant from the station, the approach to which the signal is intended to announce, so as to allow ample time for checking the engine's speed before coming up to it; and upon no other grounds can the confidence of the public as to their security be reasonably based.

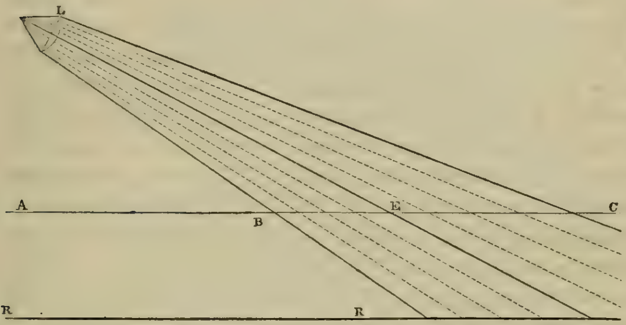
In the month of December last, it occurred to me in the course of conversation with my friend Mr Errington, civil engineer, that although the variation in the visibility of lights of distant range must, according to our present knowledge, be regarded as an evil without remedy, it might still be possible, by means of some arrangement of the lights, to render *signals for railways constantly visible at the same point during every state of the atmosphere.* For this purpose, all that seems to be necessary is, to *limit* the range of the lights, and at the same time to increase their intensity in such a manner that the combination of a short range with great power may not merely render them capable of penetrating any fog however dense, but of producing, at a certain point, an effect so brilliant and striking as forcibly to arrest the engine man's attention. After considering the matter in various points of view, I came to the conclusion that the object could be best attained by placing the light considerably *in advance* of the station, the approach to which it is intended to announce, and by giving the beam such an inclination to the horizon, that its greatest power may fall upon the engineman's face, at so short a distance from the light itself, that it could not fail to be always visible at that point, even in the thickest fog.

According to the present practice, a comparatively feeble light is exhibited at the station whose position it is intended to point out, and this light, which is permitted to pierce the gloom until its power is greatly diluted by the united effects of its own divergence, and the length of its passage through a foggy medium, must necessarily be subject to constant variation of visibility with every change of the atmosphere. The change which I have to suggest, is to place a light of great power about a mile in advance of the station, and at the same time to limit its range by the depression of the resultant beam within such a distance as to ensure its being visible at all times.

The arrangement I would propose for the attainment of this object is remarkably simple, and consists in placing one of Fresnel's annular lenses, illuminated by a gas or oil burner, as may be most convenient, in a small chamber, glazed in front, and supported on a stage of carpentry of sufficient size to span the rails, and permit the train to pass under it; but the purpose might perhaps be equally well served by placing the stage at the side of the railway, and inclining the beam obliquely to the line. In order to limit the range of the lens to a short distance, and thereby to ensure the light being visible in all states of the weather at the same point, I would incline the instrument, so that the length of the trajectory from the lens to the observer's eye should not exceed about 700 feet, which falls far short of the distance at which the light of the lens would be obscured even in the thickest fog. I may remark that the inclination of the lens is too small to require any correction in the position of the flame; but this could be easily accomplished if necessary, more especially when gas is employed. In curved lines of railway the same effect might in certain cases be produced by placing the lens on a level with the observer's eye, and directing the refracted beam so as to cut the railway obliquely. In this case the limitation of range would be produced without the necessity of inclining the lens; but the principle of rendering the signal at all times effective, by combining a short range and a powerful light, is the same in both arrangements.

The advantage of this arrangement I conceive to be great, for not only would the light be at all times visible to the engineman on his arrival at the same point, which, as already mentioned, is really the great object of signal-lights; but it is obvious that his attention would be most effectually awakened by the contrast of suddenly passing from darkness to receive the full effect of a powerful light viewed from a short distance. One other advantage of the proposed signal-light, I must observe, lies in its being peculiarly susceptible of any modification of colour, whether of a temporary or permanent kind, which the numerous and growing wants of an extended railway-system may require. The alphabet of nocturnal telegraphy, wherever a distant range is required, is unhappily extremely scanty; for the practice of all Europe seems to have shewn that, so far as colour is concerned, *red* and *white* are its *alpha* and *omega:* green and blue have been frequently tried; but cautious inquirers have all agreed in pronouncing them so equivocal when viewed from a distance, that they have been almost universally abandoned. These colours, however, and even much less marked varieties, although useless as distinctions for lights of distant range, are perfectly effective when viewed from short distances, as the brilliant display of an apothecary's window sufficiently proves.

I shall now add a very few words regarding what appears to me to be the chief arrangements which may in practice be found necessary for signal-lights on these principles; but I would not be understood as attempting to fix any thing permanently, for I am well aware that various modifications may be suggested by experiment, which I do not at present foresee in their full extent; in particular, it seems probable that the range of visibility which I have adopted in the following view of the details falls short of what will be found quite sufficient in practice even during the thickest fogs, when a light so powerful as that which may be derived from Fresnel's lens is brought into play; and should this expectation be realized, the duration of the effect of the light, which depends on the range, might be increased beyond what I have ventured to state.



Referring to the above sketch, I would propose that the lens at L should be elevated 24 feet above the rails R R, or about 15 feet above the level of the engineman's eyes; and that the point where the centre of the beam would intersect the horizon, A C, of his vision at E, should be about 700 feet from the lens. The impulse of the light would be most advantageously received at some point as near the lens as is consistent with a full effect from a flame placed in its principal focus. A more remote observer would receive the rays diluted by distance; while a nearer approach of the eye to the lens would render it necessary to adopt an ex-focal arrangement, so as to cause convergence of the rays. By the latter arrangement their divergence would be decreased, and the space covered by the light would be lessened not only in proportion to the decrease of divergence, but also to that of the cosine of the beam's inclination to the horizon. Both these circumstances would therefore combine to curtail the duration of the impression on the eye.

It may naturally be expected that I should say something regarding the duration of the impulse of the light on the eye; and upon this topic I shall, in absence of actual experiment, content myself with stating briefly the result of my calculations. If we suppose that an effective divergence of only 2° were to be obtained (and this is just one-third of what is obtained from Fresnel's lens with the great lamp), I find that the light would spread itself along the horizon of the observer's eye between B and C to the distance of about 1000 yards, which, at the speed of 40 miles an hour, would be passed over in about 50 seconds, but at the ordinary railway speed of 25 miles an hour, about 80 seconds, or $1\frac{1}{3}$ minute, would be required. Such a flash of light falling upon the polished parts of the engine, and upon the observer's face, would undoubtedly act as a most effective signal. If, however, it should be thought advisable to increase the duration of the impression by spreading it over a greater length of the line, this effect could be easily produced by a slight alteration of the inclination of the lens, so as to cause the line of railway to cut the refracted beam more obliquely; but I by no means expect that any such modification would be found necessary in practice. The nearness of the eye to the lens, and the brilliancy of the flash, would, I am inclined to think, more than compensate for the shortness of the impression.

I must add a few words regarding the expense of these signals, which would be made up of the cost of erecting the scaffold of carpentry, the price of the lens, and the maintenance of the light. The price of the stage I shall pass over as a matter which may vary according to the circumstances of the situation and the taste of individuals; but the cost of the great annular lens does not exceed L.40; and if a smaller-sized lens, which I think would be found quite sufficient for the purpose, were employed, the expense would not be more than L.10. The annual maintenance would consist of little more than the supply of a gas or an oil burner. The consideration of the expense, therefore, of maintaining such a system of signals at the necessary intervals on railways, is not for a moment to be set against the most remote risk of the least of all the numerous accidents, the records of which fill the public prints.

Edinburgh, 29*th January* 1841.

On the Forms assumed by Uncrystallized Mineral Substances, such as the Occellated Stones of Dendera in Upper Egypt, the Stones of Imatra, &c. By M. Ehrenberg.*

The ancients have often spoken of the sports or anomalies of nature *(lusus naturæ)* as so many attempts to produce all kinds of forms in earthy and stony substances. In consequence, they have not separated true petrifactions and organized fossil remains from inorganic formations, and have considered the whole as the incomplete attempts at development made by a formative earth acting as a matrix. Our notions are now altered, yet naturalists still speak of the sports of nature both in regard to inorganic and organic matters; and the collectors of curiosities continue to store up in their cabinets inorganic forms of this nature as remarkable objects, and deserving of being viewed with interest. It may be admitted that they are often deserving of this attention; but at the same time they are often nothing more than stony formations, to which friction, fracture, splitting, or some other internal mechanical action, has given a singular aspect which can be of no interest to science; or rather it may be said that the caprice or imagination of the collector has ascribed to them relations and analogies of form, just as frequently happens with figures representing clouds, in which every one, according to his fancy, thinks he can discern a multitude of resemblances to known objects.

Among inorganic forms of the highest scientific interest, which have excited attention from the most ancient date, and more than ever in recent times, are those resulting from crystallization, which are grouped with mathematical regularity, perfectly determinate in their angles, and produced by an active mode of formation. Besides this, it has been ascertained, that this remarkable exterior appearance depended on the laws of their internal formation, which enable us to explain generically and positively the most varied changes of form, and to recognise the original type of structure even in an amorphous fragment.

Besides crystalline forms, a series of others exist, always reproduced in the same shape, some of which have not been made the subject of observation, while others have been but imperfectly studied. They are, however, possessed of great scientific interest, and are frequently met with in nature. I am about to speak, says M. Ehrenberg, of the occellated stones or stones *à lunette* of Egypt, which are regular, often a foot in size, and which, along with Dr Hemprich, I discovered, in 1821, in incalculable numbers in the desert of Dendera, Upper Egypt,† where they

* This memoir was read to the Royal Academy of Berlin, 29th June 1840.

† About eight years ago we received from the Rev. Vere Monro some remarkable annular-discoid morpholites brought by him from Egypt.—Edit.

still occupy their natural situation in various degrees of development, and where I collected numerous examples now submitted to the inspection of the Academy. Ever since that time these bodies have afforded me a subject of study, and I have endeavoured to ascertain the laws of their structure in two different ways; first, by an analytical method, attempting to determine their mechanical structure by means of delicate microscopic examinations; second, by a generical method, endeavouring to produce similar forms artificially, a task which in neither of these cases had been previously undertaken. These two methods, although the trials I have made long continued fruitless, are still attended with great difficulties, and are but in their infancy, have not failed to present results which appear to me worthy of being submitted to the Academy, and, obscure though they still are, to merit not the less on that account a favourable reception.

Formerly, in 1836, when I communicated the results of my analytical observations by the microscope on the regular fundamental forms constituting earthy and friable (*tendres*) mineral substances, I spoke, although with reserve, of a phenomenon presented by certain minerals, of a regular disposition shewn in certain very minute fundamental corpuscles to form themselves into articulated rods and rings, which sometimes reminded the observer of the serial linear disposition of the free molecules of bodies under the influence of magnetism, as in *kalkguhr* and *meerschaum;* and at other times indicating a force acting with more or less energy in a circle or spirally, as in kaolin and chalk. Microscopical researches prosecuted since on these singular relations, have afforded more extensive results, and the regular corpuscles of chalk, which I had at first named granulated folia (*gekœrnte blœttchen*) will henceforth, in accordance with my recent works on the *formation of chalk by microscopic organisms,* assume the particular and characteristic name of *crystalloids.*

New and successful microscopic observations on the figured stones of Egypt, have enabled me to ascertain that these configurations, resembling the corpuscles of kaolin and chalk, but in gigantic proportions, have in all probability been produced by the same agents as the corpuscles of the latter, only with grosser materials. In fact, we immediately recognise in these Egyptian formations, whether they be in rings, discs, or spheres, and varying in size from an inch to a foot, the animals of the chalk (for example, *Textillaria globulosa*) whose undissolved calcareous coverings have been subjected, in the progress of their formation, to a force which has disposed them in annular series. These are appearances altogether different from those presented by the flints and jaspers of Egypt, in which we meet with imbedded *polythalames* only here and there. The latter are not the corpuscles themselves, but only their form silicified by a chemical operation, the nature of which is unknown to us. The small visible calcareous coverings obtained by acids in the soluble layers of the figured Egyptian stones, shew distinctly that the operation to which they owe their present condition did not consist of a composition and a substitution; that it was not of a chemical nature; that it did not take place slowly and in a continuous manner, but that it was the calm and tranquil result of a mechanical deposition. It may easily happen that we may accidentally notice, in a chemical operation, certain identical parts which have undergone no change notwithstanding the alteration of the principal mass, just as we find unaltered flour in the dough of bread, chalk in silex, or foreign substances imbedded in crystals; but heterogeneous portions, arranged in series of regular formation, present a character peculiar to the configurations in question, in which are formed, in a manner altogether remarkable and different from what is usually observed, and under the influence of a particular force, lapideous rings, free, concentric, and unconnected with each other; between which alternate layers of lime with the animalcules of the chalk are deposited, giving rise to forms having a solid nucleus with a free but solid ring, suggesting to the fancy the figure of Saturn and his ring.

From these researches it appears how important it is to examine with more attention the inorganic forms which have long been known by the name of clayey, marly, or calcareous kidneys, as well as those called Imatra stones, which have often a well-determined shape, and have hitherto been considered, in the ingenious and exact classifications of crystallography, as amorphous masses, and placed in books on mineralogy among stones along with soft minerals of the same species, or in works on geology according to their particular position, and their formation ascribed to the general agency of attraction. It appears that an attempt has likewise been made, but with little success, to ascribe to these phenomena the formation of stalactites as well as that of oolites; but Mr Sedgwick has sufficiently shewn, that the deposits of calcareous kidneys, so common in the neighbourhood of Sunderland, were altogether foreign to the formations of stalactites. Properly speaking, they are only formations resembling oligistic iron, stalactitic or mamellated.

Viewing the matter in another light, some highly respectable mineralogists have thought proper to form the crystalloids, into a group, which they place beside crystals, and in which they wish to arrange in systematic order all minerals of a capillary, bacillary, dendritic, or spicular form, or such as compose oviform, reniform, or tubular stalactites, in which the spherical shape would be ascribed to a kind of attraction, while the stalactitic would be confounded with that of adhesion and crystallization. Other savans, on the contrary, have carefully separated crystals, and have regarded all the other forms as modifications of amorphous structure. A few months ago, the Academy of St Petersburg heard a remarkable note read to them from a distinguished observer (M. Parrot) who after having examined a rich collection of Imatra stones, concluded that they must be regarded as an extinct family of molluscs devoid of shells, and of the most simple organization, to which he desired to assign the name *Imatra*.

I shall now proceed, continues M. Ehrenberg, to the description of the objects which have formed the basis of my observations.

The first or principal basis of these observations was the stony Egyptian formation mentioned above, belonging to the calcareous deposits of Upper Egypt, which presents itself in abundance in the form of spheres with a single eye, or a pair, connected as in spectacles; this formation occurs in a small horizontal deposit of marl in the midst of the calcareous rocks of Dendera. These formations are sometimes regular spheres about a foot in diameter, but the greater part do not exceed three or four inches, collected in considerable numbers, and resembling piles of common balls, sometimes with the discs more or less flattened and regular, and the nucleus globular like an eyeball, surrounded with bourrelets or concentric rings; sometimes also the discs are double, and united like the eyes of a pair of spectacles. The intermediate forms and transitions from one to another are innumerable, but we find no other kind of them.

I have noticed a similar structure on many occasions; among others, in the silex of the chalk of Rugen; in a stone greatly resembling that of Imatra, coming from a sandstone-formation, like the figure eight lying on its side ∞, which has been deposited in the Royal Cabinet of Mineralogy, and was obtained from the Muschelkalk of Oberstrehlitz; as well as in a black sphere, seven inches in diameter, procured from the coal formation of Ruhrthal, and which may be seen in the cabinet of the Royal Administration of Mines.

The recent and detailed description of the Finland stones of Imatra, at the waterfall of the same name, has afforded, in connection with the previous observations of Hoffmann, a pretty accurate idea of their forms, which I have had no opportunity of studying personally. I examined last year a very interesting and instructive collection of these regular forms, belonging to Dr Willander of Tunaberg in Sweden, who, when coming to Berlin, brought his collection with him, when he became aware that I was occupied with the subject, and with much liberality placed it at my disposal. The marl of Tunaberg is certainly a stony formation completely free from crystallization, but at the same time the most regular that has hitherto been met with. According to Wallerius and Linnaeus, such formations are named in Sweden *malrekor* or *nækbræd*. They are distinguished, as all other similar objects were by Linnaeus, by the name of *tophus ludus* and *marga porosa*. Finally, the editor of the works of this naturalist, in 1779 and 1783, Gmelin, has classed them systematically among the most heterogeneous kinds of bodies. The forms of Tunaberg do not appear to have been known above two years. They are met with near Fada-Muhle in a deposit of fine blue clay, and they always present, in a very accurate and striking manner, the character of animal structure, suggesting the idea of petrified molluscs.

Dr Willander presented me with 47 specimens chosen probably from many thousands; M. Thamnau of Berlin also, who brought upwards of

100 specimens from Sweden, permitted me to select from his collection the forms which appeared to be of most scientific interest. It is this rich and fine collection of forms, with all the circumstances of development, and coming from one and the same deposit, that I have now the honour to present to the Academy for inspection.

But I have found a much richer source of materials in the microscopic examination of the primitive phenomena presented by the structure of inorganic forms; and these are the materials which have become so important as to form the basis of all my labours. For several years past, the observations of this nature made by me have been published and disseminated, and I have already, in 1836, published in Poggendorff's Annals the most remarkable results of my researches on the formation of crystals.

The results of my observations on these forms, and the analytical researches I have made, shew at once that it is convenient to establish in all the phenomena of inorganic form, after excluding crystals, several groups entirely distinct from each other.

One of the groups of these structures, named *amorphous* or *irregular*, embraces all the dendritic, capillary, stalactitic, and radiated forms, without nucleus and similar to hæmatite, and those with a radiated-oolitic structure, having a foreign nucleus, and which may be regarded as true crystals confounded together, bearing the same relation to simple and distinct crystalline forms that the massive and composite polypi present relatively to the simple polypi. In these two cases the individual forms have no connection with the shape of the united mass, and reciprocally. These structures, sometimes easy, and at other times very difficult to analyze by the aid of the microscope, are agglomerations of small crystals more or less conformable, and following certain laws, in some cases capable of being recognised, and which, in relation to the variety or the regularity, make an approach to those of the structure of plants founded on the development of the bud. All these forms are not crystalloids, but *masses of crystals*, or *well-determined crystals heaped together generically*, the forms of which, compressed and crushed, are called druses when the crystals can be easily observed, and, when the structures are less dense and more delicate, produce dendritic figures in the shape of a shrub, moss, tree, &c.

The Egyptian morpholites, the Imatra stones of Finland, and those of the malrekor of Sweden, are entirely different in their structure from those just mentioned. These bodies present neither any radiation from the centre, nor any appreciable crystalline development in their parallel planes of formation. They exhibit in their structure, on the contrary, and in a very obvious manner, a solid circle which is several times repeated, an evidently active development in their formation, founded on uniform laws, and frequently, perhaps always, as is seen in the Tunaberg specimens, parting from many axes of formation. No trace of organic structure, whatever may be the exterior aspect, is ever remarked in any of these surprising morpholites of Sweden, no more, in fact, than in those of Egypt observed in such great numbers; but in the former the dominating and modifying influence of many axes of formation can often be discerned with ease. Generally we perceive two directions of development in the laws of their structure, one concentric, sometimes horizontal and in one sense only, which constitutes rays and discs, sometimes radiating on all sides, which produces spheres; the other is linear, and emanates from the centre. For the most part these two axes exert a nearly equal power, or else one of them is superior to the other. It thence follows that the greater number of these morpholites, in consequence of the predominating influence of the linear direction of development, appear egg-shaped or fusiform; or, owing to the concentric direction prevailing, or the two forces being equal, assume the shape of discs or spheres, or are in an indeterminate condition. Among the 100 morpholites of Tunaberg, however, which I had an opportunity of examining, there were only one or two which had not a well-marked form. When we witness, as happens in rare cases, one of the axes of formation predominate, there result oblong forms with discoidal or globular envelopes, or having dilatations in the middle, or else the shapes are globular with one or two tongue-shaped elongations opposite each other. We rarely notice three of these elongations, and four have not yet been met with. There is still one form deserving attention, and that is the decided development of a new centre of formation at the two extremities of the linear axis of formation, the direction of which always cuts the first at right angles, producing appendages in the shape of a head or beak at the ends of the axis. These appendages do not seem formed by chance, but display constant forms, most of them resembling a bird with the head, neck, tail, and wings folded together, or a tortoise, and in some cases, when the new form acquires large size, they are like a hammer.* In this remarkable mode of formation there are often found foreign bodies, small stones, fragments of granite, &c., which, as in the chalk of Egypt, are imbedded in the mass. Often also there are accidentally found in the clay-strata, rolled pebbles and the fragments of primitive rocks, which have adhered to the morpholites as they were beginning to form; this we may notice in the present collection on a piece of slaty hornblende, where large grains are perceived, and to which two small morpholites are firmly attached.

In the stones of Imatra, the observer of Petersburg, formerly mentioned, has noticed as many as five successive or developed forms, one on the other. In those of Tunaberg, I never perceived more than two and a third beginning to form; but in the collection of M. Krantz of Berlin, there is a specimen from the mountain limestone near Dublin, on which are to be seen five bodies of this species linearly developed, one on the other, and many others of the same kind adhering to its sides. In

* The well-known "Fairy stones" of Scotland, which are washed out by so many of our rivers from the beds of clay connected with the red sandstone formations, are examples of the forms here described.—Edit.

this case, the fineness of the matter, the elegance and regularity of the form, would appear, in equal circumstances, to be increased in a remarkable manner.

Besides observations on the form and a microscopic analysis of these bodies, I have made experiments with precipitates and the residuum of a great variety of substances, and have taken particular care to examine the forms of the calcareous precipitates under the microscope. The following are the principal results of my inquiries.

I could not succeed, either on a former occasion or at the present, in imitating the true corpuscles of the chalk, but only produced something bearing some resemblance to them. These precipitates of carbonate of lime presented forms of very great variety.

The microscope enabled me, in general, to perceive solid concretions, under three principal forms.

First, In the form of an indeterminate, homogeneous, vitreous mass. This structure appeared like an irregular and rapid aggregation of very minute particles of matter, pretty equal among themselves.

Secondly, As corpuscles regularly conformable, which derived their shape from very minute particles of matter, round in appearance, approaching each other in different degrees of development, and seemingly subjected to an internal central force of attraction or formation. This is the most common appearance presented by the precipitates or combinations of the most varied description. I have found these corpuscles to be in every respect analogous to the large morpholites spoken of above. In a flake or nebulosity, at first very slight, we see simple spheres formed of fine grains, double spheres, kidneys, double kidneys, articulated rods, granular rings, and lastly bodies which run into each other, and resemble a brambleberry. The four first are simple forms, the rest have always presented a higher degree of development, not appearing as simple structures but highly composite. These various series of forms are what I name *Morpholites* or *Crystalloids.* They result from an internal force which disposes the material particles in a certain order without producing any change upon them.

Thirdly, In forms having the character of a structure with parallel faces, such as that which distinguishes crystals. This mode of structure, however, appears from observation to be rarely primary, but rather secondary. The first example occurs when the regulating force has interfered, often also when it has ceased to act. Occasionally this structure proceeds with incredible activity; sometimes it advances slowly. The intervention of the force of crystallisation causes the grains to disappear. It is a chemical process of transformation. Never have I seen a crystal forming itself of visible material corpuscles, but almost always by a wonderful and sudden transformation of small morpholites or crystalloids into either simple or composite crystals, according as the former were either simple or in groups. These sudden transformations have formerly been noticed by other observers, but under different relations, and among others, in the chloro-arseniate of lead, which, from being at first in grains of saffron-yellow, is all at once transformed into dendritic crystals of deep-red, and which Professor Erdmann has recently prepared with the chloro-arseniate of potass and the acetate of lead. This salt shews us the finest example of these singular metamorphoses. It must therefore be considered as demonstrated, that crystals neither exist nor can exist without a previous mechanical crystalloidal deposition of particles.

It is not our purpose here to inquire, whether all these phenomena are owing to the general force of attraction, or, whether, as Mr Faraday has alleged with some reason, electricity be not the most general principle which regulates chemical and magnetological actions, and even many of those of vegetable and animal life; but when we can here recognise one of the modifications of the general force which regulates the formation of bodies in crystallization, we may well likewise admit a crystalloidal or morpholitic force. My design, however, has principally been to draw attention to these singular phenomena and their relations, and to shew, that at times crystalloidal structures must exercise a considerable influence in the formation of masses of friable rocks which have not reached the point of crystallization.

I have likewise been led to the discovery of a solid centre of composition in the structure of isolated rings. After many researches, I succeeded in discovering this in sulphur. When oil is poured on flowers of sulphur, we often see crystals of sulphur separate themselves around the granules, while the granules disappear. In other instances, masses of dendritic or linear crystals are formed, which afterwards constitute isolated crystals. In many other circumstances besides, we see a cloudy concentric halo, broad and interrupted, appear round each granule, which afterwards produces a crystal. The appearance *en lunette* is produced when two granules approach each other with equal force. Perhaps one might succeed, by observing the relations which similar crystallizations present when they proceed slowly, in discovering other interesting phenomena; but it was not till after many trials, that I managed to observe in a state of activity, the rapid and elegant crystallization of salts, and to watch the formation of a siliceous sphere going on under my eyes. Does the difficulty consist in the extreme minuteness, or the transparency of the elementary parts, or rather in the absence of these properties, or, lastly, in the rapidity with which the operation takes place?

Speculations as to the Primary Source of the Carbon and Nitrogen present in Plants and Animals. By Charles Daubeny, M.D., F.R.S., &c. &c.*

Dr Daubeny says: *I cannot refrain from calling attention to the fact, that carbonic acid and ammonia, which have been shewn to be the sources respectively of the carbon and the nitrogen of plants, on the one hand exist in*

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* The interesting speculations of Dr Daubeny here given are extracted from the Doctor's valuable Lectures on Agriculture just published. Long before the

such variable proportions in the atmosphere, that they may appear, from this circumstance alone, to be extraneous to its composition ; and, on the other, are both seen to proceed, even at the present day, from the interior of the earth, in various parts of the globe.

Every one who has made the phenomena of volcanos the subject of his attention, is conversant with the fact, that volumes of carbonic acid are constantly issuing from the earth, not only in places where igneous action manifests itself, but also wherever we have reason to believe that it has formerly existed, though apparently in a dormant condition at present.

Those, too, who have visited any active volcano, must needs have remarked, that sal ammoniac is sublimed in large quantities from the substance of lava currents recently ejected, and likewise from the crevices of mountains in which the eruptions have taken place.*

Professor Liebig has also pointed out, that ammonia is a constant accompaniment of the boracic acid disengaged by volcanic action from the lagoons in Tuscany, and it is satisfactory to me to find that distinguished chemist confirming, by his authority, the view which I had long ago held and maintained, namely, that the ammonia in these cases cannot by possibility be derived from the decomposition of organic matter, since no beings could live at such a temperature as that indicated by the steam which holds it in solution.

Hence, like the carbonic acid, ammonia must have been derived from certain processes taking place in the interior of the globe,† processes which may have preceded the existence of living beings, and which may have fulfilled some important office in the economy of nature, subservient to their existence and production. Startling, indeed, as the position may appear, yet, being backed by the authority of Liebig, I shall no longer scruple to suggest as a matter at least of probable conjecture, that every particle of carbon, as well as of nitrogen, which enters into the constitution of the plants and animals, either now existing, or which have existed since the beginning of time, may have been originally evolved from the interior of the globe.

The only mode that suggests itself by which we can escape from this conclusion, consists in supposing, 1*st*, that when it first pleased the Almighty to call plants into existence, He at once overspread the globe with them, without availing himself of the operation of secondary causes to bring about their dissemination ; 2*dly*, that the amount of carbon and of nitrogen contained in the plants created, sufficed for supplying the entire animal kingdom, in proportion as it extended itself, with the organic matter which it would require ; 3*dly*, that no increase to the col-

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appearance of Professor Liebig's work, we maintained, on geological grounds, in our lectures, a similar speculation.—Edit.

* See my Memoir on the Eruption of Vesuvius in 1834, published in the Philosophical Transactions for 1835.

† Liebig, p. 112.

lective amount of the animal and of the vegetable matter in existence has taken place, from the first period at which they were called into being, up to the present time. Unless all these three postulates be assumed, it seems difficult not to attribute to gases evolved from inorganic sources, the first origin of those organic matters of which both plants and animals consist.

Now, it will perhaps to some of my hearers appear futile, and even almost presumptuous, to go back, as it were, to the dawn of creation, and to speculate on events that may have occurred at the first commencement of organic life. Nevertheless, I may be permitted to observe thus much, namely, that analogy seems to favour the supposition of each species of plant having been originally formed in one particular locality,* from whence it spread itself gradually over a certain area, rather than that the earth was at once, by the fiat of the Almighty, covered with vegetation in the manner we at present behold it.

The human race, as we are informed by the highest authority, is descended from a single pair, and the distribution of plants and animals over a certain definite area, would seem to imply that the same was the general law. Analogy would also lead us to suspect, that the extension of species over the earth originally took place on the same plan on which it is conducted at present, when a new island starts up in the midst of the ocean, produced either by a coral reef or by volcanic elevation. In these cases, we do not find the whole surface at once overspread with plants, but we can trace the gradual progress of vegetation from the chance introduction of a single seed, perhaps, of each species, floated to it by currents, or wafted by winds. Nor, indeed, does it seem probable, that a preternatural cause should have operated in covering the earth with plants, when natural ones are supposed to have been instrumental in the formation of the strata upon which they grew. Since these latter have been built up gradually through the successive accumulation of mineral and organic deposits, in which act Omnipotence did not directly interpose, we should be disposed to refer the full development of the former to a slow dissemination of individuals in conformity with natural laws, and to ascribe to the immediate hand of the Deity only the first introduction of a species.

Such an exertion of creative power as is implied by suddenly calling into existence all the plants of a particular period, seems the more improbable when we recollect that it must have been repeated at several successive epochs, since we cannot suppose that the whole globe would

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* Mr Lyell proposes the following hypothesis, as being reconcilable with known facts, viz. "That each species may have had its origin in a single pair or individual, where an individual was sufficient, and that species may have been created in succession, at such times and in such places as to enable them to multiply and endure for an appointed period, and occupy an appointed space on the globe." Principles of Geology, vol. ii. chap. 8.

have been in an equally forward condition, so as to be ready at the same moment for the reception of a redundant vegetation. If, however, we were to grant that the whole of the primary vegetation of the globe had started into being at once, as soon as the conditions of the earth's surface admitted, we should be bound to suppose the character of the plants analogous rather to those existing in countries not yet brought under the dominion of man, than to those of cultivated districts.

Now, it is to be remarked, that the plants most useful to the higher classes of animals, inasmuch as they afford the greatest abundance of nourishment, are comparatively rare in a wild condition, and when they occur, are generally deficient in those principles on which their value as articles of food depends.

The forests which, under such circumstances, cover so large a portion of the surface, contain but a small amount of nitrogen, for woody fibre, which constitutes the greater part of their bulk, is wholly destitute of that principle. The cruciferæ, the different species of cereal grasses, and other plants rich in nitrogen, require for their full development animal manure, and therefore, if they had come into being at all at this early period, must have been stunted in their growth and limited in their distribution.

Thence, it may be fairly doubted whether the whole amount of the plants which existed at one time on the surface of the globe, would have furnished azote enough for the animals of any given period.

Thirdly, it seems reasonable to suppose that civilization has, upon the whole, increased the aggregate of animal life as well as that of such vegetables as contain the largest quantity of nutritious matter. I am aware how difficult it would be to establish this last position in a manner perfectly unexceptionable, because the progress of human society is attended by a corresponding diminution in the amount of those animals and of those plants which are not dependent on man, and it seems impossible to strike a correct balance between the effects attributable to these two counteracting causes. Nevertheless, the following considerations may perhaps be allowed to favour the opinion which I have been advocating.

There can be little doubt that, within the area embraced by culture, the amount of nutritious matter goes on increasing with the care bestowed upon the land, and that the number of animals maintained is in consequence proportionably increased. This will be the case on a well-managed farm, even where (being situate at a distance from a town), it consumes no more manure than is obtained on the premises. At the end of a century the live stock may be increased, the crops may be more abundant than they were at the beginning, and yet every year a large quantity of carbon and nitrogen will have been carried off in the shape of the corn and cattle sold. Whence does this excess of carbon and of nitrogen then proceed? If from the decomposition of animals and vegetables extraneous to the farm, other portions of the globe must suffer in proportion to what is gained by those in cultivation, and hence the vegetation, as well as the amount of animal life, in regions which continue in a state of nature, will decrease in proportion to the increase of those brought under subjection to man.

But we have not the slightest reason to imagine such to be the case, nor is there any ground for believing that plants in a wild condition are unable to compete with cultivated ones, in the power of supplying themselves with those principles which are necessary for their existence; although it be true that man, by selecting for culture such as afford him the greatest amount of nutriment, causes more nitrogen to be abstracted from the air than would otherwise happen.

But what appears to me the most decisive objection yet remains to be stated. Once grant, with Liebig, that the nitrogen which plants possess can only be obtained by them through the decomposition of ammonia, and it will follow, that, unless this gas be supplied from the interior of the globe, the quantity of organic matter into which this principle enters as a component part will be undergoing a continual diminution.

For we know of no natural processes taking place on the surface of the globe which generate ammonia, excepting those connected with animal and vegetable decomposition; whilst there are many—such as the combustion of various organic substances—which, by resolving bodies containing nitrogen into their constituent elements, would have diminished the aggregate amount of them which might have formerly existed.

Some compensating process, therefore, is clearly required; and that, if I mistake not, is the disengagement of ammoniacal gas from the interior of the globe. Possibly, however, it may be suggested as another alternative, that the quantity of these two gases which would be required for the subsistence of the whole vegetable and the whole animal kingdom, when first called into being, and likewise all that which might be necessary to supply the loss of ammonia occasioned by combustion, &c. through all succeeding ages, might have been ready prepared in the atmosphere prior to their creation.

But independently of the difficulty of conceiving, in the case of ammonia, by what means the particles of hydrogen and of nitrogen could have been brought to combine on the surface of the globe without having been previously deprived of their elastic condition, those who propound this hypothesis ought to be prepared to shew, that an atmosphere charged with the gases in question to the extent which is assumed, would not have been fatal to beings possessing an organization analogous to that of the existing races.

To confine ourselves to ammonia, Drs Turner and Christison have shewn that less than $\frac{1}{100}$ part of this gas, introduced into air, caused, in ten hours, a shrivelling and drooping of the leaves of a plant, and its subsequent death. It may be doubted, therefore, whether even $\frac{1}{1000}$ part would not be too powerful a dose for the continuance of the healthy functions of the vegetable world, if permanently present, considering that the juices of the plant, and the moisture of the earth in which it grows, would be continually drawing from the atmosphere a constituent so soluble in water, and thus presenting it in a state of much greater concentration.

What the amount of nitrogen existing in all the plants and animals, either living or preserved from decay, throughout the globe, may be, it would be extremely difficult to determine; but admitting Liebig's principles, it will follow, that the amount of ammonia actually existing in the air will represent the average quantity which, at each moment of time, is disengaged from the organic matter of all kinds undergoing decomposition.

The quantity of ammonia present in the atmosphere under existing circumstances will, therefore, bear the same ratio to that required for the maintenance of the whole animal and vegetable creation taken together, as the amount of organic matter, at any given time, undergoing decomposition, does to that in a state of life, or of preservation from decay. Now, it may be collected, I think, from Liebig's statements, that a pound of rain water sometimes contains as much as $\frac{1}{4}$th of a grain of ammonia, or about $\frac{1}{30000}$th part. Could we tell, therefore, the proportion which the quantity of organic matter undergoing decay bears to that in a living or sound condition, we might obtain the means of estimating whether the whole amount of nitrogen existing throughout the globe, if it were at once diffused through the atmosphere, would not communicate to it deleterious qualities.

I have said that the *onus probandi* ought to rest with those who propound the last-mentioned hypothesis; because, undoubtedly, a presumption would seem to exist in favour of the view for which I have myself contended, from our having direct evidence that the evolution of these gases from the interior of the globe is proceeding continually. Hence, it seems natural to attribute to a phenomenon at once so constant and so general, some end in the economy of nature, and to suppose it to have been going on, like the volcanic processes which produce it, without interruption, from the beginning of time.

Granting, then, what upon Liebig's principles seems most consistent with analogy, namely, that the ammonia, no less than the carbonic acid which formed the food of the first plants, has been produced, not by processes of animal decay, but by such as were proceeding within the globe prior to the creation of living beings,—the notion of a slow and continuous disengagement of both compounds, from the earliest period to the present time, will be received, perhaps, as at least the most probable mode of account for their unfailing supply. Whilst it relieves us from the difficulty of supposing the atmosphere surcharged with these gases at any one period, it suggests to us, at the same time, sublime and interesting views of the arrangements of the Deity, in thus having made all things subservient to one common end, and having ordained that the mighty agents of destruction which exist in the bowels of the earth should minister, like the malignant genii of some eastern fable, to the wants and necessities of the living beings which He has placed upon its surface.

What may have been the nature of the processes which have brought about the disengagement of these gases, I have discussed at full in my work on Volcanos,* and in other subsequent publications. At present, I will only remark that, whilst the evolution of carbonic acid indicates perhaps nothing more than the operation of subterranean heat, the escape of atmospheric air, deprived of a portion of its oxygen, seems to imply that this heat originates in internal combustion, by which oxygen is absorbed; and the disengagement of ammonia may lead us to presume that this combustion is connected with the decomposition of water.

The latter inference appears to be unavoidable, when we recollect that ammonia is a compound of nitrogen and hydrogen, and that the only substance which can well be supposed to supply the latter element within the interior of the globe is water, which we also know to be abundantly present.

Water is readily decomposed by several of those bodies which, in an oxidized condition, constitute the various products of a volcanic eruption, and hence it seems not illogical to consider its action upon the bases of the earths and alkalies, which may still exist in the interior of the globe, as the *primum mobile* of such phenomena.

What, however, I am principally anxious on the present occasion to point out, is the discovery of a *final cause*, for the extrication of those gases, which I and others, in our examination of volcanos, have so frequently been led to remark; as this alone renders it probable that the phenomenon in question is one not of local or incidental occurrence, but that it has gone on from the beginning of time, on that gigantic scale which is consistent with the grandeur of those operations that have occasioned it, and is adequate to those equally extensive uses that we have ventured to assign to its agency.

Thus much, at least, seems clear, namely, that when we attribute,—as many without due reflection appear to do—the nitrogen which plants absorb to the products of the decomposition of animals, whilst these very animals are supposed to have obtained their nitrogen from the vegetable food on which they had themselves subsisted, we are reasoning in a vicious circle, and can hardly help being brought to the admission, that if plants are allowed to have multiplied and increased by any natural causes, they must at one time have obtained their food from inorganic matter exclusively; and moreover, that, even at present, no increase to the common stock of these elements, which is treasured up already in two organic kingdoms of nature, can take place, except it be furnished from the same identical source.

In conclusion, then, I may remark, that in balancing the rival pretensions of the two theories, by the aid of which it has been attempted to explain the origin of volcanos, we cannot be justified in leaving out of the account phenomena, which, like that of the disengagement of ammonia, are of such general occurrence, and appear subservient to such im-

* Description of Active and Extinct Volcanos. London, 1826.

portant ends; and that if the chemical theory be capable (as few, I believe, have attempted to deny*) of accounting for this as well as the other products which proceed from an eruption, it ought to be adopted provisionally, as explaining the internal heat of the globe, in preference at least to that one, which considers the latter merely as the remnant of the incandescent condition, which the earth is supposed once to have possessed, but which leaves out of the consideration altogether the chemical phenomena of a volcano, as unworthy of attention.

I ought to apologize, perhaps, for indulging in such speculations on an occasion like the present; but the subject of volcanos will not appear altogether irrelevant to that of agriculture, when I remind you that, from the products of subterranean fire, many parts of the surface of our globe seem to derive the fertility for which they are remarkable. All of you recollect the luxuriance attributed to the lands of Campania, the "Vicina Vesevo arva jugo," which in Pliny's time bore three crops in the year, being sown once with panic and twice with wheat, and yet, when allowed to rest betwixt crops, produced spontaneously roses more fragrant than those which resulted from cultivation in other places—"unde vulgo dictum," says Pliny, "plus apud Campanos unguenti, quam apud cæteros olei fieri."

Nor has this land, like much of that which is found in the newly settled parts of America, lost its fertility by continued cropping, but at the present day, as of old, stands distinguished even in that highly favoured region for the abundant returns which it yields to the husbandman. "Quantum autem universas terras campus Campanus antecedit, tantum ipsum pars ejus, quæ Laboreæ vocantur, quem Phlegræum Græci appellant."

"Yet the farms and villages," says Liebig, "are situated from eighteen to twenty-four miles asunder, and as there are no roads between them, there can be no transportation of manure." He therefore attributes the permanent fertility of the soil around Naples to the alkali present in the volcanic materials of which it is made up; but all felspathic rocks are charged with the same ingredient, and some of them surrender it as readily to the agents of decomposition; nor, as we have seen, is alkaline matter alone sufficient to supply plants with all the nourishment they require. May not, therefore, the slow disengagement of ammoniacal salts, as well as of carbonic acid from crevices in the mountain, likewise have their share in fertilizing the ground? These, by furnishing nitrogen

* It was only since this lecture was in the press, that I became acquainted with a memoir by Professor Bischof, of Bonn, in Jameson's Journal for January 1841, entitled. "Reasons against the Chemical Theory of Volcanos." A more extended discussion, however, on this subject, would be out of character with the design of the present lectures; and, as the Professor proposes to offer some further remarks in a future number of the same Journal, I shall abstain from all attempt to reply to his objections, until the whole of them have been put before the public.

and carbon, may in great measure supersede the want of animal manure; and if the rock happens also to contain a portion of the earthy phosphates, there is no reason why the celebrated Terra del Lavoro should not continue to produce an unintermitted succession of corn crops, repeated as often as the disintegration of the substratum permits, so long as the volcanic processes seated underneath it continue to send forth volumes of the gases alluded to. The superior quality of the wheat grown in this part of Campania may perhaps be thought to confirm such a conjecture. It would appear, that the flour of warm climates in general contains more gluten than that of colder ones,* perhaps because heat promotes the decomposition of organic matters, and consequently renders the supply of ammonia more abundant; and to this circumstance its superior fitness for the manufacture of macaroni is generally attributed. But the Neapolitan macaroni is distinguished for its excellence, and hence it is probable, that the wheat grown in the neighbourhood of that city, which supplies the raw material for this article of food, will be found to contain a larger per-centage of gluten than common, the power of forming which might be communicated to it by the ammoniacal salts in which its soil so abounds.

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* See Davy's Agricultural Chemistry, p. 141, and Mr Hyett's interesting communication in the Eng. Agric. Journ. No. 5, for January 1841.

Boussingault, Ann. Chimie, p. 65—301, says that—

Violet-bearded wheat, from Alsace, contains .	17.3 per cent.
The same, grown in the Jardin des Plantes, .	26.7 —
Winter wheat, grown in ditto, ...	33.3 —

Hermbstœdt has given the following results, Annalen der Landwerthschaft, vol. xxii. p. 1.

100 parts of wheat in soil manured with

	Gluten.	Starch.
Human urine (dried), . . .	35.1	39.1
Bullock's blood (dried), . . .	34.2	41.3
Human fæces (dried), . . .	33.1	41.4
Sheep's dung, ...	22.9	42.8
Pigeon's dung,†	12.2	63.2
Cow's dung, ...	12.0	62.3
Vegetable humus, . . .	9.6	55.9
Same soil not manured, . . .	9.2	66.7
Sir H. Davy found—		
100 pts. good full grained wheat, sown in autumn to afford	19	77
100 pts. of wheat sown in spring, . . .	24	70
100 pts. of Sicilian wheat, . . .	21	75
100 pts. of Barbary wheat, . . .	23	74
100 pts. of full and fair Norfolk barley, .	6	79
100 pts. of Suffolk rye, . . .	5	6

† It seems extraordinary that pigeon's dung should stand lower in the scale than that of sheep.

It may appear an homely comparison, but, to liken small things with great, the analogy is complete, between the arrangements of nature, in supplying from below the gaseous materials which the crop requires for its growth, and the plan often adopted by the gardener, of placing in a vessel, underneath the roots of a plant, a body of animal manure, which, by its exhalations, communicates to it the very same principles.

Should these views receive credence, and obtain confirmation from future investigations, they will afford a proof that geology, no less than the sister sciences of chemistry and vegetable physiology, is capable of throwing light upon questions in which the agriculturist is concerned—but of this truth it would indeed have been needless to search for proofs in countries so distant, or by an appeal to phenomena with which the English farmer is happily unacquainted; since we shall have abundant opportunities in the course of these lectures of shewing, that it will be brought home to him by facts of which he has daily experience, and which present themselves to his notice in the common business of life; for, if it be the office of the chemist to explain the causes of those differences which exist between soils, and the means of improving them by manures, it is no less that of the geologist to point out where those of the best description are to be met with, what resources we have at hand in the bowels of the earth underneath, or in the contiguous strata, for remedying their defect, and what facilities for drainage, and other methods of amelioration, the physical structure of the country may chance to supply.

On the Natural History and Anatomy of Thalassema and *Echiurus.* By Edward Forbes, Esq. and John Goodsir, Esq. Members of the Wernerian Natural History Society. With a Plate. (Pl. VII.) (Communicated by the Authors.)*

Among the Radiata of the British seas are two animals which, in their general appearance, rather resemble *Annelides* than *Echinodermata*, to which latter class they structurally belong. These are the *Thalassema Neptuni* and *Echiurus vulgaris*, members of the family *Thalassemaceæ* in the order *Sipunculidæ*, a zoological and anatomical description of which species we have to-day the pleasure of submitting to the Wernerian Society.

The family *Thalassemaceæ* includes a group of vermigrade *Echinodermata*, characterized by having cylindrical worm-

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like bodies terminated at one extremity by a mouth, which is placed at the end of a short proboscis, to which is appended a remarkable sheath-like appendage, and at the other by an anus with no external appendages.

These characters distinguish it from the other families of its order; from the *Sipunculaceæ*, which have a tentaculated trunk, no sheath-like appendage, and an anus placed at its base; and from the *Priapulaceæ* which have a trunk without tentacula, no oral appendage, and the anus at the posterior extremity at the end of a long filamentose caudal appendage, which has been regarded by some naturalists as a respiratory organ.

The genera *Thalassema*, *Echiurus*, *Bonellia*, and *Sternaspis*, constitute the family. The first has a simple oral appendage and no corneous bristles surrounding its anus; the second has also a simple oral appendage, but has circles of corneous bristles or setæ surrounding the posterior extremity; the third is distinguished by its forked oral appendage; and the fourth is marked out from its allies by the possession of a corneous disk, surrounded by setæ placed near its anterior extremity. But few species are included in these four genera. Of *Thalassema* but one is known. Of *Echiurus* two have been described, the one a native of our own seas, the other of the North Pacific. Of *Bonnellia* two species are recorded, both inhabitants of the Mediterranean, as is also the only known species of *Sternaspis.*

The *Thalassema Neptuni* is a native of the coast of Cornwall and Devon, where it lives among submarine rocks. Hence Lamarck, in the first sketch of his history of Invertebrate Animals, styled it *Thalassema rupium.* It was discovered by the observant Gaertner, and by him sent to Pallas under the name by which it is now known. Pallas, however, considered it an annelide, and an ally of the earth-worm, and named it *Lumbricus Thalassema*, under which name he describes and figures it in his Spicilegia Zoologica.* Montagu afterwards found it and described it under the name of *Thalassina mutatoria.*† At the same time he expressed his be-

* Fasc. x. t. 1. f. 6. † Linnæan Trans. vol. xi. p. 24. t. v. f. 2.

lief that his animal was identical with that described by Pallas, but supposed that the figure given by that illustrious naturalist was incorrect. This, however, is not the case, the figure of Pallas well representing the animal after preservation in spirits, in which state doubtless he had only an opportunity of seeing it.

Of late it has been taken by Mr Harvey at Teignmouth: from his specimen our structural account is drawn up. Of its external characters, it need only be mentioned, additional to the descriptions of Pallas and Montagu, that it possesses a short retractile trunk, as well as an oral appendage. Montagu's account of its habits, when alive, is all we know of them, but is most full and interesting, and may be found in the eleventh volume of the Linnæan Transactions.

The *Echiurus vulgaris* is a much larger and more remarkable animal. A great number of individuals of this species were thrown up on the sandy shore of St Andrews during last winter after a severe gale of wind. The largest specimens measured about six inches long and half an inch in diameter. The body of the creature is cylindrical, annulated with little flat tubercles, which were floccose toward the two extremities. From the anterior end projected a proboscis about half an inch in length, not furnished with tentacula, and having a deep red margin at the extremity. This proboscis is retractile, but not so a singular furrowed fleshy appendage placed along side of it. This appendage is highly extensile, and forms a sort of sheath to the proboscis. A little way from its junction with the body are two shining yellow cartilaginous bristles; short, lanceolate, curved, acuminate, and retractile. These are the genital hooks. From between them runs a red line down the body towards the anus, marking the course of an internal vessel. The whole of the body is of a bright pink colour, with obscure paler narrow rings and speckles caused by the minute tubercles of the skin being of a paler hue. The anus is placed at the posterior extremity on a somewhat flattened disk, which is surrounded by two circles of corneous setæ, similar in structure to the genital hooks, but shorter. They are ten in number in each circle. The anus is round and red. The sheath of the proboscis differs in colour from the rest of the animal, being of a bright scarlet. It is so slightly affixed to the body as to break off on the least touch, and in only one or two cases did we find it attached, and then it broke away immediately on the removal of the animal.

On keeping the *Echiurus* alive in a vessel of sea-water, it was continually changing its form, swelling itself out in various parts so as to assume very strange and eccentric shapes. If a fresh supply of salt water was poured into the vessel, it would on a sudden become very vivacious, starting up towards the surface, and swimming with spiral contortions in the manner of an *annelide*. Then it would sink to the bottom of the vessel, and swell itself out with water.

The *Echiurus*, like the *Thalassema*, was first figured and described by Pallas, who obtained it from the coast of Belgium. He gave a most accurate general representation of it, but strangely omitted the true proboscis; and by all writers since his time the sheath has been described as a proboscis not only in this case, but in the descriptions of most of the other *Thalassemaceæ*.

Montagu first perceived the true relation of the *Thalassema*, and remarks in his paper that it should immediately precede *Holothuria*. This view of its position was also held by Cuvier, and more lately by Brandt. Lamarck, however, placed the *Thalassema* and *Echiurus* in his first division of *annelides*, characterized by having no feet, and including the families *Hirudines* and *Echiureæ*. In the latter, associated with the earth-worm and cirratulus, we find these animals before us. Many zoologists since his time have looked upon them as worms, but the structural details which follow will shew that their relation to the annelides is one of analogy and not of affinity, and that their true position is among the *Echinodermata* in the order of *Vermigrada* or *Sipunculidæ*.*

Echiurus—Digestive System.

The digestive tube commences by a mouth of a rounded form, very small in the state of contraction, funnel-shaped when dilated. The oral orifice is continuous with a canal

* See Forbes's British Echinodermata.

which is sacculated and contracted at intervals, particularly in its posterior half. This first portion of the intestinal apparatus, which may be denominated the pharynx, is arranged in two coils so as to resemble somewhat the figure 8. These coils are compressed and kept in position by the muscles of the oral hooks, and by the bloodvessels, which assume a complicated arrangement in this part of the animal. The tube then contracts into a highly muscular but very narrow œsophagus. This rather suddenly dilates into the remaining part of the canal, which is nearly uniform in diameter, thin and delicate in texture; arranged in a somewhat spiral direction till near the posterior part of the animal, returning upon itself in the same manner for two-thirds of the length of the body, and then proceeding to the cloaca as a straight and rather narrow tube. The cloaca is smaller than the same organ in the *Holothuriadæ*. From mouth to anus the canal measures from three to four feet. The pharynx is two inches long, the œsophagus four, and the remainder is so uniform in diameter as to render it impossible to distinguish any division into stomach, intestines, &c., and so fragile as to render measurement very difficult. The pharynx exhibits distinct circular muscular fibres, and in the œsophagus they are so strong and so arranged in bundles as to give it the appearance of a windpipe. The tube is not connected to the parietes of the body by a mesentery, but by numerous delicate muscular threads irregularly arranged and intermingled with minute bloodvessels. Near the middle of the body the folds of intestines are filled with a yellow bilious mass, but we could detect no trace of a liver, or of glandular structure in the coats of the gut.

The respiratory sacs open into the cloaca on each side of the rectum. These sacs do not ramify, and are about one-third of the length of the animal, and exhibit in the living individual lively motions—contracting, dilating, elongating and twisting. They are of a vivid red colour from the number of vessels distributed to them, and have a mottled appearance from numerous microscopic organs attached to their external surface. When a small portion of the respiratory organ is cut from the living animal and placed under the microscope in a little seawater, the dots observed with the naked eye on its outer or peritoneal surface, exhibit the appearance of a number of funnels, with their necks attached, and their cup-like extremities standing erect. Each of these funnels has its outer surface, rim, and inner surface or cavity, covered with cilia which exhibit lively motions. The inner or mucous surface of the respiratory sac has a number of rounded somewhat lobulated elevations on it, each corresponding to one of the funnels on the outer surface. These elevations are covered with cilia, but on the membrane between them none could be seen. The ciliated funnels could be withdrawn into the pouches formed by the ciliated elevations of the internal surface; but we could not obtain ocular demonstration of what we suspect to be the case—that the cavities of the funnels open into the common respiratory cavities, and that the ciliated elevations of the inner surface disappear when the funnels in the outer surface are extended; and, *vice versa*, a current being in this way established between the respiratory cavities and the common cavity of the body of the animal, which is full of sea-water. The muscular fibres of the two respiratory sacs exhibit a peculiar arrangement. Both the transverse and longitudinal fibres have an undulating course so as to surround the necks of each of the funnel-shaped organs as the fibres of the human gravid uterus surround the uterine sinuses. If the currents of sea-water flow through the funnels, the contraction of the muscular fibres can stop that flow, and enable the animal to fill the respiratory sacs by the following process. By contracting the anterior part of its body, and pushing the contained sea-water back, the animal distends its posterior portion into a bulbous shape, in consequence of which the enclosed portions of the anal spines become widely separated; and from their connection with the cloaca, that cavity is dilated, and, acting like a syringe, sucks in more water. The animal then closes the anus, and contracts the cloaca by pushing the water in its body forwards. This simultaneous action forces the water contained in the cloaca into the respiratory sacs, along which it is conveyed by their powerful vermicular or peristaltic action. A slight relaxation of the muscular fibres of the sacs, and the erection of the ciliated funnels, will allow the water to pass into the cavity of the body, while the action of the cilia will remove it through the same channels in a contrary direction.

The vascular system consists of two longitudinal vessels, one running along the ventral surface of the body, the other along the unattached surface of the intestine. The intestinal trunk is always full of blood in the weak or dead animal, the ventral trunk always empty or collapsed. From this circumstance, from the general arrangement of the vascular system, and from the position of the respiratory organs, we are inclined to think, although we have not been able to verify the opinion by actual observation, that the former vessel is the venous, the latter the arterial trunk. The vein commences by numerous radicles on the œsophageal portion of the digestive tube, runs along the edges of the gut, collecting branches as it proceeds. On the rectum, the trunk disappears by being divided into innumerable branches, which are apparently arterial, and proceed to the respiratory sacs, which, as before stated, are highly vascular. The arterial or ventral vessel is apparently formed by radicles from the respiratory sacs (branchial veins). Its walls are thin, and are perceived with difficulty on the surface of the nervous cord. In its course it supplies vessels to the intestines ; and when it arrives at the convolutions of the pharynx, it sends off from its right side a large trunk, which, proceeding to the right oral hook, surrounds it and its muscles by dividing and again closing. It then proceeds to the commencement of the œsophagus, and joins a vessel to be described immediately. The ventral vessel, after giving off this great trunk, proceeds to the oral extremity of the pharynx, round which it forms a vascular circle. The latter sends branches back upon the pharynx; and a branch forwards, which forms a second circle or vascular zone round the lip, on the surface of the nervous ring, and a large trunk which, running to the middle of the pharynx, dilates into a sacculated sinus, which probably owes its peculiar appearance to the transverse contractions into which this portion of the tube is generally thrown. This sinus runs along the second portion of the pharynx, and at the commencement of the œsophagus receives the trunk formerly described as proceeding from the ventral vessel. It then terminates by ramifying on the œsophagus, and supplying this portion of the tube with aërated blood. The use of the large trunk which comes off from the ventral vessel, is evidently to supply the mouth, trunk, and anterior part of the digestive tube, with arterial blood, when the animal has projected the anterior part of the body, and when the constriction of the snout, and the pressure of the sand in which it is boring, would prevent the free circulation of the blood in the two vascular circles, at times when the supply is absolutely necessary from increased muscular action.

The nervous system is very simple, being merely a ring surrounding the anterior part of the pharynx when it becomes continuous with the skin. From this ring a nervous cord runs along the under side of the animal to the extremity of the body, terminating abruptly by sending off a few branches. Along its course the cord gives off numerous lateral twigs, which are unsymmetrical, and continue free for a short distance from their origin, and then disappear in the muscular parietes of the body. When the animal is contracted, the cord is arranged in close undulations, and exhibits no ganglionic enlargements. It consists of a moderately long sheath, in which the nervous matter is contained in a very soft condition.

The reproductive system consists of four sacs which open on the ventral surface by minute orifices, two immediately behind the genital hooks, the other two about an inch farther back, and both pairs about one-third of an inch from the median line. When the *Echiurus* is not in season, they are about one inch and a half long, one-fourth of an inch in diameter, highly transparent, so as to be almost invisible, and possessed of the power of twisting in all directions. When the male is in season, they become greatly enlarged, four inches long, half an inch in diameter, with one or two contracted portions. The contained fluid is milk-white, and rather consistent; and when examined under a high power, it is seen to swarm with exceedingly active globular *Spermatozoa*, which exhibit rapid whirling and dancing motions. The male organs, when in this condition, are remarkably beautiful objects, being covered with large thread-like and transparent scarlet bloodvessels, which are relieved by the dead cream-white of the organs themselves. We have not seen the female sacs fully distended; when moderately so, the eggs appear to be arranged as in the roe of osseous fishes, and are about the size of millet-seed. Examined under the microscope, the egg appears as a highly transparent globule, enclosing towards its centre a number of smaller globules or cells.

The structure of *Thalassema Neptuni* is in all respects identical with *Echiurus*, the only difference, and that an unimportant one, being the less complicated arrangement of the intestinal tube.

The oral and anal hooks and spines of *Echiurus*, and the oral hooks of the *Thalassema*, are protruded and withdrawn exactly as the setæ and hooklets among the *Annelides*.

From the anatomical description we have now given, it is evident that the genera *Echiurus* and *Thalassema* must be arranged in the class *Echinodermata*. The body filled with sea-water—the respiratory apparatus—the digestive system—and the intestinal venous trunk are the leading anatomical peculiarities, and are characteristic of the Echinodermatous animals. The colour and circulation of the blood, the want of an aquiferous system, the ventral nervous cord, and the muscular system, shew the relation of these animals to the *Annelides*, and prove that the transition from a vermiform radiate animal to a true articulate animal, is effected by the symmetrical atrophy and hypertrophy of certain of the radiate elements in each ring.

EXPLANATION OF PLATE VII.

Fig. 1. *Echiurus vulgaris*, of the natural size, as seen from the ventral aspect.

Fig. 2. *Echiurus vulgaris* laid open from the back, the integuments stretched, and the greater part of the intestinal tube removed; *a* the pharyngeal portion of the intestinal tube; *b* the œsophageal portion; *c c* the portion corresponding to the stomach and intestine, which is of great length in this animal. The venous trunk is seen coursing along its free edge, and branches of the ventral vessel (artery) ramify on its attached or mesenteric edge; *d* rectum; *e e* respiratory sacs, covered with ciliated processes; *ffff* generative sacs; *gg* ventral vessel or arterial trunk; *h* the arterial branch which, surrounding the right genital hook, proceeds to the pharyngeal portion of the intestine, for the purpose of supplying it with blood when the animal is boring; *i i i* the pharyngeal artery, and the two oral arterial circles; *k k* the nervous

cord giving off unsymmetrical nerves, and terminating in a ring which surrounds the pharynx; *ll* the two genital hooks with their muscles, one of which is common to both, and approximates their upper extremities. Fig. 3. The nervous circle, part of the cord, and the extremity of the latter magnified. Fig. 4. A section of a part of the muscular parietes of the body, and of the sheath of one of the genital hooks, to shew that the latter is a tegumentary organ developed in a follicle, and provided with a muscular apparatus as in similar organs among the *Annelida*. Fig. 5. A portion of one of the respiratory sacs, magnified to shew the ciliated funnel-shaped processes in the state of erection. Fig. 6. A portion of one of the respiratory sacs laid open, to shew the ciliated projections on its internal surface. Each of these corresponds to one of the funnel-shaped processes on the external surface. Figs. 7, 8, 9. Diagrams to illustrate the mode in which the sea-water may be supposed to pass through the ciliated organs of the respiratory sacs, into and out of the abdominal cavity. Fig. 7. The organ erected, and the passage free. Fig. 8. The organ half retracted. Fig. 9. The organ wholly retracted, the muscular fibres contracted over it, the passage closed, and the ciliated projection formed on the internal surface of the sac. Fig. 10. Male organs in season. Fig. 11. *Spermatozoa*. Fig. 12. Female organs in season. Fig. 13. Eggs magnified, shewing the cells in their interior.

On the Employment of the Safety-Lamp in the Coal-Mines of Germany. By Professor Gustav Bischof, of Bonn. (Communicated by the Author.)*

Three years ago the Prussian government charged me with the examination of the means of preventing the dreadful accidents in coal-mines, occasioned by explosions of fire-damp. For this reason I have visited many coal-mines in the neighbourhood of *Saarbrucken*, *Wellesweiler*, *Aix-la-Chapelle*, and in the Principality of *Schaumberg*. I have examined into all the circumstances concerning the exhalations of fire-damp, and collected this inflammable gas issuing from blowers in three coal-mines, for the purpose of analyzing it.† Supported

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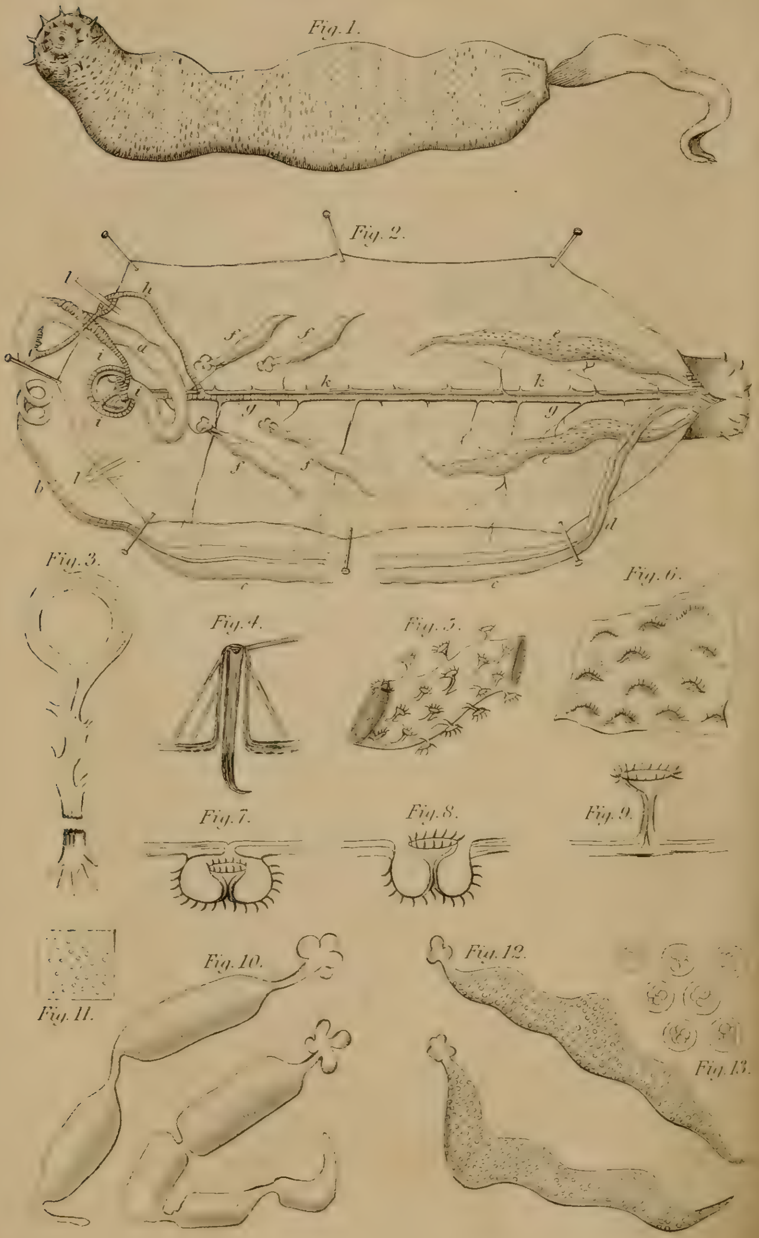
† Our readers are referred to Professor Bischof's Physical and Chemical Examination of Fire-Damp, published in this Journal, Vol. xxix. p. 309, and last Number, p. 127.—Edit.

Echiurus Vulgaris.



by the authorities in the fullest manner, having received information from many officers of the coal-mines relative to the subject, and having had access to all public papers regarding the use of the safety-lamp, in Prussian coal-mines, during the last twenty-one years, I was enabled to become acquainted with this subject, and the experiments made in these mines about it.

Notwithstanding that the safety-lamp has been used in the Prussian coal-mines during this space of time, even in the most dangerous localities and under the most trying circumstances, yet not a single accident has occurred fatal to its security. But I have frequently heard complaints as to its small illuminating power. I had therefore a great many safety-lamps made, whose cages of wire-gauze had apertures of different sizes.

Experiments regarding the security of safety-lamps are rather difficult. By placing the lamps in a locality filled with an explosive mixture, sufficient results are obtained, provided the place be very large, or if new quantities of the explosive mixture are constantly passing through it. On the contrary, when the quantity of the explosive mixture is too much limited, the intenseness of the burning of the explosive mixture within the cage gradually decreases, and the wire-gauze prevents the passage of the flame through it, from not having reached the red heat. Indeed, a lamp may only be considered as conferring safety, when no communication of explosion takes place when the wire-gauze is perfectly red hot.

Considering these circumstances, it is obvious, that it is only by placing a lamp near the spot where a quantity of inflammable gas is issuing and mixing with the circulating current of atmospheric air to the explosive point, or by conveying a lamp into a very large space filled with an explosive mixture, that its safety can be proved.

In order to institute experiments according to the former method, I caused to be made a cylinder of pasteboard 24 inches in height and 15 inches in diameter. The cylinder was furnished with four small glass windows for observing the phenomena taking place in its interior. The cylinder was closed by a cover and a bottom. The former had five holes, which might be closed up by cork-stoppers. The bottom had three holes, one of which was for drawing up the safety-lamp, the second for admitting the inflammable gas, the third for the penetration of the atmospheric air into the cylinder.

This apparatus was fastened near the blowers, and the inflammable gas introduced through a tube stuck into the fissure from which the gas was issuing. The safety-lamp suspended to a wire going through the cork-stopper in the middle hole, was drawn up into the cylinder. By opening or closing the other holes, the circulation of the explosive mixture formed in the interior of the cylinder, was regulated in such a manner as always to surround the safety-lamp with new quantities of it.

There were different layers of gas in the cylinder; at the top for the most part the light inflammable gas, at the bottom chiefly the atmospheric air, but in the middle there was an explosive mixture of the strongest exploding power. It was therefore evident that the safety-lamp near the bottom scarcely ascertained the actual presence of the fire-damp, whilst at the top it was soon extinguished. But in the middle of the cylinder the phenomena depending on the presence of the explosive mixture took place in the most powerful degree. The flame of the lamp increased not only to the top, but it was there bent down, and the whole cage filled with fire.

In this part of the cylinder the safety-lamp remained from fifteen to twenty minutes, provided that no inflammation through the wire-gauze took place. The latter was for the most part red-hot. The lamp was for some time put in pendulum motion, which was as violent as possible, in order to imitate the passing of the workmen in the mines where inflammable gas prevails. On another occasion the lamp was held obliquely, in order that the increased flame might reach the wire-gauze.

I wished to examine the safety-lamps, not only relatively to the greatness of the apertures of their wire-gauzes, but also to that of their diameters. I had six kinds of wire-gauze of brass made, and five cylinders were made of different diameters of these six sorts. Therefore thirty cylinders were obtained, the number of the apertures and the greatness of the diameters of which are the following:

Number of apertures to the Square Inch.	Diameters in Lines.				
	18.5'''	21.5'''	26.'''	28.'''	37.'''
380	No. 1	No. 2	No. 3	No. 4	No. 5
308	... 6	... 7	... 8	... 9	... 10
184	... 11	... 12	... 13	... 14	... 15
162	... 16	... 17	... 18	... 19	... 20
104½	... 21	... 22	... 23	... 24	... 25
58*	... 26	... 27	... 28	... 29	... 30

* The apertures were not quite strictly squares.

I employed, as is evident, wire-gauzes of much larger apertures than are usual in coal-mines, for I had in view to find the maximum at which they are not permeable to flame.

For these thirty cylinders of wire-gauze five lamps were made, the size of which accorded with that of the cylinders. The forms of these lamps were in general like those constructed originally by Sir Humphrey Davy himself, but the cages made of platinum wire were wanting.

I shall not dwell on the description of the individual experiments. It is sufficient to ascertain the cylinders which, under all circumstances, have prevented the communication of explosion. Prior to these experiments, I had experimented as to whether an explosion in the apparatus would be dangerous to myself and my assistants, but this was not the case. After each experiment, all holes of the apparatus were opened in order to remove the impure air, and it was only after a new accumulation of inflammable gas, that a fresh experiment was commenced.

There is still a circumstance for consideration. As the inflammable gas, penetrating through the wire-gauze, is partly burning, the more of it will burn the greater the diameter of the cylinder. Now, the circulation of the inflammable gas taking place during the different experiments in the same proportion, on applying the larger cylinder, a greater quantity of gas must be consumed than on applying the less ones. This was the reason that, on using the cylinders of twenty-six lines in diameter, the phenomena alluded to, viz., the enlargement of the flame, and the red heat of the wire-gauze, were soon decreased. The lamp must therefore sometimes be removed from the apparatus for collecting the explosive mixture before continuing the experiments. On that account, the safety of the above-mentioned cylinder, and still more that of the two following, could not exactly be so proved as that of the two smaller cylinders. Indeed, the experiment made on applying the cylinder No. 26, had shewn that the transmission of flame through the wire-gauze, is not produced till the lamp has remained for a long time in the explosive mixture, as the saving power of the wire-gauze decreases as its temperature increases.

The results of my numerous experiments are,

First, Cylinders of 104 *apertures to the square inch, are perfectly safe when placed in the apparatus filled with the strongest explosive mixture, formed by the blower in Gerhard's mine, but cylinders of* 58 *apertures are no longer safe.*

Secondly, The diameter of the cylinders of 104½ *apertures may increase to two inches without a decrease of their security being observable.*

The experiments I have made near the blower in the gallery of Wellesweiler, gave in general the same results; but it was distinctly evident, that the fire-damp produced by this blower afforded a stronger explosive mixture than that of the former one, which accords with the chemical analysis. Thus the lamp No. 26 was scarcely two minutes in the apparatus ere an explosion happened, whilst, when filled with the fire-damp of the gallery of Gerhard's mine, an explosion took place only after twenty minutes. Also the enlargement of the flame and the heat of the wire-gauze were greater than in the fire-damp of Gerhard's mine. There is no doubt the effects produced by the blower of Wellesweiler would have been yet more intense, if the quantity of fire-damp of this blower had been as considerable as that of the blower of Gerhard's gallery.

In order to establish whether a safety-lamp, burning for a long time in explosive mixtures, is heated so much as to effect an explosion on the outside of the lamp, I caused an officer of mines to make such an experiment. He allowed a new safety-lamp of 784 apertures to the square inch to burn in the apparatus placed near the blower of Gerhard's gallery during eight hours. After two hours the wire-gauze was covered with soot, and after three hours the meshes were greatly closed up. The flame was extinguished without an explosion ensuing. After this, the wire-gauze was cleaned and the lamp placed again in the apparatus; the same phenomena were now observed in a still shorter time.

I shall not quote farther experiments instituted in places 40 inches in height and in diameter, in a coal-stratum where exhalations of inflammable gas were to be supposed to exist, but I shall give an account of experiments made on such a large scale as to afford results of the most exact kind.

I caused an excavation of 14 feet in height, 5 feet in length, and 3_5 feet in breadth, to be made in a coal-stratum in the vicinity of the blower of Gerhard's gallery. This excavation, called in Germany, "Uebersich brechen," being above the gallery, experimenters standing in the latter, where there was good ventilation, were enabled to make their experiments without danger. The excavation was soon charged with fire-damp.

The lamps from No. 1 to No. 13 were in succession drawn up into the excavation, and remained in it from five to ten minutes, during which time they were strongly agitated. Though the effects produced by the explosive mixtures were very vehement, for the wire-gauzes became quite red-hot, the whole cylinders were filled with fire, the oil began to boil, and a humming noise like that of the chemical harmonica was heard, yet the wire-gauzes remained quite impermeable to flame. Some of these lamps, drawn up too high into the excavation, were extinguished. The lamp No. 3 fell down, as the pack-thread by which it was suspended was burned, but an explosion did not take place. No. 5 caused, it is true, an explosion, but it was found that the wire-gauze, from its having remained for eight minutes in the explosive mixture, had suffered oxidation, and had fallen to pieces.

On the contrary, the lamp No. 14 instantly effected an explosion, before the wire-gauze became red-hot. It was therefore not necessary to employ the lamps after No. 14, as they would doubtless have given the same results.

While in the apparatus, the cylinders having 104 meshes to the square inch, were found perfectly safe, a cylinder of 184 meshes, and 28 lines in diameter, effected an explosion; the reason of this unequal result, no doubt, depending on the circumstance that the apparatus contains 2½ cubical feet of explosive mixture, but the excavation 233 cub. feet. From this is to be seen the importance of making experiments in such large spaces, in order to prove the security of the safety-lamps.

Similar experiments were also instituted in an excavation made in the coal-strata of Wellesweiler. The phenomena were in general the same, but the results rather different, though strictly in accordance with the results of the chemical analysis. The lamp No. 8 was quite safe, but No. 9 and No. 10 produced an explosion. No 13 likewise produced instantly an explosion; therefore these three lamps, which were quite safe in the former excavation, produced in the latter an explosion.

So large as these excavations were, it is nevertheless to be remarked, that the experiments give sufficient results only when the disengagement of the fire-damp from between beds of coal takes place in the same proportion as is consumed by burning the safety-lamps. On the contrary, the vehemence of the effects gradually decreases, and no safety-lamp can produce an explosion, as it would have done provided the explosive mixture had been there in its strongest form.

The officers of mines, who made the latter experiments enumerated, have had a favourable opportunity of inquiring into the circumstances taking place when the fire-damp is in violent motion.

A gallery 35 feet in length, 5 feet in height, and 4 feet in breadth, rising at an angle of 20 degrees, was filled with about 300 cubical feet of explosive mixture. Against this gallery another one was carried. Through the intervening space between these two galleries a hole of 1 inch in diameter and 40 inches in length was bored and closed up by a stopper. On approaching a safety-lamp of 900 apertures to the square inch to the hole, and on opening the stopper, the presence of fire-damp was ascertained by the flame being increased nearly by one inch. But the streaming of the air was so vehement, as to push the flame by half an inch aside, and at last to extinguish it. Other safety-lamps of 308 and 184 meshes to the square inch, gave the same results. Lastly, a common mining lamp was placed before the hole. The flame of the lamp was pushed 1½ inch aside, and was blue coloured, but did not kindle the fire-damp which was streaming out. On removing the lamp, the blue flame disappeared, without being extinguished.

From these observations it follows, that the inflammation of the gas issuing from the hole, and kindled by a lamp, is not continued backwards; and this is owing to the strong streaming of the gas. Hence it appears to follow, that if holes are bored against a rising gallery filled with an explosive mixture, an explosion is not to be feared with a common lamp, and still less with a safety-lamp.

As the fire-damp of the artesian well in the principality of Schaumburg contains 16 per cent. olefiant gas, and only 5 per cent. nitrogen (see last No., p. 146), it was to be expected that many of the lamps, which were quite safe in the explosive mixtures of the coal mines, would no longer be safe in this case.

The results of my experiments made with this fire-damp were, that all the thirty wire-gauzes, without exception, produced explosions. But a safety-lamp of 620 apertures to the square inch was, under all circumstances, quite safe.

The experiments enumerated shew, that in many coal-mines wire-gauzes of larger apertures than now in use may be employed. Such a contrivance would be useful to the workmen, as complaints are frequently made of the obscure light afforded by the safety-lamp. Indeed, I have had myself opportunities of observing this during my frequent visits to mines filled with fire-damp. The size of the apertures in the wire-gauzes employed in any coal-mine may be easily ascertained by experiments in excavations made in the mines. But even in coal-mines charged with the most dangerous explosive mixtures, wire-gauzes of 620 apertures to the square inch, appear to restrain the flame within its narrow bounds, and to prevent explosions.

Finally, I have to remark, that the Royal Academy of Sciences at Brussels announced last year a prize-question,— "Rechercher et discuter les moyens de soustraire les travaux d'exploitation des mines d'houille aux charges d'explosion." I undertook this question, and was so fortunate as to see my memoir crowned by the Academy. The greater part of my experiments were enumerated in that memoir, but another portion of them made during this year is now for the first time published. As the experiments in the Prussian coal-mines are still going on, I shall have occasion for farther communications on a subject so important for the interests of humanity.

Remarks on the Present State of our Knowledge relative to Shooting-Stars, and on the Determination of Differences of Longitude from Observations of those Meteors. By Mr Galloway.

At the meeting of the Astronomical Society of the 8th January 1841, Mr Galloway read a communication on this subject.

Earlier Opinions.—After adverting to some of the earlier opinions which have been entertained on the nature of fire-balls, shooting-stars, and other igneous meteors, the author remarks, that no very definite theory was formed respecting them till towards the end of the last century; for although the cosmical origin of the more remarkable bolides and fire-balls had been suspected, the shooting-stars were generally regarded as atmospherical phenomena, which were ascribed by some to electricity, and by others to the inflammation of hydrogen gas accumulated in the higher regions of the atmosphere.

Chladni's Views.—In 1794, Chladni published his celebrated work, in which he gave a catalogue of all the recorded observations of fire-balls; and, from a comparison of the different descriptions, inferred that these meteors have not their origin in our atmosphere, but are cosmical masses moving through the planetary spaces with velocities equal to those of the planets, which, when they encounter the earth's atmosphere, are inflamed by the resistance and friction, and become luminous, sometimes bursting into pieces, and scattering masses of stone and iron on the ground. This opinion was at first greatly ridiculed; but the repeated and even not unfrequent fall of meteoric stones, and the discovery by Howard that all of them present an almost perfect similarity of constitution, widely different from that of any substance found on the earth, at length forced conviction even on the most sceptical. From the close resemblance between fire-balls and shooting-stars, and, indeed, the impossibility in many cases of distinguishing the one class of meteors from the other, Chladni was led also to ascribe a cosmical origin to the latter phenomena. At this period, however, there were no observations from which precise or certain conclusions could be formed respecting the altitudes, velocities, or paths, described by the shooting-stars—the elements by which the question of their existence within or beyond the atmosphere could be solved.

Observations by Brandes and Benzenberg.—In the year 1798, the first series of observations for determining these points, was undertaken in Germany by Brandes and Benzenberg. Having selected a base-line of about nine English miles in length, and stationed themselves at its extremities, they began to observe on nights previously agreed on; and when a meteor was seen, they immediately traced its apparent path on a celestial map, noting carefully the exact times of its appearance and extinction, with any other circumstances likely to assist in identifying it. The meteors observed simultaneously at both stations were in this manner recognised with considerable certainty; and the comparison of their paths on the two maps afforded data for the determination of their parallaxes and altitudes. The results were as follows:—Between the 11th of September and the 4th of November 1798, only twenty-two corresponding observations were obtained, from which the altitudes could be computed. The altitude of the lowest was about 6 English miles; there were seven under 45 miles; nine between 45 and 90 miles; six above 90 miles; and one had an altitude of about 140 miles. There were only two observations from which the velocity could be deduced; the first gave 25 miles, and the second from 17 to 21 miles in a second. The most remarkable result was, that at least one of the meteors moved upwards, or away from the earth. By these observations, the perfect similarity between fire-balls and shooting-stars, in respect of velocity and altitude, was completely established. Another attempt, on a more extensive scale, to determine the altitudes and velocities of shooting-stars, by means of simultaneous observations, was made by Brandes in 1823, assisted by a number of associates resident in Breslaw and the neighbouring towns. The observations were continued from April to October, and during this interval about 1800 shooting-stars were observed at the different places, out of which number ninety-eight were found which had been observed simultaneously at more than one station. The altitudes of four of these were computed to be under 15 English miles; of fifteen between 15 and 30 miles; of twenty-two between 30 and 45 miles; of thirty-five between 45 and 70 miles; of thirteen between 70 and 90 miles; and of eleven above 90 miles. Two of these last had an altitude of about 140 miles; one of 220 miles; one of 280 miles; and there was one whose height was computed to exceed 460 miles. Thirty-six orbits were obtained; in twenty-six of which the motion was downwards, in one horizontal, and in the remaining nine more or less upwards. In three cases only the observations were so complete as to furnish data for determining the velocity; the results were respectively 23, 28, and 37 English miles in a second, the last being nearly double the velocity of the earth in its orbit. The trajectories were frequently not straight lines, but incurvated, sometimes horizontally, and sometimes vertically, and sometimes they were of a serpentine form. The predominating direction of the motion was from north-east to south-west, contrary to the motion of the earth in its orbit,—a circumstance which has been generally remarked, and which is important in respect of the physical theory of the meteors.

Quetelet's Observations.—A similar set of observations was made in Belgium in 1824, under the direction of M. Quetelet, the results of which are published in the *Annuaire de Bruxelles* for 1837. M. Quetelet was chiefly solicitous to determine the velocity of the meteors. He obtained six corresponding observations from which this element could be deduced, and the results varied from 10 to 25 English miles in a second. The mean of the six results gave a velocity of nearly 17 miles per second, a little less than that of the earth in its orbit.

Wartmann's Observations.—The last set of corresponding observations referred to in the paper was made in Switzerland, on the 10th of August 1838; a circumstantial account of which is given by M. Wartmann, in Quetelet's *Correspondence Mathématique* for July 1839. M. Wartmann, and five other observers, provided with celestial charts, stationed themselves at the Observatory of Geneva; and the corresponding observations were made by M. Renier and an assistant, at Planchettes, a village about sixty miles to the north-east of that city. In the space of seven and a half hours, the number of meteors observed by the six observers at Geneva was 381; and during five and a half hours, the number observed at Planchettes by two observers was 104. All the circumstances of the phenomena—the place of the apparition and disappearance of each meteor, the time it continued visible, its brightness relatively to the fixed stars, whether accompanied with a train, &c., were carefully noted. The trajectories were then projected on a large planisphere. The extent of the trajectories described by the meteors was very different, varying from 8° to 70° of angular space, and the velocities appeared also to differ considerably; but the average velocity concluded by M. Wartmann was 25° per second. It was found, from the comparison of the simultaneous observations, that the average height above the ground was about 550 miles; and hence the relative velocity was computed to be about 240 miles in a second. But as the greater number moved in a direction opposite to that of the earth in its orbit, the relative velocity must be diminished by the earth's velocity (about 19 miles in a second). This still leaves upwards of 220 miles per second for the absolute velocity of the meteor, which is more than eleven times the orbital velocity of the earth, seven and a half times that of the planet *Mercury*, and probably greater than that of the comets at their perihelia.

Deductions from the above observations.—From the above results, it is obvious that the heights and velocities of the shooting-stars are exceedingly various and uncertain; but if the observations are in any respect worthy of confidence they prove that many of these meteors (according to Wartmann's observations, by far the greater number) are, during the time of their visibility, far beyond the limits to which atmosphere is supposed to extend, and that their velocities greatly exceed that which is due to bodies moving at the same distance from the sun under the influence of solar gravitation. It is perhaps impossible to form any correct estimate of the absolute magnitudes of the meteors. Their apparent magnitudes differ greatly; the greater number resembling stars of the third or fourth magnitude, while many are equal to stars of the first, and some even surpass *Jupiter* and *Venus* in brilliancy. It is remarkable that the largest are those which have the greatest altitude, and only the smaller ones appear to come within 20 or even 40 miles of the earth.

Epochs of Recurrence of Falling Stars.—With respect to the casual observations of the phenomena, the accounts of which are very numerous, the most interesting conclusion which has been inferred from them is the periodical recurrence of shooting-stars in unusual numbers at certain epochs of the year. Of these epochs, the most remarkable is that of November, on account of the prodigious number of meteors which have been seen in some years at that time. The principal displays were 1799, 1832, 1833, and 1834. On the 11th of November 1799, thousands were observed within a few hours by Humboldt and Bonpland at Cumana; and on the same night by different persons over the whole continent of America, from the borders of Brazil to Labrador, and also in Greenland and Germany. On the 12th November 1832, they were seen over the whole of the north of Europe; and on the 12th of November 1833, the stupendous exhibition took place in North America, which has been so often described. From the accounts of this phenomenon collected by Professor Olmsted, M. Arago computed that the number of meteors on this night amounted to 240,000. In 1834, a similar phenomenon recurred on the night of the 13th of November, but on this occasion the meteors were of a smaller size. In 1835, 1836, and 1838, shooting-stars were observed on the night of the 13th November, in different parts of the world; but though diligently looked for on the same night in the last few years, do not appear to have been more numerous than on other nights about the same season,—a circumstance which has shaken the faith of many in their periodicity.

The second great meteoric epoch is the 10th of August, first pointed out by M. Quetelet; and although no displays similar to those of the November period have been witnessed on this night, there are more instances of the recurrence of the phenomena. In the last three years shooting-stars have been observed in great numbers, both on the 9th and 10th; but they appear in general to be unusually abundant during the two first weeks in August. The other periods which have been indicated are the 18th of October, the 23d or 24th of April, the 6th and 7th of December, from the 15th to the 30th of June, and the 2d of January; and it is not improbable that further observations will add to the number.

Theories regarding the Origin and Phenomena of Shooting-Stars.—The different theories which have been given to explain the origin and phenomena of the shooting-stars are next stated. The following are the principal:—

1. That the shooting-stars and fire-balls are substances projected from volcanos in the moon. It is known that a body projected vertically from the moon with a velocity of about 8500 feet in a second would not fall back upon the lunar surface, but would recede from it indefinitely; and in order to reach the earth the projectile would only require, under the most favourable circumstances, to have a velocity of about 8300 feet. Such a velocity, which is only about four or five times greater than that of a cannon-ball, is quite conceivable; but the extraordinary exhibitions of 1799 and 1833, to say nothing of their supposed periodicity, is utterly irreconcilable with the theory of a lunar origin. Benzenberg, however, adopts this theory, and supposes the shooting-stars to be small masses of stone, from one to five feet in diameter, which are projected from lunar volcanoes, and circulate about the earth or about the sun when their projectile velocity exceeds a certain limit.

2. Dr Olbers, and some other astronomers, have supposed the shooting-stars to be the *débris*, or fragments of a large planet, burst into pieces by some internal explosion, of which *Ceres*, *Pallas*, *Juno*, and *Vesta*, are the principal remaining portions. The smaller fragments continue to circulate about the sun in orbits of great eccentricity, and when they ap-

proach the region of space through which the earth is moving, they enter the atmosphere with great velocity, and by reason of the resistance and friction are rendered incandescent, and emit a vivid light so long as they remain within it.

3. It has been suggested by Biot that the extraordinary displays observed in November may be explained by supposing the meteors to have their origin in the zodiacal light. The extent of this lens-shaped nebulosity is not well ascertained; but as the plane of its principal section is not parallel to the ecliptic, if the earth passes through it at one season, it must be remote from it at another. But shooting-stars are observed at all times of the year; and the November meteors differ from those of other seasons in no respect excepting in their greater multitude.

4. The hypothesis first suggested by Chladni is that which appears to have met with most favour, having been adopted by Arago and other eminent astronomers of the present day to explain the November phenomena. It consists in supposing that, independently of the great planets, there exist in the planetary regions myriads of small bodies which circulate about the sun, generally in groups or zones, and that one of these zones intersects the ecliptic about the place through which the earth passes in November. The principal difficulties attending this theory are the following:—First, that bodies moving in groups in the circumstances supposed must necessarily move in the same direction, and consequently when they become visible from the earth, would all appear to emanate from one point and move towards the opposite. Now although the observations seem to shew that the predominating direction is from north-east to south-west, yet shooting-stars are observed on the same nights to emanate from all points of the heavens, and to move in all possible directions. Secondly, their average velocity (especially as determined by Wartmann) greatly exceeds that which any body circulating about the sun can have at the distance of the earth. Thirdly, from their appearance and the luminous train which they generally leave behind them, and which often remains visible for several seconds, sometimes for whole minutes, and also from their being situate within the earth's shadow, and at heights

far exceeding those at which the atmosphere can be supposed capable of supporting combustion, it is manifest that their light is not reflected from the sun; they must therefore be self-luminous, which is contrary to every analogy of the solar system. Fourthly, if masses of solid matter approach so near the earth as many of the shooting-stars do, some of them would inevitably be attracted to it; but of the thousands of shooting-stars which have been observed, there is no authenticated instance of any one having actually reached the earth. Fifthly, instead of the meteors being attracted to the earth, some of them are observed actually to rise upwards, and to describe orbits which are convex towards the earth; a circumstance of which, on the present hypothesis, it seems difficult to give any rational explanation.

The most recent hypothesis is that of Capocci of Naples, who regards the aurora borealis, shooting-stars, aerolites, and comets, as having all the same origin, and as resulting from the aggregation of cosmical atoms, brought into union by magnetic attraction. He supposes that in the planetary spaces there exist bands or zones of nebulous particles, more or less fine, and endued with magnetic forces, which the earth traverses in its annual revolution; that the smallest and most impalpable of these particles are occasionally precipitated on the magnetic poles of our globe, and form polar auroras; that the particles a degree larger, in which the force of gravitation begins to be manifested, are attracted by the earth and appear as shooting-stars; that the particles in a more advanced state of concretion give rise in like manner to the phenomena of fire-balls, aerolites, &c.; that the comets, which are known to have very small masses, are nothing else than the largest of the aerolites, or rather *uranolites*, which in course of time collect a sufficient quantity of matter to be visible from the earth. This theory of Capocci differs from Chladni's only by the introduction of magnetic forces among the particles, and it is obvious that all the objections to the former theory apply with equal force to this. It may be remarked, however, that some physical connexion between the phenomena of shooting-stars and the aurora had been already suspected, and the observations adduced by M. Quetelet afford reason to suppose that the latter phenomenon is also periodical.

From the difficulties attending every hypothesis which has hitherto been proposed, it may be inferred how very little real knowledge has yet been obtained respecting the nature of the shooting-stars. It is certain that they appear at great altitudes above the earth, and that they move with prodigious velocity; but everything else respecting them is involved in profound mystery. From the whole of the facts M. Wartmann thinks that the most rational conclusion we can adopt is, that the meteors probably owe their origin to the disengagement of electricity, or of some analogous matter, which takes place in the celestial regions on every occasion in which the conditions necessary for the production of the phenomena are renewed.

Attempts to deduce differences of Longitude from the Observation of Falling Stars.—The concluding part of the paper contains an account of the different attempts which have been made to deduce differences of longitude from the observation of shooting-stars. That meteors which appear and are extinguished so suddenly, and which, by reason of their great altitude and brilliancy, are visible over considerable portions of the earth's surface, would afford excellent natural signals, provided they could be identified with certainty, was an obvious thought; but so long as they were regarded merely as casual phenomena, it could scarcely be hoped that they would be of much use, in this respect, to practical astronomy. As soon, however, as their periodicity became probable, the observation of the phenomena acquired a new interest. In observing the meteors for this purpose, it is assumed that they appear instantaneously to observers stationed at a distance from each other, and that the meteors seen by different observers so placed are identically the same. These points are not altogether free from uncertainty; but the results of the trials that have been already made may be regarded as favourable, and as shewing that among the other methods of determining astronomical positions, the observation of shooting-stars is not to be disregarded. At the November meeting of this Society, in 1839, an account was given of Professor Schumacher's observations at Altona on the night of the 10th of August 1838. On the same night corresponding observations were made at several observatories in Germany; but those at Breslaw appear to have been the most successful. From twelve coincident observations at Altona and Breslaw, Professor Boguslawski computed the difference of longitude of the two places to be 28^m $22^s.07$, which differs less than a second from that which had been previously adopted. In Silliman's *American Journal* for October 1840, an account is given of simultaneous observations made on the 25th of November 1835, at Philadelphia, and at the College of New Jersey, at Princeton. Seven coincidences were observed, and the mean result gave a longitude differing only $1^s.2$ from the mean of other determinations; the whole difference being two minutes. This appears to have been the first actual determination of a difference of longitude by meteoric observations. In the corresponding observations of Wartmann and Reynier at Geneva and Planchettes, the differences of longitude deduced from three of the meteors, which were attended with peculiarities so remarkable as to leave no doubt of their identity, were respectively 2^m, 2^m 3^s, 2^m 5^s, whence it would seem that a single observation may be in error to the amount of several seconds of time. In the *Bibliothèque Universelle de Génève* for August 1840, there is given an account of the determination by this method of the difference of longitude between Rome and Naples. The corresponding observations were begun in November 1838, and were continued at intervals under the direction of Father Vico at Rome, and of Capocci and Nobili at Naples. The apparent paths of the meteors were traced on a celestial globe, and the times of appearance and extinction compared with clocks regulated by astronomical observations. The observed times of the extinction of the phenomena presented a very satisfactory agreement, inasmuch as it is stated that there was in general a difference of only a few tenths of a second of time between the partial results for a difference of longitude amounting to 7^m $5^s.7$. The merit of first suggesting the use of shooting-stars and fire-balls as signals for the determination of longitudes is claimed by Dr Olbers and the German astronomers for Benzenberg, who published a work on the subject in 1802. Mr Baily, however, has pointed out a paper published by Dr Maskelyne twenty years previously, in which that illustrious astronomer calls attention to the subject, and distinctly points out this application of the phenomena. The paper, which is printed on a single sheet, is entitled " A plan for observing the Meteors called Fire-balls, by Nevil Maskelyne, D.D., F.R.S. and Astronomer Royal," and is dated Greenwich, November 6. 1783. After recounting some observations, from which he infers that such meteors appear more frequently than is commonly imagined, and stating the particulars to be attended to in observing them, he adds :—" It would be well if those persons who happen to see a meteor would put down the time by their watch when it first appeared, or was at its greatest altitude, or burst, or disappeared, and again when they heard the sound : and as common watches are liable to vary much in a few hours, that they would, as soon after as may be, find the error of their watch, by a good regulator ; for, if *the exact time could be had at different places, the absolute velocity of the meteor, the velocity of the sound propagated to us from the higher regions of the atmosphere, and the longitudes of places, might be determined.*"

On the Living Representatives of the Microscopic Animals of the Chalk-Formation. By M. Ehrenberg.

1. *Infusoria of Mexico and Peru.*

M. Ehrenberg communicated to the Royal Academy of Sciences of Berlin, at its sitting on 2d July 1840, a note on the remarkable infusoria living in the seas of Mexico and Peru, which may aid the problematical explanation of the fossil forms of the chalk-formation.

Notwithstanding the rich materials which the author has already collected from different quarters of the globe of microscopic forms now alive, yet such of those out of Europe as constitute, properly speaking, genera, are still extremely rare. The proper generic forms he has had an opportunity of observing, whether in Africa or in Asia, have for the most part been since found by him in Europe in the identical species ; and it is very possible that those whose type has not been again met with may have continued in their present rank only in consequence of the series of observations being incomplete, and because they have not been sought after with sufficient care. The forms recently sent from Mexico by M. Carl Ehrenberg, hitherto exhibit only European genera and even European species. The forms of Peru were unknown when the great work of the author on the Infusoria was printed.

In 1837, M. Camille Montagne of Paris described, in the *Annales des Sciences Naturelles*, a century of new cryptogamous plants, and among these two microscopic forms accidentally found on the marine confervæ of Callao in Peru, one by M. d'Orbigny, the other by M. du Petit Thouars; he named the one *Achnanthes pachypus*, the other *Trochiscia moniliformis*. M. Montagne shewed the specimens of these two forms to M. Ehrenberg when in Paris in 1838, and the latter considered them to belong to the *Bacillariées*. He thought that the *Trochiscia* rather belonged to the genus *Meloseira* Agardh (*Gallionella*), and designated it by the name of *moniliformis*.

At all events, these two forms are different from those of Europe. The *Achnanthes pachypus* is a distinct species of this genus, very nearly related to *A. subsessilis*. The form of the Trochiscia or Meloseira is more remarkable. It possesses a character which raises it to the rank of a particular genus, and it is the first extra European genus which has been well determined. Its form is that of a Gallionella with a pedicel like the Achnanthes. It is to the Gallionella what Lynedra is to Navicula, or what Podosphenia, Gomphonema, or Echinella are to Meriodon, or rather Cocconema to Eunotia, Stentor and Trichodina to Vorticella, and Epistylis or finally Euglena to Colacium. In fact, it is necessary to unite all the genera with a pedicel to those deprived of this organ which approach them, if it be not thought that the characters of the form of Peru are sufficient to constitute a new genus. But if this distinction is made, this form, the first extra-European one which shall have been definitively characterized, will take the generic name of *Podosira moniliformis*.

A more attentive examination of the small branch of *Polysiphonia dendroidea* on the alga of Callao to which the *Podosira* was attached, has afforded facts of even greater interest in a scientific point of view. We have there found two hitherto unknown forms of Bacillariées. One of these has all the appearance of *Tabellaria vulgaris* (*Bacillaria vulgaris*), but it is divided in the interior by two partitions or curved folds according to the length of each distinct aciculus in their chambers or cavities. This structure of distinct aciculi throws some light over that of the same bodies in the chalk-marl of

Oran, which M. Ehrenberg had considered as but little distinct in a species of the genus Navicula, which was distinguished by a kind of internal folds, and which he has described in his preceding communications under the name of *N. Africana*, considering it at the same time as characteristic of the chalk-formation.

The examination of the living form has shewn that this Navicula, which seemed to deviate widely from the genera now existing, is related to one nearly allied to those now living, and in which the distinct acieuli, in consequence of an imperfect division, present zig-zag bands. The three chambers in the living animalcula are occupied by three greenish corneous cavities or discs which have been regarded as the ovaries in all the family. The middle of the body, as in *Tabellaria*, is occupied by a hollow transverse tube, the openings of which appear in every respect similar to the two medial openings of Navicula; but their functions do not appear to be the same, since these openings are not free, but rather fitted closely to the similar openings in the allied animal.

This imperfect distinction of the individuals, the development in the shape of a ribbon, as well as the necessarily different position of the alimentary openings in the forms of the marl when compared with the Naviculæ, completely separate them from these, in like manner as the subdivision of the interior chambers removes them from the Tabellariæ. They thus form a particular generic group, which is completely distinct from European forms. This second new genus has been named *Grammatophora*, and the species *G. oceanica*.

The apparent identity of the fossil *N. Africana* with the *Grammatophora oceanica* extends only to the genus and not to the species, since an alga found at Vera Cruz by M. Carl Ehrenberg, and which has quite recently come to hand, has furnished two other forms very nearly allied to this genus, which have been distinguished by the names *G. Mexicana* and *G. undulata*. The partitions in these forms often present the figure of a point of interrogation.

An example of *Coscino-discus excentricus* has likewise been found on a Mexican alga, and there, as at Cuxhaven, it lives in the sea, and its fossil coverings have also been discovered in the chalk-marl of Oran in Africa.

A seventh form, the fourth new one from Peru with which we are acquainted, is a *Cocconeis*, found adhering to the branchlets of the *Polysiphonia*. It has a strong resemblance to *C. undulata* of the Baltic Sea, but is specifically different. It may be named *C. oceanica*.

M. Ehrenberg also read a notice on the discovery of *polirschiefer* belonging to the black *dusodile* of Geistinger-Wald, and on its nature as an infusorial slate.

The author has shewn, partly before the Society of Naturalists at Berlin, and partly in the *Annalen der Physik und Chemie* of 1839, that the yellow mineral species, known by the name of *dusodile*, appears, on microscopic analysis, as a *polirschiefer*, composed of the shells or coverings of infusoria impregnated with bitumen, and that what is called foliated and papyraceous coal is nothing else than black dusodile. It was therefore probable that in deposits of dusodile, particularly towards the extremities, the polirschiefer would be found unaltered and not penetrated by the bitumen. The researches of the author, in reference to this point, have not hitherto been successful; but a letter lately sent to him by M. Steininger of Trèves, the author of valuable works on the volcanos of the Rhine, informs him that in a mine of Geistinger-Wald he found, under lignite, a quantity of papyraceous coal, polirschiefer, and a kind of adhesive slate, and that he had already published this fact in 1821 in his work entitled *Matériaux pour servir à l'Histoire des Volcans du Rhin*, page 43. The new researches of the author on infusorial slate have determined M. Steininger to examine his assertions afresh, and transmit specimens to Berlin. It follows from the examination of the latter, that there are polirschiefers adjoining the dusodile of Geistinger-Wald (papyraceous coal) which have produced the deposit of Infusoria, which, by the penetration of bitumen, has been transformed into papyraceous coal or black dusodile.

The principal forms of the siliceous coverings which compose the mass are, Gallionellæ of very different sizes, perhaps the different states of development of *G. varians*, among which are deposited five species of Naviculæ, among others *N. fulva*, and a very large pedicellated form, designated by the name of

N. carinata. We likewise distinguish a form very nearly related to *Fragillaria diophthalma*, and great numbers of another almost identical with *Gomphonema gracile.* Of the eight forms which have been recognised, four evidently belong to those of fresh water and the present era. They are not smooth on the surface, full in the interior, resembling stony microscopical nuclei, just as we often find in shells, but very rarely among the Infusoria.

The author has placed before the Society drawings of the American forms and those of Geistinger-Wald, as well as specimens of *polirschiefer* sent to him by M. Steininger.

Diagnosis of New American forms.

Podosira, new genus.

Character of the genus: animal belonging to the family of the Bacillarii possessing the characters of *Gallionella,* but supported by a pedicel.

P. moniliformis, Trochiscia moniliformis Montagne 1837, corpuscles globose, separate, very finely punctate; the ovaries vesicular, greenish. Du Petit Thouars found it at Callao on the Callithamnius floccosus and Polysiphonia dendroidea, and it was first detected by Montagne on specimens of these transmitted to him.

Grammatophora, new genus.

Character of the genus: animal belonging to the family of the Bacillarii, possessing the external characters of Tabellaria, but divided into three longitudinal compartments by two internal folds often curved in various directions.

G. oceanica; bacilli quadrate or oblong; the middle laterally turgid; gradually attenuated at both ends, and obtuse; the ovary trilobed on both sides and green. Gathered alive along with the former by Du Petit Thouars on the algæ of the Peruvian seas at Callao.

G. Mexicana, bacilli quadrate or oblong; the centre equal on the sides; both ends suddenly decreasing and obtuse. Found in a living state by Charles Ehrenberg on the algæ of the Mexican ocean at Vera Cruz.

G. undulata, bacilli quadrate or oblong; laterally moniliform, undulated, four times constricted, hence five-jointed; the apices obtuse. Found alive by C. Ehrenberg in the same place as the preceding.

The fossil species which is found in the cretaceous deposits at Oran, in Africa, and formerly named *Navicula Africana,* now *G. Africana,* differs in the following characters: bacilli oblong, scarcely turgid in the middle, and very obtuse at the apex. It is now very likely that *Navicula Bacillum* should be referred to the genus Tabularia.

Cocconeis oceanica, new species.

Shell elliptical, suborbicular, convex and very smooth on the back; marked exteriorly with simply curved concentric lines; not undulated nor transversely striated. Found at Callao by M. du Petit Thouars.

II. *Microscopic Animals of the North Sea.*

At the meeting of the Royal Academy of Sciences of Berlin of the 13th August 1840, M. Ehrenberg communicated the continuation of his researches on the numerous microscopic animals of the chalk-formation which are still found alive.

By the kind assistance of M. Berzelius, the author had received some fresh sea-mud from the coast of Sweden, which Bishop Eckstroem of Gothenburg had obtained for the purpose from the Island of Tjoern in the Cattegat. This mud proved to be extremely rich in new and interesting microscopic animals. There are no less than twelve living species which are of importance in a scientific point of view, and whose siliceous coverings were only known in a fossil state in the chalk-marls of Caltanisetta in Sicily, and of Oran in Africa; so that the number of species belonging to the chalk which are found in a living state, is about doubled. A very interesting fact is, the existence in a living state of the *Grammatophora* (formerly *Navicula*) *Africana*, which has hitherto been known only as a fossil of the marls of Oran; and also that of the *oceanica*, lately brought from Callao in Peru, and which had previously been only met with in the marls of Greece. M. Ehrenberg has likewise discovered in the waters of the Cattegat a prismatic siliceous infusory form, which would belong to the genus *Staurastrum*, if it could be placed in the subdivision of infusory animals having a soft *carapace*; and which, moreover, is distinguished by four large openings at the four angles. It is proposed to form it into a new genus, and to give it the name of *Amphitetras antediluviana*. There was likewise found among the living infusory forms occurring in the North Sea, one which is quite like the *Dictyocha speculum*, but spinous like the *Dictyocha aculeata* of Sicily. Finally, M. Ehrenberg met with a series of eight species of the genus *Actinocyclus*, and of the radiated division without partitions which form the great mass of the siliceous portion of the chalk-marls of Caltanisetta, and still more of Oran, and which are well characterized by the number of their rays. He detected species with 6, 7, 8, 9, 10, 11, 12, and 15 rays, and which are designated *Actinocyclus biternarius* (with the *senarius*, which is also found), *A. septenarius*, *A. octonarius*, *A. nonarius*, *A. denarius*, *A. undenarius*, *A. bisenarius* (not *duodenarius*), and A. *quindenarius*. Of the whole of this subdivision, there were no living forms previously known, so that the members of it were supposed to characterize the chalk-marl of the ancient world, which is, therefore, now disproved. All these forms are polygastric infusoria of the family *Bacillaria*.

M. Ehrenberg has likewise found in a living state, in the water near Cuxhaven, which he examined with care about a year ago, three calcareous *Polythalamias* of the chalk, and two siliceous infusory animals of the chalk-marl. These are *Rotalia globulosa*, *R. perforata*, *Textilaria globulosa*, *Gallionella sulcata*, and *Navicula didymus*.

To these seventeen forms of the present world, and of the chalk-formation, are to be added two other siliceous infusory which occur alive in the seas of the North, and which have lately been detected in the chalk-marl, viz., *Striatella arcuata*, and *Tesella catena*.

The above nineteen forms of the chalk, and which are new as recent ones, together with those announced in October 1839 and June 1840, make the number of genera of these animalcules twenty-one, and of species forty, partly polythalamias and partly infusory animals, which are common to the present world and the chalk-formation. Anatomical preparations or drawings of all these forms were submitted to the Academy.

Notice regarding a cheap and easily used Camera Lucida, applicable to the delineation of Flowers and other small objects. By Sir John Robison, K. H., F.R.S.E., M.S.A. Communicated by the Society of Arts for Scotland.*

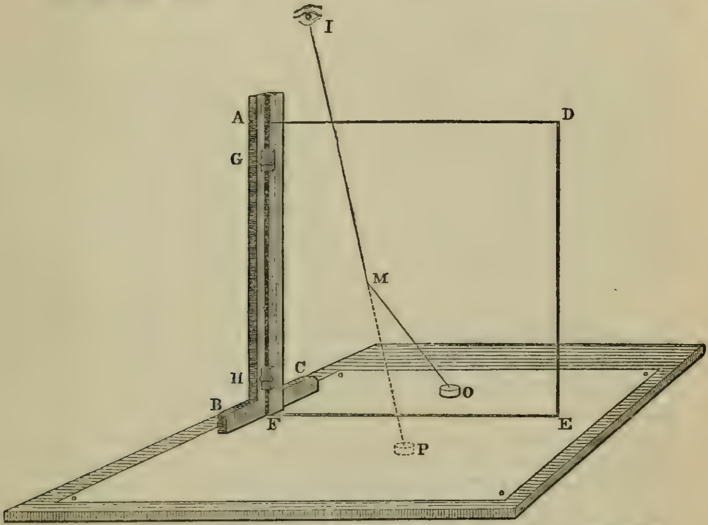
March 13. 1841.

Dear Sir,—In compliance with the wish expressed at the late meeting of the Society of Arts, I beg to send you a description and sketch of the Camera Lucida which I then exhibited. I again repeat, that its applicability to the delineation of objects is confined to such as are of small size, and as may be laid beside it on the drawing board. It is in this way par-

* Exhibited to the Society of Arts, 8th March 1841.

ticularly calculated for the accurate representation of botanical specimens, which it will render in their true proportions, without trouble in adjustments. The disadvantage of the limited range of this instrument is in some measure compensated by the facility of its application and by its cheapness and simplicity.—I am, dear Sir, very truly yours,

James Tod, Esq. JOHN ROBISON.
Sec. Soc. Arts.



The figure represents a piece of thin plate-glass A D E F set upright on a drawing-board by means of the wooden standard A B C, in a groove in which the edge of the plate is retained by the wedges G and H.*

To use this apparatus it is placed in front of the artist, with the standard A B C towards him; the object to be copied is laid on *the left hand side* of the plate, as at O: the head of the observer being also a little to the left of the pillar, and the eye directed towards the middle of the plate as at M, a distinct image of the object O will be perceived as if it lay on the paper at P; and as at the same time a pencil held on the right hand side of the plate will be equally visible, it may be applied to trace the image at P.

* The wooden standard is not fixed to the drawing-board, and may be set on it in any convenient position.

*Observations on Terra del Fuego.**

Species of Fungus used as a staple article of Food.—There is one vegetable production in this country, which is worthy of mention, as it affords a staple article of food to the aborigines. It is a globular fungus, of a bright yellow colour, and of about the size of a small apple, which adheres, in vast numbers, to the bark of the beech-trees. It probably forms a new genus, allied to the morell. In the young state, it is elastic and turgid, from being charged with moisture. The external skin is smooth, yet slightly marked with small circular pits, like those from the small-pox. When cut in two, the inside is seen to consist of a white fleshy substance, which, viewed under a high power, resembles, from the numerous thread-like cylinders, vermicelli. Close beneath the surface, cup-shaped balls, about one-twelfth of an inch in diameter, are arranged at regular intervals. These cups are filled with a slightly adhesive, yet elastic, colourless, quite transparent matter; and from the latter character, they at first appeared empty. These little gelatinous balls could be easily detached from the surrounding mass, except at the upper extremity, where the edge divided itself into threads, which mingled with the rest of the vermicelli-like mass. The external skin directly above each of the balls is pitted, and as the fungus grows old, it is ruptured, and the gelatinous mass, which no doubt contains the sporules, is disseminated. After this process of fructification has taken place, the whole surface becomes honey-combed, with empty cells, and the fungus shrinks, and grows tougher. In this state it is eaten by the Fuegians, in large quantities, uncooked, and when well chewed, has a mucilaginous and slightly sweet taste, together with a faint odour like that of a mushroom. Excepting a few berries of a dwarf arbutus, which need hardly be taken into the account, these poor savages never eat any other vegetable food besides this fungus†.

Forests.—I have already mentioned the sombre and dull character of the forests,‡ in which two or three species of trees grow, to the exclusion of all others. Above the forest land there are many dwarf

* From Mr Darwin's valuable and interesting "Journal and Remarks made during the Exploratory Expedition of the Adventure and Beagle." London, Colburn. 1839.

† In New Zealand, before the introduction of the potato, the root of the fern was consumed in large quantities. At the present day, I should think Terra del Fuego was the only country in the world where a cryptogamic plant afforded a staple article of food.

‡ Captain Fitzroy informs me, that in April (our October), the leaves of those trees which grow near the base of the mountains change colour, but not those in the more elevated parts. I remember having read some obser-

alpine plants, which all spring from the mass of peat, and help to compose it. The central part of Terra del Fuego, where the clay-slate formation occurs, is most favourable to the growth of trees; on the outer coast, the poorer granitic soil, and a situation more exposed to the violent winds, do not allow of their attaining any great size. Near Port Famine I have seen more large trees than any where else: I measured a Winter's-bark, which was four feet six inches in girth, and several of the beech were thirteen feet. Captain King also mentions one of the latter, which was seven feet in diameter seventeen feet above the roots.

Zoology.—The zoology of Terra del Fuego, as might have been expected from the nature of its climate and vegetation, is very poor. Of mammalia, besides *Cetacea* and *Phocæ*, there is one bat, a mouse with grooved front-teeth (*Reithrodon* of Waterhouse), a fox, sea-otter, guanaco, and one deer. The latter animal is rare, and is not, I believe, to be found south of the Straits of Magellan, as happens with the others. Observing the general correspondence of the cliffs of soft sandstone, mud, and shingle, on the opposite sides of the Strait, together with those on some intervening islands, one is strongly tempted to believe that the land was once joined, and thus allowed animals so delicate and helpless as the tucutuco and reithrodon to pass over. The correspondence of the cliffs is far from proving any junction; because such cliffs generally are formed by the intersection of sloping deposits, which, before the elevation of the land, had been accumulated near the then existing shores. It is, however, a remarkable coincidence, that, in the two large islands cut off by the Beagle Channel from the rest of Terra del Fuego, one has cliffs composed of matter that may be called stratified alluvium, which front similar ones on the opposite side of the Channel, while the other is exclusively bordered by the older rocks: in the former, called Navarin Island, both foxes and guanacoes occur; but in the latter, Hoste Island, although similar in every respect, and only separated by a channel a little more than half a mile wide, I have the word of Jemmy Button for saying that neither of these animals are found. I must confess to an exception to the rule, in the presence of a small mouse, of a species occurring likewise in Patagonia.

The gloomy woods are inhabited by few birds; occasionally the plaintive note of a white-tufted tyrant-flycatcher may be heard, concealed near the summit of the most lofty trees; and more rarely the loud strange cry of a black woodpecker, with a fine scarlet crest on its head. A little dusky-coloured wren (*Scytalopus fuscus*) hops, in a skulking manner,

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vations showing that, in England, the leaves fall earlier in a warm and fine autumn, than in a late and cold one. This change in the colour being retarded in the more elevated, and, therefore, colder situations, must be owing to the same general law of vegetation. The trees of Terra del Fuego during no part of the year entirely shed their leaves.

among the entangled mass of the fallen and decaying trunks. But the creeper (*Syxallaxis Tupinieri*) is the commonest bird in the country. Throughout the beech forests, high up and low down, in the most gloomy, wet, and impenetrable ravines, it may be met with. This little bird, no doubt, appears more numerous than it really is, from its habit of following, with seeming curiosity, any person who enters these silent woods; continually uttering a harsh titter, it flutters from tree to tree, within a few feet of the intruder's face. It is far from wishing for the modest concealment of the tree-creeper (*Certhia familiaris*), nor does it, like that bird, run up and down the trunks of trees; but industriously, after the manner of a willow-wren, hops about, and searches for insects on every twig and branch. In the more open parts, three or four species of finches, a thrush, a starling (or *Icterus*), two *Furnarii*, and several hawks and owls, occur.

The absence of any species whatever in the whole class of reptiles is a marked feature in the zoology of this country, as well as in that of the Falkland Islands. I do not ground this statement merely on my own observation, but I heard it from the Spanish inhabitants of the latter place, and from Jemmy Button, with regard to Terra del Fuego. On the banks of the St Cruz, in 50° south, I saw a frog; and it is not improbable that these animals, as well as lizards, may be found as far south as the Straits of Magellan, where the country retains the character of Patagonia; but within the damp and cold limit not one occurs. That the climate would not have suited some of the orders, such as lizards, might have been foreseen; but with respect to frogs, this was not so obvious.

Coleopterous insects occur in very small quantities. Until I had endeavoured by every means to find them, I could not believe, that a country as large as Scotland, covered with vegetable productions, and with a variety of stations, would ever have been so unproductive. The greater part of my small collection consists of alpine insects (Harpalidæ and Heteromera) found beneath stones, above the limit of the forest. Lower down, with the exception of some few Curculiones, scarcely any could be found. The Chrysomelidæ, which are so pre-eminently characteristic of the Tropics, are here almost entirely absent.* This must depend on the climate; for the quantity of vegetable matter is superfluously great. In the hottest part of the summer, the mean of the maxi-

* I believe I must except one alpine Haltica, and a single specimen of a Melasoma. Mr Waterhouse, who was good enough to look at my collection from this place, tells me, that of the Harpalidæ there are eight or nine species—the forms of the greater number being very peculiar; of Heteromera, four or five species; of Rhyncophora six or seven; and of the following families one species in each:—Staphylinidæ, Elateridæ, Cebrionidæ, Melolonthidæ. The species in the other orders were even fewer. In all the orders, the scarcity of the individuals was even more remarkable than that of the species.

ma for thirty-seven successive days was 55°, and the thermometer on some of the days rose to 60°; yet there were no Orthoptera, very few Diptera, Lepidoptera, or Hymenoptera. In the pools of water I found but few aquatic beetles, and not any fresh-water shells. Succinea at first appears an exception; but here it must be called a terrestrial species, for it lives on the damp herbage far from water. Land-shells could only be procured in the same situations with the alpine beetles. I have already contrasted the climate, as well as the general appearance of Terra del Fuego with that of Patagonia, and the difference is strongly exemplified in the entomology. I do not believe that they have a species in common; certainly the general character of the insects is widely different. If we turn from the land to the sea, we shall find the latter as abundantly stocked with living creatures as the former is poorly so. In all parts of the world a rocky and partially protected shore perhaps supports, in a given space, a greater number of individual animals than any other kind of station. Here, under every stone, numerous crawling creatures swarmed, and especially crustacea of the family of Cymothoades. The number of Sphaeroma was truly wonderful: as these animals, when coiled up, have some resemblance to Trilobites, they were an interesting sight to a geologist. On the tidal rocks patelliform shells of large size were very abundant. Even at the depth of forty or fifty fathoms, the bottom of the sea was far from steril, as was shewn by the abundance of small strong corallines.

Aquatic Forests of the Southern Hemisphere.—There is a marine production, which from its importance is worthy of a particular history. It is the kelp or *Fucus giganteus* of Solander. This plant grows on every rock from low-water mark to a great depth, both on the outer coast and within the channels. I believe, during the voyages of the Adventure and Beagle, not one rock near the surface was discovered which was not buoyed by this floating weed. The good service it thus affords to vessels navigating near this stormy land is evident; and it certainly has saved many a one from being wrecked. I know few things more surprising than to see this plant growing and flourishing amidst those great breakers of the western ocean, which no mass of rock, let it be ever so hard, can long resist. The stem is round, slimy, and smooth, and seldom has a diameter of so much as an inch. A few taken together are sufficiently strong to support the weight of the large loose stones to which in the inland channels they grow attached; and some of these stones are so heavy, that when drawn to the surface they can scarcely be lifted into a boat by one person.

Captain Cook, in his second voyage, says, that at Kerguelen Land, "some of this weed is of a most enormous length, though the stem is not much thicker than a man's thumb. I have mentioned, that on some of the shoals upon which it grows, we did not strike ground with a line of twenty-four fathoms. The depth of water, therefore, must have been very great. And as this weed does not grow in a perpendicular direction, but makes a very acute angle with the bottom, and much of it afterwards spreads many fathoms on the surface of the sea, I am well warranted to say that some of it grows to the length of sixty fathoms and upwards." Certainly at the Falkland Islands, and about Terra del Fuego, extensive beds frequently spring up from ten and fifteen fathom water. I do not suppose the stem of any other plant attains so great a length as 360 feet, as stated by Captain Cook. Its geographical range is very considerable; it is found from the extreme southern islets near Cape Horn, as far north, on the eastern coast (according to information given me by Mr Stokes) as lat. 43°, and on the western it was tolerably abundant, but far from luxuriant, at Chiloe, in lat. 42°. It may possibly extend a little farther northward, but is soon succeeded by a different species. We thus have a range of fifteen degrees in latitude; and as Cook, who must have been well acquainted with the species, found it at Kerguelen Land, no less than 140° in longitude. The number of living creatures of all orders, whose existence intimately depends on the kelp, is wonderful. A great volume might be written, describing the inhabitants of one of these beds of sea-weed. Almost every leaf, excepting those that float on the surface, is so thickly incrusted with corallines, as to be of a white colour. We find exquisitely-delicate structures, some inhabited by simple hydra-like polypi, others by more organized kinds, and beautifully compound Ascidiæ.* On the flat surfaces of the leaves various patelliform shells, Trochi, uncovered molluscs, and some bivalves, are attached. Innumerable crustacea frequent every part of the plant. On shaking the great entangled roots, a pile of small fish, shells, cuttle-fish, crabs of all orders, sea-eggs, star-fish, beautiful Holuthuriæ (some taking the external forms of the nudibranch molluscs), Planariæ, and crawling nereidous animals of a multitude of forms, all fall out together. In Chiloe, where, as I have said, the kelp did not thrive very well, the numerous shells, corallines, and crustacea, were absent; but there yet remained a few of the flustraceæ, and some compound Ascidiæ; the latter, however, were of different species from those in Terra del Fuego. We here see the fucus possessing a wider range than the animals which use it as an abode.

I can only compare these great aquatic forests of the southern hemisphere with the terrestrial ones in the intertropical regions. Yet if the latter should be destroyed in any country, I do not believe nearly so many species of animals would perish, as, under similar circumstances, would happen with the kelp. Amidst the leaves of this plant numerous species of fish live, which nowhere else would find food or shelter; with their destruction the many cormorants, divers, and other fishing birds, the otters, seals, and porpoises, would soon perish also; and, lastly, the Fuegian savage, the miserable lord of this miserable land, would redouble his cannibal feast, decrease in numbers, and perhaps cease to exist.

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* I have reason to believe that many of these are exclusively confined to this station.

Dr Boué's Researches in Geography, Natural History, &c. in the European Provinces of Turkey.

We are happy to have this opportunity of bringing before the notice of our readers a work which, both from the interest attached to the countries treated of, and from the diligence and research which it evinces, deserves, we think, a high rank amongst the contributions of modern travellers and naturalists. Its author, Dr Boué, is one of the most laborious, and, we should add, the most disinterested geologists in Europe.

Without being connected with any public institution, he has devoted his life and fortune for the last twenty-five years to extensive travels through every part of Europe, and to the accumulation of every kind of information respecting the structure of the globe which he could obtain from books, from lectures, or from personal communication with those similarly engaged.

His first work comprehended the fruits of his labours whilst a student at Edinburgh, and was entitled "*Essai Géologique sur l'Ecosse.*" It is still considered the most complete general view of the geognosy of that country hitherto published; and when it appeared, was highly praised by Jameson, his instructor in geology, and other competent judges. He afterwards published a great variety of Memoirs on the Geology of France and Germany, and an entire volume on that of the latter country, in consequence of which services to science, he, some years ago, was chosen President of the Geological Society of France, a high honour for one who was not a Frenchman, and who had ever kept aloof from any of the parties and coteries of that metropolis. For the last four years he has been employed wholly in the investigation of the provinces of European Turkey—a strong proof of his zeal for science, as his private means are too limited to allow of his purchasing those comforts which could alone render travelling in such a country endurable to persons of European habits, and as he has never derived the slightest assistance from Government or from any public body.

How little was known of the provinces he has explored may be seen from the very meagre account given of them in the most authentic work of modern geography, that of Malte Brun.

Those travellers who, like Quin, passed through them in their way to Constantinople, have commonly given the most vague and erroneous accounts, partly from having hurried through the country, and partly from ignorance of the languages there current. Dr Boué, on the contrary, made the provinces the principal objects of his attention, and took care to make himself duly acquainted with the languages of the people he visited. In proof of his competency to the task, I cannot do better than quote the testimony of Mr Berghaus, in his Almanack, who remarks:* "We may, with justice and propriety, term Boué's journey through European Turkey a journey of discovery for geography as well as for geology. For although our maps of Turkey are filled up with the most minutely delineated chains of mountains, and exhibit a perfectly complete hydrographical set of serpentine rivers and streams, yet we know well that these apparently accurate representations belong, for the most part, to the phantasmagorical class, and can scarcely deceive the most credulous.

"Most of the other countries of Europe have been surveyed and described, but we grope in profound darkness when we inquire into the natural external form and the geognostical constitution of the Turkish possessions. European prejudices and mercantile interests are undoubtedly the chief means that have, to so great an extent, prevented travellers from visiting a country which, now that many portions of it enjoy the blessings of peace, and that the former fanaticism of its inhabitants has begun to disappear, does not present the great difficulties formerly encountered. The indefatigable Boué, who has examined the geological structure of nearly all the countries of Europe, and who saw in Turkey an entirely unexplored field for new observations, resolved to devote three or four years to its investigation, and to associate with himself in his enterprise, naturalists who prosecuted other departments in natural history.

"In the year 1836, during a portion of his journey, he enjoyed the society of two French geologists, M. Montalembert and Viquenel, of M. Friedrichtshal, a botanist, and of M.

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* From Jameson's Journal, vol. xxv. p. 174.

Adolf Schwab, a zoologist, the two latter natives of Moravia. It is to be regretted that Boué did not take with him a measuring geographer, that is, a person provided with the requisite instruments, who should have been able to determine positions, and to ascertain the three co-ordinates of a great number of points, so as to furnish the foundation for a new and accurate map, of which want our traveller bitterly complains." Such is the testimony of a German geographer to the merits of the undertaking, of which the results are embodied in the four volumes now published.

Nor can it be said that his time was devoted to a country deficient in interest. The recent events in the East are calculated to increase our curiosity as to the character of the people who occupy these regions; for, without entering here on any political discussion, it seems evident that one of two events must shortly happen; either that the Turkish empire will undergo an entire dissolution, in which case these several provinces will rise into so many distinct states, or be absorbed by the Russian or Austrian dynasties, which border upon them; or else that Turkey will acquire a portion of the civilization of the West, and become in some degree regenerated under the influence of European example. Whichever of these events may occur, the provinces of European Turkey must clearly emerge out of their present state of barbarism, and will become in consequence every year more interesting to the other nations of Europe.

Even at present they afford a curious subject of contemplation to the philosopher, as examples of the Eastern type of manners ingrafted on a European, and in many places upon a Christian, population; whilst the classical reader cannot fail to take an interest in localities which recall to his recollection the names of Epirus, of Macedonia, of Thessaly, and of Thrace.

We propose, therefore, to present such a brief sketch of the work alluded to, as may convey some slight idea of its character and its value.

*A Brief Abstract of Dr Boué's Work on Turkey.**

This work is divided into three parts. The first compre-

* Entitled "La Turquie d'Europe, par Ami Boué. 4 vols. Paris, 1840."

hends the geography, geognosy, natural history, and meteorology of the country, and occupies one volume. The second part relates to the social condition of the people, and occupies two volumes. The first of these volumes describes the language and character of the inhabitants, their dress, food, houses, public edifices, fortresses, manners and usages, antiquities, &c. The second volume treats of the state of agriculture, commerce, education, religious instruction, civil administration, &c. The third part is historical and political, giving an account of the civil condition, past and present, of the eight nations which are contained in Turkey. The whole is concluded by an article on the mode of travelling, table of heights and of distances, and the roads.

The portion which relates to the geography of Turkey is hardly susceptible of analysis. It is in great detail, and implies a long residence in the country.

It appears that there are no less than seven or eight systems of mountains in Turkey, which render its description very complicated, as there is no central chain to serve as a point of departure, as is the case in Italy and in the Alps. Of these systems the following may perhaps be pointed out as the principal:—

1*st*, The range of hills running parallel with the Adriatic, which forms the country of Montenegro. It runs from north-west to south-east, and its greatest heights are 8500 to 9000 feet above the sea. It is the Scardus of the ancients, the Schardagh of the Turks.

2*d*, The chain of Despotodagh (the Rhodope of the ancients), which runs from north-west to south-east, dividing Macedonia from Thrace. The highest summits are from 7000 to 7800 feet above the sea.

3*d*, The chain which divides Thessaly and Epirus from Macedonia. It comprehends the ancient Pindus, and extends down to Mount Olympus. The height cannot be less than 5000 feet.

4*th*, The Balkan, which stretches from west to east, dividing Bulgaria from Thrace. Its elevation is comparatively small, the Great Balkan not exceeding 3000 feet, the Little Balkan 2000.

5th, A chain extending through the centre of Turkey, and dividing Wallachia from Servia, being a continuation of the hills of the Bannat.

6th, There is another chain separating Servia from Bosnia, and a third traversing the centre of the province. These three chains all meet in Servia, forming the boundary between it and High Mœsia.

The highest mountains are those of the western chain in Bosnia, which is another Switzerland in the configuration of the country; and next are the mountains of Thrace, called Despotodagh. Those of Schar and of the Balkan come next in point of elevation. Between these chains lie several rich valleys, as those of Wallachia, of Servia, of Macedonia, and of Thrace.

The directions of the chains are in general such as to afford greater facilities for travelling from north to south than from west to east; accordingly, it is found much more easy to construct a road from Belgrade to Constantinople, than from Trieste across to the Black Sea.

Geognosy of Turkey.

The geological structure of European Turkey is more simple than that of the western portions of Europe. There seems to be no trace in it of the coal formation, or of any rocks intermediate between this and the cretaceous deposits. Gneiss, mica, and clay-slate constitute the high ground of Despotodagh, Rhodope, the Balkan, and the greater part of the principal chains above enumerated. Subordinate to gneiss are rocks consisting of whitestone, hornblende rocks, garnet rock, and granular limestone. Subordinate to the mica and clay-slates we meet with talc and chlorite slates, quartz rock, compact and granular limestones, dolomites, and, more rarely, slates impregnated with hornblende. The inclination of the beds is very various, depending on the different directions in which the volcanic forces appear to have operated upon them. Both the gneiss and the other slates seem to pass into certain conglomerates, which may be regarded as greywacke, as is the case in High Mœsia. The latter rocks pass into sandstone, grit, and calcareous breccia. The fossils which these rocks occasionally contain near the Bosphorus, would lead us to place them in the Silurian system.

Secondary rocks.—The only rocks belonging to the secondary class seem to be cretaceous, which may be divided into three groups, namely, 1*st*, siliceous beds with little limestone; 2*d*, calcareous beds often containing hippurites; and 3*d*, a formation very rich in nummulites.

The siliceous deposit constitutes the centre of Servia and certain portions of Eastern Mœsia. The hippurite limestone forms a large portion of the Balkan, of Mount Pindus, and of Transylvania, and a still larger part of Bosnia and Croatia. The nummulite formation is seen in Epirus, and in much of the western portion of Turkey. The inferior or siliceous beds of the cretaceous formation contain vegetable remains (*Fucoides*), together with various shells characteristic of chalk, such as *echini*, *orbitolites*, *ostreæ*, *plageostoma*, &c. The superior beds consist of green sand in their lower, and of calcareous beds with flints in their upper portions. The petrifactions are like those of the chalk of the west of Europe. They abound in caverns, in which the rivers frequently lose themselves, as happens also in Greece, where they are called *Katovothrons*.

This formation also comprehends, in Turkey, as in the Alps, enormous masses of dolomite, and of limestone, full of cracks and fissures, and containing a little magnesia. These dolomites constitute a series of peaks in Albania and in other provinces. They appear to contain masses, though in appearance small, of diorite.

The nummulite formation contains much mineral pitch, which, by being inflamed, appears to be the cause of the *Nymphæum* of Plutarch, which exists in Dalmatia. Above the chalk are extensive fresh-water formations, together with enormous deposits of travertin. Large beds of conglomerate are seen as a part of this formation in Thessaly. The partial destruction of these beds has caused large blocks to be scattered over the surface of the country, which appear like erratic boulders, but which Dr Boué distinguishes from them. There is every appearance that Thessaly was once a lake, which drained off when the fissure at Tempe was produced.

But the deluge of Deucalion and Pyrrha our author considers as subsequent to the draining of the valley, and to have been caused by the stopping of the course of the river, which now communicates with the sea.

Igneous rocks.—Turkey has examples of nearly every kind of igneous rock excepting basalt, but quartzous porphyry is rare. Granite is seen in the centre of the country, and in many places contiguous to the Black Sea. Protogene or talcose granite is occasionally met with, and more rarely syenite. Serpentine is a very abundant rock in the chalk both in the west of Turkey and likewise in Servia. But the largest eruptions of serpentine lie betwixt Prisren and Scutari, and in the Pindus, where it is accompanied with euphotide.

Diorite (the ophite of the Pyrenees) is only met with in Albania, where it pierces the cretaceous slates, and produces sundry changes in them, hardening and turning them into jasper.

Trachytic rocks are very abundant, especially in Macedonia and Thrace. The millstone porphyry of Hungary is seen in the mountains of Karatova.

Dr Boué distinguishes the trachytes from the syenitic porphyries often associated with them, regarding the former as subærreal, the latter as partly submarine.

Pyroxenic porphyry is found in great abundance at the foot of the Balkan, north of Aidos, accompanied with a breccia.

In High Mœsia, near Sophia, the rocks resemble much those of the Val de Fassa in the Tyrol. This porphyry traverses the middle and upper tertiary formation, whilst the trachytes appear somewhat more ancient, beginning at the epoch of the chalk, and terminating at that of the nearest tertiary.

The diorites and serpentines traverse the ancient rocks, and especially the chalk; whilst the granites appear to have been ejected at the end of the ancient primary epoch. The continuance of igneous action in Turkey seems to be evinced by the great abundance of thermal waters which occur at the foot of most of the chains. They almost all contain sulphuretted hydrogen.

Thus on the great scale Turkey may be regarded as formed by a bifurcation of the Alps, which here, as in Styria and Carinthia, divides into two branches. Of these the south-eastern, having reached Upper Mœsia, divides into an eastern and south-south-eastern portion, between which two rise, as in the Alps, enormous mountains of crystalline slates. The latter, accompanied by these same secondary rocks, extend to the Archipelago as well as into Asia Minor, and support in the Taurus, as in Turkey, enormous masses of chalk.

The crystalline rocks of Turkey are distinguished from those of the Alps in containing many more distinct basons, and a larger number of great cavities, which are filled with tertiary deposits, but are unaccompanied by erratic blocks, although the deposit of *Meteora* in Thessaly would seem to indicate a similar event during the tertiary epoch. They are also pierced by great trachytic eruptions even in the centre of the chains, whereas in the Alps these latter are only found at the foot of the range. The serpentines of Turkey bear the greatest resemblance in the mode of their distribution to those of the Tyrol.

The western side of Turkey presents the counterpart of the Italian peninsula, with this difference, that sandstone rocks predominate in the latter and calcareous in the former; and that the tertiary or subapennine deposits are less abundant on the Turkish side.

The operation of volcanic forces seems to be evinced even at the present day by the frequent earthquakes which are recorded by ancient writers, and are frequently experienced at present, as along the western coast. The most destructive was that of 1667, which destroyed Ragusa. Wallachia is also exposed to the same calamity, as in 1838.

The islands bordering upon the coast often suffer severely, particularly Zante; and in others, as at *Melida* near *Ragusa* off the coast of Dalmatia, subterraneous noises are sometimes heard, which are referable to the same cause.

Vegetation of Turkey.

We find in Turkey the elements of at least five foreign Floras, viz. that of Hungary, that of Transylvania and Bulgaria, of the Chersonesus, of Asia Minor, and of Greece; to which may be added perhaps that of Dalmatia and Italy.

We may distinguish three botanical regions; 1. That of the low land, in which corn, vines, and fruit-trees are abundant; 2. The sub-alpine region, in the lower part of which the greater part of the forests occur.

The character of the forests, however, depends upon the elevation. From 3200 to 2500 feet above the sea they consist chiefly of oak; of which the species most common are *Quercus robur* (the English oak) and its varieties, *pubescens* and *pedunculata*, *Q. cerris* (the Turkey oak), *Ægilops* (*Velonia*), *cylindrica* and *apennina;* together with the *Q. ilex* (holm oak), and *Q. coccifera* (*Kermes*), in Epirus and Central Albania. The Judas tree, myrtle, *Nerium oleander*, *Colutea arborescens*, *Tilia argentea*, and several species of poplar, are associated with the oak in this country. Below the region of oaks, and rising to the level of about 2500 feet, the forests consist of the Spanish-chestnut, with the filbert and hazel-nut; on the borders of the Adriatic, and at the foot of the mountains of Thessaly, orange and lemon trees are found, and also at this low level the olive and the pomegranate will flourish. The larch-tree reaches to Central Albania, and the Oriental plane extends as far as the Balkan. The *Laurus nobilis* or Bay laurel forms groves at a height of 1500 or 1600 feet in Epirus.

The upper limit of maize in Thessaly is about 2850 feet above the sea; of the rice-grounds, 1090, in Thrace; of the cotton, 2500; oats and barley reach 3800 on Mount Pindus, and about 2400 in the Balkan. The beech grows from a height of 2000 to 4900; the fir from 6000 to 2400; but it is to be remarked that whilst the *Pinus picea* (the silver fir,) *Brucea* (Naples fir), *Pinaster* (the *Pinaster*), and *Picea* (stone pine), attain the elevation of 6000, the *Abies communis*, or Norway fir, and the Larch, seem to be limited to a height of 4000.

3. With respect to the third or alpine region of Turkey, from 6000 feet upwards, the plants that characterize it are certain lichens, saxifrages, and gentians, the *Juniperus nana*, *Dryas octopetala*, *Draba aizoon*, *Ranunculus nivalis*, and other species found also for the most part at similar levels in the Alps.

Dr Boué concludes this part of his work with a long catalogue of the plants of Turkey, and states that the best localities for botanical researches are the Balkan, Mount Rhodope, and the chains betwixt Albania, Croatia, and Servia, especially Mounts Scardus and Pindus. I will say nothing of the Fauna of Turkey, to which the best portion of his first volume is devoted, except to remark, that this country affords the great supply of leeches for the rest of Europe, respecting which, in a subsequent volume, Dr Boué has given some curious details.

The climate of Turkey is subject to great extremes of heat and cold, and the winters are more rigorous than the latitude would lead us to suppose. Owing to the heat and the number of stagnant waters, fevers arising from malaria are very frequent after the commencement of July. April, May, and June are therefore the best months for travelling. Dr Boué has given some useful instructions for those who meditate a journey in this country. There are, it seems, three methods by which it may be effected. The first is a simple passport; the second one of a superior kind, called Bourjardi, given by the Pashas, containing an order for lodging; the third a Firman, an order from the central government, which entitles the traveller to have a Tartar as a travelling companion and protector. The traveller thus provided has a right to claim a lodging in any place he arrives at from the Pasha. It is necessary, however, to carry a bed, an iron-kettle for making soup, &c. and many other articles not required in other parts of Europe, and to be prepared against the contingency of finding the house consigned to you as a lodging deserted in consequence of the exactions of the Turks, who when they travel often go away without making any remuneration to their hosts for their reception.

With respect to the people, the Montenegrans and Albanians are the most wild and barbarous, the Servians the highest in the scale of civilization. The former carry their family feuds to such an extent, that blood can only be expiated by blood. Nevertheless, travelling is not unsafe, unless in cases where some suspicion or national enmity attaches to the individual. The Servians, on the contrary, are highly praised by our author for their simplicity and probity of character. They revolted from the Sultan under a chieftain called Tzerni George about the beginning of the century, and though reduced to submission in 1812, when the Porte had concluded a treaty of peace with Russia, and had its troops at liberty, yet afterwards they were driven to take up arms by the oppression they underwent, and at length obtained of the Sultan a kind of half compromise, which left them at liberty to govern themselves under the superintendence of a native Servian prince, Milosch, paying an annual tribute only to the Sovereign. Prince Milosch is represented as a man of great natural acuteness, though totally uneducated, being unable even to read or write. With many of the vices of the barbarian, he seemed in the main well fitted to govern such a people, affecting no show or state, living in a plain and homely style, and conducting the government in an Eastern fashion, exempt from many of the abuses that had crept into the Turkish administration. Dr Boué contrasts the state of things in Servia and in Greece, and gives the preference to the former.

He concludes with a table of the heights above the sea of no less than 350 places, measured by himself barometrically during his travels in Turkey.

Appendix to Dr Richardson's Observations on Solar Radiation.

Professor Forbes having referred to Leslie's Photometer in the remarks which he has had the kindness to make on my observations, I have, with the view of rendering the paper more complete, added two tables containing abstracts of a register of that instrument kept at the same time with that of the radiation thermometers. In the spring months the action of the sun on the photometer was so powerful as to drive the coloured liquid beyond the limits of the scale attached to the instrument, and in twelve different instances, in March 1826, entirely into the clear bulb. To remedy the shortness of the scale, I divided the two limbs of the photometer, including the bend at the bottom, into 35°, each degree being as nearly as possible equal to one of the scale attached to the descending limb by the maker. As results such as I have mentioned did not seem to be contemplated by the inventor of the instrument, and as I conjectured that the very low mean temperature of the air in February, March, and April, might affect the indications of the instrument by greatly contracting the coloured fluid, I did not print the register in Franklin's Appendix. The abstracts now given, however, serve to corroborate the deductions made from the observations on the radiation thermometer as to the greater effects of the sun-light in spring in high latitudes, whether it be owing to reflection from the snow or some other cause. Table VIII. contains an abstract of the register for three months in which the observations were most regularly made. Table IX. includes the months in which the observations were made only on favourable or convenient days. From the crust which forms on the snow in March, the radiation from it affects the eyes more severely in March and April than at other times.

TABLE VIII.

Results of a Register of the Indications of Leslie's Photometer kept at Fort Franklin in 1826.

Hour.	February, whole Month.		March, whole Month.		May, 26 last Days.	
	Mean Height of Photometer.	No. of Obs.	Mean Height of Photometer.	No. of Obs.	Mean Height of Photometer.	No. of Obs.
A.M.						
8	...	...	13.24	17	6.09	22
9	5.53	28	16.19	27	7.09	25
10	11.31	28	22.47	29	7.74	22
11	14.41	28	20.26	29	8.67	23
Noon.	14.20	28	22.08	30	9.70	24
1	11.37	28	20.41	31	9.45	22
2	10.12	28	19.28	29	8.42	24
3	7.05	16	15.34	29	7.28	20
4	3.05	11	12.39	29	6.16	22
5	...	...	9.12	18	6.95	14

TABLE IX.

Results of a Register of the Indications of Leslie's Photometer kept at Fort Franklin in 1825–26.

Hour.	October.		November. 1825.		December.		January. 1826		April. 1826. First 7* Days only.	
	Observations spread through the whole Month.									
	Mean Height of Photometer.	No. of Obs.	Mean Height of Photometer.	No. of Obs.	Mean Height of Photometer.	No. of Obs.	Mean Height of Photometer.	No. of Obs.	Mean Height of Photometer.	No. of Obs.
A.M.										
8	1.63	7	...	...	...	...	...	...	11.99	22
9	2.45	14	0.69	7	...	...	...	...	14.00	25
10	5.00	15	4.12	13	0.55	2	8.33	3	17.30	22
11	7.83	11	5.68	18	2.99	15	16.00	4	22.76	23
Noon.	5.43	19	6.96	23	5.16	18	12.30	8	21.70	24
1	3.60	16	7.24	15	2.68	17	16.17	3	20.12	22
2	5.69	15	5.84	14	0.71	9	9.73	6	17.79	24
3	4.99	9	2.81	8	...	...	2.70	2	18.00	20
4	2.15	4	...	...	...	...	...	...	13.97	22
5	...	...	...	...	...	...	...	...	7.20	14

* Note.—The observations on the Photometer were interrupted by my absence from the Fort from the 8th of April to the 6th of May; but up to the end of April the register of the radiation thermometer was kept by the other officers when I was absent.

BRITISH METEOROLOGICAL TABLES.

Mean state of the Barometer and Thermometer at Canaan Cottage, near Edinburgh. By A. Adie, Esq.

Lat. 55° 57′.—Height above the mean level of the sea, 216 feet.

1840.	Thermometer Register.				Barometer.		Quantity of Rain.
Months.	Morning.	Evening.	Minim.	Maxim.	Morning.	Evening.	
January, . . .	38.55	38.93	33.37	45.43	29.24	29.24	3.72
February, . .	36.72	36.38	31.66	43.28	29.60	29.58	1.58
March, ...	42.94	37.61	33.16	49.13	30.05	30.03	.43
April, ...	50.66	45.47	39.17	57.57	29.77	29.79	.19
May, ...	49.29	44.95	40.87	55.52	29.64	29.65	3.97
June, ...	58.37	50.70	46.07	64.17	29.56	29.60	2.51
July, ...	52.07	51.36	47.81	64.09	29.57	29.52	3.46
August, ...	58.61	56.39	50.55	67.71	29.65	29.60	1.99
September, . .	49.43	48.07	43.57	59.23	29.41	29.43	2.59
October, ...	44.32	43.74	39.03	53.03	29.71	29.43	2.01
November, . .	40.70	41.04	35.63	47.73	29.32	29.34	2.33
December, . .	37.80	37.03	30.81	41.71	29.92	29.91	0.68
Mean Annual Temperature and Pressure,	45°.46		46°.63		29.61 inches.		25.26 in.

Lord Gray's Meteorological Table for 1840.

Extracted from the Register kept at Kinfauns Castle, North Britain.
Lat. 56° 23′ 30″.—Above the level of the sea, 150 feet.

1840.	Morning, ¾ past 8. Mean Height of		Evening, 8 o'clock. Mean Height of		Mean Temperature by Six's Thermometer.			Depth of Rain in Garden.	No. of days.	
	Barom.	Ther.	Barom.	Ther.	Lowest.	Highest.	Mean.		Rain or Snow.	Fair.
January, .	29·381	37·387	29·407	35·226	33·258	41·646	37·452	3·96	17	14
February,	29·737	37·103	29·744	36·345	33·138	42·034	37·586	2·12	15	14
March, . .	30·209	39·161	30·211	38·870	32·612	48·484	40·548	·25	4	27
April, . . .	29·915	46·966	29·926	44·433	38·500	55·700	47·100	·14	2	28
May, ...	29·755	48·548	29·763	45·483	41·064	56·290	48·677	2·85	16	15
June, . . .	29·691	54·633	29·676	52·800	46·766	63·033	54·900	1·60	14	16
July, . . .	29·580	56·322	29·599	53·935	46·903	64·129	55·516	4·45	20	11
August, . .	29·686	56·548	29·636	56·741	49·483	66·258	57·870	1·98	7	24
September,	29·503	50·600	29·510	48·200	43·300	57·766	50·533	2·23	17	13
October, . .	29·841	44·741	29·845	43·032	38·741	52·677	45·709	2·97	11	20
November,	29·447	40·466	29·490	39·733	35·866	46·933	41·400	2·17	12	18
December,	30·040	33·129	30·030	36·034	32·933	41·774	37·355	·45	7	24
Average of the year,	29·732	45·717	29·741	44·238	39·380	53·060	46·220	25·17	142	224

ANNUAL RESULTS.

MORNING.

BAROMETER.	Observations.	Wind.	THERMOMETER.	Wind.
Highest,	27th December,	N. 30·64.	9th August,	SW. 64°.
Lowest,	26th January,	SW. 28·20.	30th January,	NE. 24°.

EVENING.

Highest,	8th March,	E. 30·62.	9th August,	SW. 67°.
Lowest,	24th January,	SW. 28·16.	6th January,	NW. 24°.

Weather.	*Days.*	*Wind.*	*Times.*
Fair	224	N. and NE.	44
Rain or Snow,	142	E. and SE.	98
		S. and SW.	92
	366	W. and NW.	132
			366

Extreme Cold and Heat by Six's Thermometer:—

Coldest,	30th January,	Wind NW.	20°
Hottest,	9th August,	Wind SW.	76°
Mean Temperature for the year 1840,			46°.220.

Results of two Rain-Gauges.

	In. 100.
1. Centre of Kinfauns Garden, about 20 feet above the level of the sea,	25·17
2. Square Tower, Kinfauns Castle, 180 feet,	25·22

Abstract of Meteorological Register for the year 1840, *kept at Inveresk, near Musselburgh.* Communicated by Admiral Sir David Milne.

Year 1840.	Barometer at 9 A.M.	Barometer at 9 P.M.	Thermom. at 9 A.M.	Thermom. at 9 P.M.	Ther. least in Night.	Ther. highest in day.	Rain in Inches.	Fair or dry days.	Rainy or Snowy days.	Nights Aurora seen.	Days.
January, ...	29.349	29.355	37.838	37.613	30.903	42.129	3.666	10	21	4th.	31
February, ...	29.715	29.723	36.413	35.582	32.175	41.103	1.555	16	13	6th.	29
March, ...	30.021	30.022	38.903	38.	29.193	47.806	.444	24	7	...	31
April, ...	29.954	29.943	48.233	45.003	39.066	57.066	.222	22	8	24th.	30
May, ...	29.780	29.780	48.409	46.548	41.870	55.870	3.833	15	16	...	31
June, ...	29.400	29.400	56.266	52.533	47.966	62.933	2.111	10	20	...	30
July, ...	29.576	29.565	56.709	52.903	48.225	61.838	3.	9	22	...	31
August, ...	29.697	29.728	59.193	55.064	49.612	65.806	2.	15	16	...	31
September, ...	29.482	29.505	50.733	48.2	44.6	57.633	2.555	10	20	...	30
October, ...	29.801	29.804	45.322	43.387	38.516	52.387	2.	22	9	19th.	31
November, ...	29.310	29.334	40.433	39.233	35.633	45.666	2.666	17	13	...	30
December, ...	30.003	30.038	34.645	35.322	31.354	40.	.777	20	11	21st.	31
Average of Year, . .	29.721	29.729	46.139	44.142	39.374	52.543	24.829 Total.	190 Total.	176 Total.	5 Nights.	366
Total Degrees below Freezing Point, ...			96	99	301	6					
Total Degrees above Freezing Point, ...			5271	4513	3000	7525					

WINDS.

Year 1840.	Winds at A.M. P.M.	N.	N.N.E.	NE.	E.N.E.	E.	E.S.E.	SE.	S.S.E.	S.	S.S.W.	SW.	W.S.W.	W.	W.N.W.	NW.	N.N.W.
January	A.	...	1	1	1	...	...	...	1	3	7	9	3	3	1	...	1
	P.	...	1	1	1	...	...	...	2	4	5	6	3	6	2	...	...
February	A.	1	...	...	1	4	2	5	1	3	...	10	1	...	1	...	...
	P.	3	...	1	...	5	1	3	1	1	2	8	1	1	1	...	1
March	A.	5	...	3	3	1	1	3	1	...	...	3	...	3	3	5	...
	P.	7	1	5	1	2	...	3	2	...	...	...	...	1	3	4	2
April	A.	6	2	2	1	1	...	...	...	1	1	3	...	7	3	2	1
	P.	5	2	3	1	3	1	...	...	...	...	3	2	7	1	2	...
May	A.	5	2	7	...	2	1	...	1	...	1	2	2	3	1	4	...
	P.	7	3	4	...	2	...	...	1	...	1	4	2	2	2	3	...
June	A.	2	...	1	...	...	1	...	1	2	1	6	3	5	3	4	1
	P.	4	1	...	...	...	...	...	2	3	1	5	1	10	...	1	2
July	A.	2	...	1	...	1	...	2	...	...	3	3	3	9	1	4	2
	P.	4	...	2	...	1	1	...	...	3	1	6	1	8	...	3	1
August	A.	4	...	1	...	1	...	2	...	...	1	11	1	8	...	2	...
	P.	4	3	2	...	...	...	...	1	...	3	7	1	8	2	...	...
September	A.	4	1	1	...	...	1	...	...	4	2	6	2	9	...	...	...
	P.	3	1	1	2	...	...	1	...	1	3	4	5	7	2	...	...
October	A.	...	1	1	1	...	...	...	...	2	1	4	...	15	2	4	...
	P.	3	...	4	1	...	...	1	...	...	...	5	2	9	2	4	...
November	A.	5	...	...	...	1	1	4	2	1	2	6	1	6	...	1	...
	P.	4	...	...	1	1	...	3	1	5	...	6	1	4	...	2	2
December	A.	4	...	...	1	1	2	2	2	6	1	6	2	3	1	...	...
	P.	4	1	...	...	2	1	3	...		2	4	1	4	2	...	...

Barometer at 9 a.m. was highest on the 27th of December, 30.67, wind calm from S.S.E.; and was lowest on 26th January, 28.00, wind strong from SW.

Barometer at 9 p.m. was highest on 26th December, 30.66, wind calm from E.S.E.; and was lowest on 24th January, 28.02, wind strong from W.S.W.

Thermometer at 9 a.m. was highest on 9th August, 69, wind calm from W.; and was lowest on 24th December, 20, wind calm from SE.

Thermometer at 9 p.m. was highest on 20th August, 66, wind gentle from W.; and was lowest on 23d December, 25, wind calm from S.

Thermometer in Night was highest on 20th August, 60; and was lowest on the 23d December, 19.

Thermometer in Day was highest on 9th August, 76; and was lowest on the 7th of January, 30.

February was the coldest month; August hottest; May wettest; and April driest.
The coldest day was 24th December, when average of Thermometer was 26.
The hottest day was 20th August, when average of Thermometer was 66.
Total rain fallen in the year, 24.829 inches.
Mean temperature of the year, 45.549.

Abstract of Meteorological Observations for 1840, *made at Applegarth Manse, Dumfriesshire.* By the Rev. W. Dunbar, D.D.

Long 3° 12′ W. Lat. 55° 13′ N.; Height above the Sea, 180 feet; Distance from the Sea, 10 miles; Rain-Gauge, 5 feet from the ground. The observations made at 9 A.M. and 9 P.M.

BAROMETER.

1840. Months.	Atmospheric Pressure, Morning.	Red. to 32° Fahr., corrected to sea-level.	Atmospheric Pressure, Morning.	Reduced and Corrected.	Mean of Morning and Evening.	Reduced and Corrected.	Mean Range in the day.	Mean Range in the night.	Mean Range in 24 hours.	Monthly Extremes. Highest.	Lowest.	Greatest Range in 24 hours.	Least Range in 24 hours.
January, . .	29.440	29.581	29.474	29.594	29.457	29.587	0.176	0.181	0.357	30.260	28.410	0.730	0.060
February, .	29.740	29.858	29.795	29.951	29.767	29.889	0.109	0.124	0.233	30.530	28.730	0.650	0.010
March, . . .	30.217	30.331	30.203	30.310	30.212	30.320	0.060	0.072	0.130	30.590	29.500	0.400	0.020
April, . . .	29.094	29.187	29.071	29.161	29.082	29.174	0.075	0.088	0.163	30.280	29.430	0.490	0.020
May, . . .	29.773	29.872	29.782	29.878	29.777	29.875	0.087	0.066	0.153	30.250	29.140	0.420	0.000
June, . . .	29.415	29.508	29.761	29.854	29.603	29.681	0.078	0.085	0.163	30.000	29.300	0.400	0.040
July, . . .	29.650	29.743	29.664	29.760	29.657	29.751	0.087	0.076	0.163	30.090	29.120	0.420	0.030
August, . .	30.496	30.490	29.757	29.839	30.081	30.164	0.074	0.084	0.158	30.180	28.700	0.830	0.020
September, .	29.575	29.680	29.589	29.679	29.582	29.678	0.130	0.133	0.263	30.040	28.760	0.580	0.030
October, . .	29.863	29.967	29.859	29.952	29.861	29.964	0.103	0.097	0.200	30.490	29.000	0.680	0.020
November, .	29.434	29.563	29.459	29.564	29.416	29.563	0.125	0.192	0.317	30.300	28.460	0.870	0.020
December, .	30.026	30.167	30.055	30.166	30.040	30.166	0.129	0.099	0.228	30.590	28.930	0.570	0.020
Means, . . .	29.719	29.829	29.706	29.806	29.713	29.817	0.102	0.108	0.210				
1839, . . .	29.683	30.145	29.713	29.834	29.697	29.823	0.114	0.145	0.259				

THERMOMETER.

Months.	Mean of greatest Heat.	Mean of greatest Cold.	Mean Temp. of Morning.	Mean Temp. of Evening.	Mean of Extreme.	Mean of Morn. and Evening.	Mean of both.	Mean Range of 24 hours.	Monthly Extremes. Highest.	Lowest.	Greatest Range in 24 hours.	Least Range in 24 hours.	Highest at 9 A.M.	Lowest at 9 A.M.	Highest at 9 P.M.	Lowest at 9 P.M.	Temp. of Spring Water.
Jan.	42.00	34.30	37.10	38.00	38.10	37.50	37.80	7.70	49.50	19.50	21.50	3.00	45.50	23.00	47.00	25.00	43.30
Feb.	40.34	33.31	36.51	35.90	36.87	36.70	36.78	6.30	47.50	22.00	12.00	1.00	45.00	27.50	45.50	29.00	44.16
March,	46.70	32.70	39.00	39.00	39.70	39.60	39.35	13.60	58.00	28.00	31.50	1.50	45.50	30.00	46.00	30.00	45.30
April,	56.00	40.20	49.50	46.50	48.10	48.00	48.05	15.80	70.50	33.00	27.50	4.00	62.60	38.00	58.50	35.50	47.80
May,	58.50	43.10	52.50	49.40	50.80	50.90	50.85	15.80	75.00	35.00	31.00	4.00	66.60	40.00	61.00	41.50	47.70
June,	61.00	48.20	55.00	53.40	54.60	54.20	54.40	12.80	69.00	39.00	21.00	6.00	62.60	48.00	60.00	49.00	50.33
July,	61.10	49.40	56.30	53.60	55.20	54.90	55.05	11.80	66.00	40.50	21.50	5.50	62.50	50.50	60.50	49.00	52.00
Aug.	61.70	51.50	60.00	57.30	56.60	58.60	57.60	14.10	75.00	41.50	23.50	4.00	69.00	55.00	65.50	51.50	52.30
Sept.	56.60	44.40	50.90	49.50	50.50	50.20	50.30	12.30	65.00	35.00	22.00	4.50	61.00	42.50	58.00	43.00	50.90
Oct.	52.00	38.80	44.80	44.40	45.40	46.60	45.00	13.40	61.00	29.50	21.00	6.00	53.50	37.00	54.00	34.00	48.60
Nov.	45.80	37.40	40.20	41.70	41.60	40.90	41.20	5.90	53.50	23.50	18.50	3.50	49.50	25.00	48.00	28.50	45.60
Dec.	40.20	32.40	35.00	36.60	36.30	35.80	35.05	4.58	51.50	19.50	32.00	0.20	44.50	25.00	45.00	30.00	42.16
Means,	51.82	40.47	46.40	45.52	46.14	45.96	46.03	11.19									
1839.	50.98	39.80	46.16	44.95	45.30	45.55	45.47	11.02									

Dr Dunbar's *Meteorological Observations for* 1840.—(Continued).

WINDS—THEIR DIRECTION AND FORCE, AND WEATHER, STATED IN THE NUMBER OF DAYS IN WHICH EACH PREVAILED.

Months.	N.	N.N.E.	NE.	E.N.E.	E.	E.S.E.	SE.	S.S.E.	S.	S.S.W.	SW.	W.S.W.	W.	W.N.W.	NW.	N.N.W.	Calm.	Moder.	Brisk.	Strong.	Storm.	Sun shone.	Rain fell.	Snow.	Frost.	Thunder.	Rain in inches.
January,	1½	1	4½	—	1	—	3	2	3	2	5	3	4½	—	½	—	8	5	3	9	6	22	18	2	7	—	4.61
February,	—	1	3	2	9½	—	1	—	3½	—	7	1	1	—	—	—	14½	8	1½	2	2	25	13	2	10	—	2.62
March, .	4½	2	3½	3	2	—	—	1	4	1	—	2	2	2	3	1	15	9	5	2	—	29	5	1	17	—	0.15
April, .	1	—	½	2	3	1	½	2	7	1	7½	1	2½	—	1	—	15	8	3	3	1	29	6	4	4	—	0.28
May, . .	—	—	4	4	2	2	2	1	—	3	2	2	2	2	4	1	7	6½	7½	5	5	24	17	—	—	4	2.37
June, . .	1	—	2	—	—	—	—	—	2½	—	10	1	8½	1	2	2	10	7	9	1	3	27	20	—	—	1	2.37
July, . .	½	½	—	1	1	—	½	—	4	—	8	3	7	—	2½	3	12	12	4	2	1	29	22	—	—	3	5.28
August, .	—	—	1	—	—	1	4½	—	7	4	8½	—	—	—	5	—	12	11	5	3	—	27	15	—	—	1	3.69
September,	1	—	3	3	2	—	½	1	5	1	9½	2	1	—	1	—	6	12	5	5	2	28	21	—	—	1	3.54
October, .	3	—	2	—	5	—	—	—	3	—	4	—	5	1	7	1	10	16	3	—	2	28	10	—	2	2	1.17
November,	1	—	½	1	5	5	2	—	2	—	5	1	1	3	2½	1	14	7	3	3	3	21	16	—	6	—	3.26
December,	1	1	7½	1	4	3	3	1	1	—	3½	1	2	—	2	—	12	8	6	1	4	21	9	1	14	—	0.92
Total, .	14½	5½	31½	17	34½	12	17	8	42	12	70	17	36½	9	30½	9	135½	109½	55	36	30	310	172	10	60	12	30.26
1839, . . .	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	121	107	63	42	32	286	153	33	53	5	43.29

REMARKS.

The oscillations of the Barometer have been less frequent during 1840 than in the preceding year, as it appears that the mean daily range of the mercury in the latter was 0·259 of an inch, whereas in the former it has been only 0·210. The greatest height of the mercury was on the 8th of March, viz. 30·590; wind S.: lowest on the 24th January, when it was 28·410; wind S.SW.

The Thermometer stood highest on the 3d of May, viz. 75°; wind SE: lowest on the 7th of January and the 30th of December, viz. 19°·50; wind W. and SW. The mean of extremes differs from the mean of Morning and Evening less than two-tenths of a degree.

The Rain in 1840 is less than has fallen since 1832, and almost a third less than fell last year, though the number of days on which it rained was greater in 1840 than in 1839. The driest month in the year was March; the wettest, July.

Results of a Meteorological Journal, kept at Harraby, near Carlisle, for the year 1840. By Joseph Atkinson, Esq.

Barometer.

Mean height at 9 A. M.	29.861
Mean height at 9 P. M.	29.876
Mean height at both,	29.868
Highest A.M.—on the 8th March,	30.729
Lowest A.M.—on the 26th January,	28.506
Highest P.M.—on the 8th March,	30.705
Lowest P.M.—on the 13th November,	28.483

Thermometer.

Mean of maximum,	52.8
Mean of minimum,	43.3
Mean of both,	48.0
Highest—on the 7th August,	74.0
Lowest—on the 24th December,	21.5

Rain.

Total quantity,	32.095
Average total quantity for 24 years,	30.558
Number of days on which rain fell, 209, Snow, 18,	227
Average number of days for 24 years,	187

Wind.

Number of Days.

N.	12¼	E.	20¾	S.	19½	W.	42
NNE.	14½	ESE.	7	SSW.	24¾	WNW.	16
NE.	14½	SE.	20½	SW.	39¼	NW.	11½
ENE.	20¼	SSE.	22½	WSW.	70¼	NNW.	10¾

	DAYS.
Total easterly,	132¼
Total westerly,	233¾
Calm,	56
Moderate,	248¼
Breeze,	27¾
Strong breeze,	13¾
Stormy,	20

Weather.

Clear,	46½
Sun shone out,	288
Cloudy,	92½
Rain, 209, Snow, 18,	227
Frost,	49
Thunder,	9
Hail,	3

Description of several New or Rare Plants which have lately Flowered in the Neighbourhood of Edinburgh, and chiefly in the Royal Botanic Garden. By Dr Graham, Professor of Botany.

March 10. 1841.

Acacia setigera, *A. Cunningham.*

A. *setigera*; pilosa; ramis diffusis teretibus; phyllodiis ellipticis, obliquis, undulato-tortuosis, venosis, marginatis, marginis basi superiore uniglandulosis, apice in mucronem elongatum subulato-setiformem acuminatis; pedunculis axillaribus monocephalis (vel capitato-racemosis) phyllodio longioribus, foliis superioribus sub flores sæpe abortivis.—*Hooker.*

Acacia setigera, *All. Cunn.* in Herb. Hook.—*Hook.* Icones, 166.

Description.—*Stem* erect, round, green and pilose, but becoming brown and naked; branches diffused, round, green, pretty closely covered with rather harsh spreading colourless hairs. *Phyllodia* (1 inch long, 10 lines broad) on short petioles, crowded, ascending obliquely, bearing one edge up, glaucous, obovato-subrotund, undulate, coriaceous, marginate and ciliated, having a small sessile gland on the upper edge near the base, slightly hairy on both surfaces, somewhat unequally divided by the midrib, which is prolonged into an ascending subulate mucro; veins oblique and slightly reticulated, as well as the rib prominent on both sides. *Peduncles* (three-fourths of an inch long) generally single-headed, occasionally supporting several heads in a racemose manner, axillary and collected towards the branches, the phyllodia at their bases becoming gradually smaller, and at length, near the apex, being altogether awanting. *Capitula* about the size of large peas, dense, yellow, all, or nearly all, hermaphrodite. *Calyx* very small, brown, adpressed, 5-cleft, segments blunt. *Corolla* small, about five times as long as the calyx, 5-cleft, segments erect, elliptical. *Stamens* numerous, nearly three times as long as the corolla; filaments undulate; anthers of two small rounded lobes. *Pistil* longer than the stamens; germen green, elliptical, glabrous; style rather stouter than the filaments, placed obliquely upon the apex of the germen; stigma blunt and inconspicuous.

We received the seeds of this shrub, gathered on the banks of the Goulbourn River, from Dr Maclagan in 1838. It flowered in the greenhouse of the Botanic Garden in December last, and continued in blossom during January and February. I cannot doubt that it is the plant figured by Hooker, as quoted above; and I believe it is identical with a specimen in my herbarium from the late Mr Fraser, marked a tall pendulous shrub, observed in Oxley's second expedition in flower in August on all the barren lands north of the Arbuthnot range. Hooker's specimen is from sandstone ridges on the western branches of Hunter's River. In general, De Candolle's division of the leafless Acacias into three sections, according to their inflorescence, is found useful and easy of application. There are a few exceptions, which, like the present species, stand across one of the boundary lines. In general, the flowers are placed in solitary capitula; but, both in the cultivated plant and in my native specimen, there are also capitula arranged in a racemose manner along a common peduncle. This circumstance is not exhibited in Hooker's figure, nor noticed in his description. Sir William Hooker inadvertently published another species under the same name, Icones Pl. 316. This last is *Acacia Riceana*, Henslow, Botanist, t. 135.

Gardoquia betonicoides, *Lindl.*

G. *betonicoides;* radice repente, caule erecto, corollis calyce subglabro triplo longioribus, foliis ovato-cordatis grosse crenatis utrinque subglabris subtus purpurascentibus, cymis pedunculatis erectis, floribus congestis.

Gardoquia betonicoides, *Lindl.* Bot. Reg. in Misc. n. 159.—Bot. Mag. 3860.

Description.—*Root* creeping. *Stem* (in the specimen described) nearly 3 feet high. *Leaves* ($1\frac{1}{2}$ to $2\frac{1}{2}$ inches long, $1\frac{1}{4}$ to $1\frac{3}{4}$ broad) ovato-cordate, blunt, deeply crenate, glandular and subglabrous on both sides, at first green on both sides, soon becoming purplish below; middle rib and distantly reticulated veins prominent below, channelled above; *petiole* nearly as long as the leaf, channelled above. *Bracts* resembling much diminished, subsessile, subentire leaves; *bracteoles* linear, subulate. *Cymes* erect, distant at the lower part of the terminal pseudo-spikes, approaching higher up, with many crowded erect *flowers*. *Calyces* densely adpressed, subglabrous, subequal. *Corolla* (1 inch long) agreeably perfumed, somewhat spreading, thrice as long as the calyx; tube clavate, compressed laterally, grooved and veined, shortly glanduloso-pubescent, limb oblique, 5-cleft, the lowest lobe the largest, crenate and revolute, the others subequal, the two uppermost the flattest. *Stamens* didynamous; filaments adhering to the tube of the corolla to unequal heights, the two inner the farthest exserted, the others nearly as long as the upper lobes of the corolla; anthers dark, lobes parallel; pollen white. *Pistil* rather longer than the shorter stamens; style glabrous, and colourless, except at the apex, where it has a faint tinge, the same as the corolla, its lobes subulate, spreading, subequal. *Disk* small, round, fleshy supporting the abortive achænia.

The plant was received at the gardens of the Caledonian Horticultural Society, from Mr Low of Clapton in October 1839, and flowered freely during the summer and autumn following, both in the greenhouse and open border, requiring no particular treatment.

Helichrysum niveum, *Grah.*

H. *niveum;* perenne, caule erecto, subsimplice, scabro; capitulis maximis solitariis terminalibus, squamis utrinque niveis conniventibus ovatis mucronulatis; foliis oblongo-spathulatis, utrinque viridibus, pubescentibus, basi in petiolum angustatis, semiamplexicaulibus.

Helichryseum niveum, Bot. Mag. 3857.

Description.—*Stem* (above 3½ feet high) somewhat woody, erect, simple below, corymbose at the top, green. *Leaves* (7 inches long, nearly 2 broad) gradually smaller upwards, scattered, as well as the stem rough without pubescence, sessile, the lower ones obovate and much attenuated at the base, the upper more nearly elliptical, green on both sides, with a strong middle rib, and four to six principal veins extending nearly to the apex of the leaf, which is entire in the edges. *Capitula* terminal, on elongated, subsimple, corymbose branches. *Involucre* large, scariose, of snowy whiteness, spreading into a hemisphere; scales elliptical, very numerous, imbricated, the outer and inner smaller than the rest, concave, none of them radiating, the inner green at the base. *Florets* very numerous, yellow, cylindrical, all hermaphrodite, 5-toothed. *Stamens* inserted near the base of the corolla, the apices of the anthers subexserted. *Stigmata* revolute, truncated, hairy at the apices. *Style* colourless, filiform, its apex projecting above the stamens. *Germen* glabrous, crowned with a rough *pappus,* almost plumose, as long as the corolla. *Receptacle* naked, pitted.

This large and extremely ornamental species, was raised by Mr Low of Clapton, from seed sent from Swan River by Mr Drummond, late of Cork, and seedlings sent to the garden of the Caledonian Horticultural Society in October 1839, flowered very abundantly during July and August following, forming an exceedingly attractive border plant.

The species has proved to be perennial, pushing, in the year after flowering, many branches from the lower part of the stem. It has not ripened seed at Edinburgh. It has much affinity with the *H. macranthum* of Bentham, but is distinguished by its large capitula, pure white, cup-shaped involucre, and perennial root. It is in the highest degree worthy of cultivation.

Mirbelia speciosa, *Sieb.*

M. *speciosa;* foliis linearibus, mucronatis, aveniis, sparsis vel subverticellatis, marginibus revolutis integerrimis; floribus in spicam foliosam, subterminalem, interruptam, dispositis.

Mirbelia speciosa, *Sieb.* Pl. Exs. Nov. Holl. n. 367.—*De Cand.* Prodr. 2. 115.

Description.—*Shrub* branched, slender; branches elongated, bluntly angular, having scanty adpressed pubescence, grey and with many small green spots. *Leaves* scattered or subverticillate, linear, mucronate, spreading wide, having short erect petioles, veinless, dark green and broadly channelled above, paler below, and there with a few adpressed hairs upon the revolute entire edges, and on the strong middle rib which is slightly depressed along its centre; hairs produced at the base. *Flowers* in distant verticels towards the extremities of the branches, large, handsome, and solitary in the axils of scarcely diminished leaves, which are longer than them. *Peduncles* thrice as long as the petioles, somewhat silky, with adpressed hairs, and having at the apex two opposite adpressed subulate persisting bracts. *Calyx* silky, like the peduncle, bilabiate, upper lip bifid, segments ovato-subulate, reflexed; lower lip tripartite, segments spreading, longer, narrower, and more acute than those of the upper lip. *Corolla* of nearly uniform lilac; vexillum (9 lines across) three times as long as the calyx, reflexed, subrhomboid, notched in the centre, slightly striated, and having an obcordato-linear bright yellow spot towards its nearly white claw; alæ rather shorter than the vexillum, falcato-spathulate, edges vertical, nearly without tooth, claw short; keel half the length of the alæ, blunt, its petals cohering only in the middle, the claws longer than those of the alæ. *Stamens* included within the keel, free, the two upper occasionally abortive; filaments pale greenish, glabrous; anthers yellow, subrotund; pollen yellow, granules minute, oblong. *Pistil* rather shorter than the lower stamens, rather longer than the upper, everywhere glabrous; stigma capitate, verrucose; germen green; ovules about four.

This is a remarkably pretty species, and was raised at the Botanic Garden in 1836 from Australian seeds, which were obligingly given to me by Alex. G. Speirs, Esq. of Culcreuch, but I do not know from what part of the country the seeds were obtained. The plant has been kept in the greenhouse, with the usual treatment of Australian plants, and flowered for the first time, but then abundantly, in February and March 1841.

Siphocampylus revolutus, *Grah.*

S. *revolutus*; caule terete, erecto, parce ramoso, ramis villosis, flexuosis; foliis alternis, breviter petiolatis, rugosis, rigidis, cordato-ovatis, acuminatis, superioribus apice revolutis, supra scabrosis, subtus pubescentibus, marginibus reflexis, simpliciter dentatis; pedicellis folio brevioribus, tubo calycis turbinato-hemisphærico, lobis subulatis, divergentibus demidio breviore; corolla calycibus 5° longiore, externe villosiusculo, tubo base apiceque constricto, limbo subæqualiter 5-fido, lobis lanceolatis superioribus rectis, aliis reflexis, interne pilosiusculis.

Description.—*Stem* erect, round, sparingly branched; branches zigzag, villous, green. *Leaves* alternate, on short stout channelled petioles, rigid, wrinkled, spreading wide, dark green, and rough, with very short hairs above, lighter and villous below, cordato-ovate, acuminate, the apex of the upper ones revolute, the edges reflexed, simply dentate; middle rib and veins very prominent below, channelled above. *Peduncles* solitary, axillary, erect, half as long as the leaves, villous, without bracts. *Calyx* green, villous, tube turbinato-hemispherical, with ten strong ribs, and as many round glands between the apices of these; limb 5-partite, segments twice as long as the tube, subulate, diverging at the apex. *Corolla* five times as long as the calyx, purplish-red; tube entire, slightly deflexed, contracted at the throat, and for a space equal to the length of the calyx segments at the base, where it is deeply marked by five grooves, in the centre inflated and compressed laterally; limb 5-partite, segments subequal, linear-lanceolate, acute, slightly hairy within, the two upper straight and paler within, the lateral one spreading or reflexed, slightly falcate downwards, the lowest revolute, and, as well as the lateral ones, nearly white. *Stamens* as long as the corolla; filaments inserted along with this into the top of the calyx, adhering to the tube as far up as the extent of the contracted portion at the base, above this uniting into a tube, red and glabrous; anthers leaden-coloured, cohering into a curved tube, the two lower bearded with white hairs at the apex, the three upper having a very few similar hairs in the commissures. *Style* incased by the stamens, projecting beyond the anthers, glabrous, red; *stigma* of two blunt revolute lobes; *germen* inferior, green, glabrous, and with a free conical apex, bilocular; *ovules* very numerous, small, on large central placentæ.

Seedling plants of this species were received at the garden of the Caledonian Horticultural Society from Mr Low of Clapton in September 1839. They grew to the height of five feet last year in the stove without flowering; cuttings were formed, these rooted readily, and, when of a small size, flowered in February 1841. We possess the plant at the Botanic Garden, also from Mr Low. It is kept in the greenhouse, and is very healthy, but has not yet come into flower. Neither with us nor in the Society's Garden, has it received any particular soil or treatment.

This is a true *Siphocampylus*, and altogether unlike the plants known in cultivation, as *Siphocampylus bicolor* and *S. Cavanillesii*, which are true *Lobelias*.

Proceedings of the Royal Society of Edinburgh.

1840, *December* 7.—Sir T. M. Brisbane, Bart., President, in the Chair. The following communications were read:—

1. On certain Physiological inferences which may be drawn from the study of the Nerves of the Eyeball. By Dr Alison. Part First.

2. On the Plane and Angle of Polarization at the Surfaces of Crystals. By Professor Kelland.

December 21.—The Right Hon. Lord Greenock, V. P. in the Chair. The following Communications were read:—

1. On the Polarization of the Chemical Rays of Light, by Dr Sutherland, Liverpool. Communicated by the Secretary.

2. On the Nutrition of Vegetables, by Dr H. R. Madden, Penicuik. Part First. Communicated by Dr Christison.

The object of the author in this part of his investigation, is to shew that the portion of the food of plants which they receive from the soil, and which he endeavours to prove is chemically combined with it,—although to appearance generically the same in all soils,—is not composed, as some imagine, of one single proximate principle, the same in all circumstances, but consists of several principles varying in their respective proportions in different soils. And he farther attempts to establish the general proposition, that the varying proportion of these principles may be one great cause of the relative fitness of different kinds of soil for the cultivation or nourishment of different kinds of vegetables.

In the course of explaining these views, which were supported chiefly by speculative considerations, but which the author hopes to confirm by experimental researches in which he is now engaged, he had occasion to refer to the doctrine recently advanced by Liebig, that the relative fitness of different soils to different plants seems to depend, not on the organic matter contained in them, but in a great measure on their relative composition as to saline ingredients corresponding or not corresponding with the composition and amount of saline ingredients in plants. The author controverts this proposition, and endeavours to prove by reference to the composition of those soils in which wheat reciprocally thrives or languishes, that Liebig's doctrine is untenable. It is well known that a sandy soil, which, after one process of manuring, will raise in succession an excellent crop of turnips, barley, hay, and oats, nevertheless does not answer at all well for wheat;—which, on the contrary, produces most abundantly on a clayey soil. Liebig holds the cause of this difference to be, that in sandy soils there is not enough of the saline ingredients, more especially of potash-salts, which are essential to the constitution of wheat. The author proves, however, by calculations founded partly on experiments by Liebig himself, and partly on experimental researches of his own, that sandy soil, after being properly treated with farm-yard manure, not only contains a much larger amount of saline matters, including potash-salts, than is required for the constitution of a superior crop of wheat-straw and grain, but likewise, that it actually supplies three times the quantity of salts, and among these, three times the quantity of potash, required for a fine wheat crop, to the turnips, barley, hay, and oats successively raised on it, and nearly double the quantity of potash necessary for the wheat to the turnips alone. These facts will appear from the following table:—

Salts in an imperial acre of—

		Total Salts.	Potash.
	Wheat,	358.3 lb.	60. lb.
The crops of a rotation after a single application of manure, viz.:—	Turnips,	389.7 ...	92.4 ...
	Barley,	310.0 ...	40.0 ...
	Hay,	200.0 ...	20.0 ..
	Oats,	207.0 ...	20.0 ...
	Total,	1106.7...	172.4...

3. On the Fossil Fishes of the Old Red Sandstone of Orkney. By Dr Traill.

4. Mr Milne made a verbal communication respecting Instruments for registering Shocks of Earthquakes.

1841, *January* 4.—Dr Abercrombie, V.P. in the Chair. The following Communication was read :—

1. On certain Physiological Inferences which may be drawn from the study of the Nerves of the Eyeball. By Dr Alison. Part Second.

January 18.—The Right Hon. Lord Greenock, V.P. in the Chair. The following Communications were read :—

1. On the Mode in which Musket-balls and other Foreign Bodies become enclosed in the Ivory of the Tusk of the Elephant. By John Goodsir, Esq. Communicated by Professor Syme.

The author commenced by stating, that " in all the specimens he had examined, two circumstances were at once detected ; first, That the balls were enclosed, not in the true ivory, but in an abnormal structure ; and secondly, that the holes by which the balls entered were either partially or completely cicatrized in cases of wound of the socket; which led him to suppose that, as the tusk is an organ of double growth, the membrane of the follicle and the pulp both play important parts in the process of enclosure, and that there is no regeneration of true ivory,—an hypothesis which was afterwards verified by observation. From a consideration of the opinions of Camper, Blumenbach, Lawrence, and Cuvier, it appeared that doubts are entertained as to the existence of cicatrices after wounds of the tusk, and opinions held as to the impossibility of the occurrence of such phenomena in a non-vascular substance like ivory. To investigate this subject with success, two principles must be kept in view : first, that a tusk is formed from within outwards, as well as from without inwards ; and secondly, that the ivory and cement are never changed by vital action in form or substance, after their original deposition." With these two principles, the author proceeded to explain the healing of different wounds of the tusk, and the mode of enclosure of balls and other foreign bodies, by describing in detail the development and structure of the bony mass which appears in the pulp after wounds. He stated, 1. That wounds of the surface of the pulp are followed by ossification round the injury; 2. That the track of a ball across the pulp ossifies at the two extremities, but not necessarily in the rest of its extent; 3. That the track ossifies when abscess ensues, or when it becomes fistulous; and 4. That balls and foreign bodies are always enclosed in a mass of ossified pulp. This ossified pulp, when examined in thin sections under the microscope, presents a formation identical with the irregular ivory which fills the pulp cavity of the tusks of the walrus, and the tusk of the cetacea, and consists of anastomosing Haversian canals, secondary medullary canals, and wavy bundles of Retzian tubes. These canals and tubes are situate in a clear matrix, in which there are occasional patches of coarse cells, through the medium of which the bundles of Retzian tubes communicate with one another, and with the tubes of the regular ivory. The formation of the irregular ivory which surrounds wounds, abscesses, and foreign bodies in the pulp, does not proceed indefinitely, but is limited by the closing up of the orifices of the Haversian canals, and the consequent separation of their contained ramifying pulp from the general system. The irregular ivory is then, in reference to the general pulp of the tusk, in the same relation as the regular ivory, and at length becomes enclosed in the latter by the transformation of the pulp on its surface. It was then stated that foreign bodies enter the pulp in three ways; 1. Through the base of the pulp, without wounding the ivory; 2. Through the free portion of the ivory; and 3. Through the sides of the socket. A case of the first kind is described by Mr Combe in the Philosophical Transactions. Wounds of the second kind, when there is no trace of the track of the ball, have, with the exception of the formation of the irregular ivory, been sufficiently explained by former authors. In reference to wounds of the third kind, Mr Goodsir demonstrated that cicatrices, partial and complete, do occur, and that they are produced by the plugging up of the hole from within by irregular ivory or ossified pulp, and from without by cement formed by the membrane of the follicle. In conclusion, it was stated that every case of wound, fracture, and enclosure of foreign bodies in ivory, might be explained by the facts, that a tusk was an organ of double growth, and that its follicle played an important part in the healing of wounds through the socket.

2. On the Theory of Waves. Part II. By Professor Kelland.

3. Analysis of Berg-Meal from Umea Lapmark. By Dr Traill.

Professor Traill gave an account of the composition of a substance brought under the name of *Berg-Meal* from Swedish Lapmark by Mr Laing in 1838. It was found just under a bed of decayed mosses, forty miles above Degersfors, in Umea Lapmark. When examined by the microscope, it was found to consist of several species of minute organic remains, which Ehrenberg has considered as the siliceous skeletons of infusoria; the largest measured from 0.006 to 0.0005 of an inch. On analysis, Dr T. obtained 22 per cent. of organic matter, entirely destructible by a red heat; and he found the snow-white residue, which still retained the microscopic forms, to consist of 71.13 of silica, 5.31 alumina, and 0.15 oxide of iron. He considers the organic matter and the silica, as the essential ingredients, and the others probably as accidental. As a mixture with food, the quantity of organic matter in the Berg-Meal gives it a preference over the steatites and clays used for a similar purpose by some rude tribes.

February 1.—Dr Abercrombie, V. P. in the Chair. The following communications were read:—

1. On the force of Solar Radiation in the Arctic Regions, by Dr Richardson. Communicated by the Secretary.

2. An attempt to reconcile the Theories of the Debacle and the Action of Glaciers, in accounting for the Distribution of Erratic Blocks. By Sir G. S. Mackenzie, Bart.

The author commenced by alluding to the disposition of geologists to draw conclusions of a general nature too hastily from the facts observed by them, illustrating this remark by referring to the various opinions successively promulgated, not only by geologists generally, but even by the same geological writer, on the subject of the till or boulder clay, the gravel, and sand, by which Great Britain is every where more or less covered. At one period, all these superficial deposits were referred to the action of water alone; now it is the fashion to explain them by the agency of glaciers.

In treating of the appearances presented by these deposits, the author observes, that those which at first sight might be thought to indicate tranquil deposition from water, might in reality be due to a different cause. There is sometimes the semblance of stratification, which arises from an internal movement and segregation of the different matters in the mass. As a proof that this phenomenon does often occur, the author mentioned that, in the old ramparts of Tours formed originally of rubbish, he observed that in a part where they were cut across to form a road, the materials had so arranged themselves as to exhibit stratified beds.

The author does not offer any positive opinion as to the truth of the theory, which implies that the above-mentioned superficial deposits are due to the erosion and movement of glaciers covering the whole surface of the country. He mentions, however, one locality in Ross-shire, where there are appearances on the lateral rocks of a valley strongly indicative of glacial erosion.

On the supposition that there are phenomena which indicate the action of water as well as of ice, in the formation of these superficial deposits, the author states the view which occurred to him, for embracing both of these agents, to be as follows:—He supposes that a volcanic eruption took place in the Icy Sea, some where to the north-west of the British Islands, which had the effect of breaking up the ice along the coasts, and that icebergs or sheets of ice loaded with cargoes of boulders, clay, and gravel, were driven or floated over the British Islands, where they dropped their cargoes, and, in many instances, stranded on the hill tops. At this period, the author supposes that the relative levels of land and sea were very different from what they are at present, a great part of the British Islands being then submerged. This theory, the author stated, was not the result of much reflection or observation, and he merely threw it out for the consideration of geologists.

3. Contributions to Optical Meteorology. No. I. On the Polarization of the Light of the Sky. By Professor Forbes.

The author began by recapitulating the observations already made known on this subject.

The facts generally admitted (principally on the authority of M. Arago) appear to be, (1.) That a clear sky reflects light polarized in planes passing through the sun, the eye of the observer, and the point of the sky observed. (2.) That this polarization is a maximum in a zone 90° from the sun. (3.) That in the parts of the sky nearly opposite to the sun, this description ceases to be accurate; for the polarization, in a vertical plane passing through the sun and the observer, vanishes at an angle with the sun considerably less than 180°, —perhaps 150° or 160° (varying according to circumstances),— and gradually reappears in a plane perpendicular to the former, at greater angles than this. (4.) That the polarization is more intense in the neighbourhood of the horizon than of the zenith. (5.) M. Babinet has recently remarked that, under certain circumstances, there is a second neutral point in the neighbourhood of the sun.

Professor Forbes has verified these facts in nearly every particular, by the aid of a modification of Savart's polariscope, constructed of two plates of quartz, peculiarly cut and combined, together with Mr Nicol's single-image calc-spar prism, which the author has substituted with great advantage for the tourmaline commonly used in France.

With this instrument he finds, (1.) That a uniformly cloudy sky exhibits distinct traces of polarization. (2.) That rain-clouds generally polarize light; but not, so far as he has observed, those charged with snow. (3.) That the common rainbow *entirely* vanishes in one position of Nicol's prism (the fact of its polarization was discovered by Biot). (4.) That the polarization of moonlight reflected by the sky is very sensible, and likewise the diffuse light, or *burr*, which surrounds the moon in cloudy weather. (5.) That the light reflected from dry clear air, between the observer and objects a mile distant, is sensibly polarized.

With respect to the planes of polarization of skylight, he considers that they may be represented by a fiction of this kind: That there is a certain amount of polarization due to the regular reflection of sunlight from the sky in meridional planes passing through the sun and the observer; the polarization being most intense towards azimuth 90°, and vanishing at 0° and 180°. Combined with this polarization is another, distinct from it, and represented by a more intense effect due to reflection *parallel to the plane of the horizon* in all azimuths, which unites with, modifies, and even overpowers the regular polarization in meridional planes just referred to.

The result will be the composition of the effects of the reflection of light at a concave spherical surface having the sun for one pole, with that due to reflection at a cylindrical surface perpendicular to the horizon.

If the latter be tolerably uniform in all azimuths, it will evidently overpower and replace the former in points nearly opposite to the sun, and which become visible when the sun is low.

The author stated his conception of the physical cause for such an arrangement of the planes of polarization to be, that whilst, at considerable elevations, the number of reflecting particles is not so great as near the horizon, the effect due to a single reflection will be the less intense; and, consequently, the horizontal reflection is generally stronger than that in any other plane. But further than this, many familiar facts shew that the horizontal vapours (or opaque particles, of whatever kind they be, which occur in air), are, like a sheet of paper, capable of receiving light and of emitting it, not necessarily in the plane of reflection; and such light, after *several* reflections nearly parallel to the horizon and completely encircling it,—as we often see it do when a slight whitish haze is visible all round,—reaches the eye, much enfeebled, no doubt, by numerous reflections, but more intensely polarized on that account, and reinforced by the number of the reflecting particles. The lights thus irregularly reflected or emitted by the horizontal strata of air, and again regularly reflected by other particles in the same strata, will come to the eye more or less polarized in planes parallel to the horizon. A similar action will produce M. Babinet's second neutral point towards sunset; and Mr Forbes has remarked generally, after sunset, that the planes of polarization no longer converge accurately to the luminary, but are more or less twisted into a forced parallelism to the horizon.

February 15.—Dr Abercromby, V.P. in the Chair. The following communications were read:—

1. Farther Researches on the Voltaic Decomposition of Aqueous and Alcoholic Solutions. By Professor Connell.

2. On the Preparation of Paracyanogen, and the Isomerism of Cyanogen and Paracyanogen. By Samuel Brown, M.D. Communicated by Dr Christison.

After a short statement of the discovery of paracyanogen by Professor Johnston, and of its leading properties, the author proceeded to shew how, with certain precautions, cyanogen may be converted entirely, or nearly so, into the isomeric form paracyanogen. This he accomplished by exposing bicyanide of mercury suddenly to the temperature most favourable to the production of paracyanogen, which he found to be a low red heat, and employing also pressure, by confining the cyanogen gas which is at first expelled. By these means he succeeded in resolving the salt almost entirely into mercury and paracyanogen, the latter of which amounted in some trials to more than nine-tenths of the cyanogen contained in the bicyanide. The pressure required was not quite two atmospheres, namely 1.74.

The author further stated, that he had succeeded in proving that paracyanogen once formed cannot be again converted into cyanogen. Professor Johnston supposed the contrary, because he obtained cyanogen gas by exposing paracyanogen to a strong heat. But the author found that this arose from the latter having retained some cyanogen by absorption, that after the absorbed gas is removed heat subsequently expels nitrogen only, and that the same result is obtained from the first by using pure paracyanogen, prepared by dissolving the impure substance in concentrated sulphuric acid, and separating it in a state of purity by leaving the acid exposed to the air so as to attract humidity.

To these facts the author added some views as to the composition of cyanogen and paracyanogen, and their relation to one another. In order to account for the exceeding difference in properties prevailing between these two bodies, which appear nevertheless to consist of the same relative proportions of the same elements,—chemists generally consider the former to consist of one equivalent of carbon and two of nitrogen ($N\ C^2$) and the latter of two equivalents of the former and four of the latter ($N^2\ C^4$). But the author endeavours to shew that the true constitution of paracyanogen is that in which it is regarded as a compound of two equivalents of cyanogen, that is, of two " equal and similar atoms" of the same body.

In conclusion, the author proceeded to apply these views to the constitution of the simple or elementary bodies; and endeavoured to shew that there is nothing unreasonable in the supposition, that,—as chemists are now acquainted with various instances of compound bodies which have widely different forms, different physical properties in general, and different chemical relations, although agreeing exactly in their intimate constitution,—so, in like manner, some of those bodies, which are at present accounted elements distinct from one another, may really be isomeric, that is, different forms of one common element. And he stated that he hoped to be able to adduce experimental evidence of such being the fact with two of the most familiar of the elements, which until now have been considered wholly distinct."

3. A notice was communicated by Mr Mylne from Joseph Atkinson, Esq. of results obtained with Rain-Gauges of different forms.

Proceedings of the Wernerian Natural History Society.

(Continued from last Number, p. 215.)

January 9. 1841.—David Falconar, Esq. in the Chair. The Assistant-Secretary read a paper by Professor Gustav Bischof, of Bonn, on the employment of the Safety-Lamp in the Coal-Mines of Germany (published in this No. of the Journal, p. 378).

Mr Goodsir exhibited a series of preparations illustrative of the Anatomy of the Tunicated Mollusca, and demonstrated the arrangement and structure of the various systems of organs in this family, alluding particularly to the great vascularity of all the tissues, as displayed by minute injection. He directed the attention of the Society to one preparation in particular, consisting of the test of the *Phallusia vulgaris*, in which so many vessels were injected that it had assumed a bright vermilion tint, particularly in those places where stones, shells, or coral lines, were embedded in, or adhered to it. Baron Cuvier had pointed out the origin and trunks of these vessels, but the test itself has not generally been looked upon as a highly organized structure. These preparations are deposited in the Anatomical Museum of the University.

Notices were then read on traces of ancient glaciers in the south of Scotland, and on the occurrence of some remarkable fossil trees near Manchester.

January 23.—Professor Jameson, P. in the Chair. Mr Walter C. Trevelyan read an account of the habits of some tame Eels. In a small pond in a walled garden at Craigo, the seat of David Carnegie, Esq. near Montrose, these Eels have been kept for nine or ten years. They lie torpid during the whole winter, except the sun be shining bright, when they will occasionally come out from their hiding-place under some loose stones, and sprawl about the bottom of the pond, but refuse to take any food. The 26th April was the first day in 1840 that they rose for worms, but they eat sparingly until the warm weather begins, when they become quite insatiable; one of them will then swallow twenty-seven large worms one after the other. When they were first put into the pond, and had no food given to them, they devoured one another. They generally lie quietly at the bottom of the pond, except when any of the family go and look into it, when they instantly rise to the surface, sometimes for food, and at others merely to play with the hand or take the fingers in their mouths. About the month of August they become very restless, and take every opportunity of the pond overflowing from rain to get out; when sought for in the garden, on these occasions, they are invariably found travelling *eastwards* (the direction of the sea, which is about four miles from Craigo). Towards the end of August or beginning of September, they retire to their winter retreat under the stones. Whether they breed in this pond or not is uncertain, but on clearing it out last summer a few very small eels were discovered, and how else they could have found their way there is not easy to conjecture, as there is a fine rose on the mouth of the pipe by which the water enters. From their rapacity shewn in devouring their companions, some gold fish, it is possible they may eat the greater part of their own small fry.

A communication was made by Messrs Forbes and Goodsir on the Natural History and Anatomy of Thalassema and Echiurus (published in this No. of the Journal, p. 369).

Mr Forbes read a paper *on Kapnea, a new genus of Actiniadæ.* Mr Forbes constituted this genus for the reception of an actinia dredged up from deep water in the Irish Sea. The genus he defined: Kapnea (καπνη, a chimney), body cylindrical, invested in part by an 8-cleft epidermis, and adhering by a broad base. Tentacula simple, retractile, very short, tubercular, surrounding the mouth in three series. Species *Kapnea sanguinea,* Forbes. Tentacula 16 in each series; body and disk scarlet; epidermis brown; hab. among Millepora in deep water, Irish Sea. Mr Forbes considered the regular form of the epidermis as an imperfect tube, and remarked that the clefts in this tube and the number of the tentacula were multiples of four, which he considered as the typical number of the Actiniadæ.

The President laid on the table various Meteorological Registers; and exhibited a fine specimen of the Chimæra Fish from Cuba: of a Hen having the complete male plumage, and which laid several eggs after having undergone the change; and of a large Female Duck, species unknown, but allied to the eider, procured in the Edinburgh market.

February 20.—Dr Robert Hamilton, V. P. in the Chair. Dr Traill read a memoir of the life and writings of the Rev. George Low, minister of Birsay, author of the Fauna Orcadensis. Various very interesting original MS. volumes, which fortunately had been obtained from various sources, by the zeal and perseverance of Dr Traill, were exhibited; and these enabled the author to throw much light on the scientific history of the subject of the biography.

Mr Goodsir exhibited and described a new species of Gymnorhynchus (*Gymnorhynchus horridus,* Goodsir), from the liver of the Sun-fish. This species is characterized by an additional and separate circle of large curved hooks on each of the four proboscides. Many specimens were found alive and active, eight days after the death of the fish, and they were all enclosed in elongated sacs or sheaths, consisting of an outer cellular and an inner serous-like tunic. Mr Goodsir alluded at some length to the remarkable circumstances connected with the cysts enclosing many of the entozoa, and stated as his opinion, that, as in the present instance, certain of them could hardly be considered as the result of irritation. He hazarded the opinion that the internal smooth tunic might be a persistent portion of the ovum of the entozoa,—a hypothesis not consistent with the laws of development, or with any of the conditions of animal existence.

March 6.—Dr Hamilton, V. P. in the Chair. A communication was read by Dr Neill from Professor Fleming of King's College, Aberdeen, on a species of Raia new to the British Fauna, and illustrative drawings were exhibited.

SCIENTIFIC INTELLIGENCE.

GEOLOGY AND MINERALOGY.

1. *Dr Buckland on the Agency of Animalcules in the Formation of Limestone.*—At a recent meeting of the Ashmolean Society of Oxford, Dr Buckland read a paper on this subject. He began with exhibiting some polished thin slices of Stonesfield slate, lately presented to him by Mr Tenant, which Mr Darker has discovered to be crowded with microscopic shells; he also announced that Mr Darker and Mr Tenant have discovered microscopic shells to abound in thin slices of certain strata of Derbyshire limestone, and proceeded to discuss the question, how far the abundant occurrences of such remains in the carboniferous and oolitic limestones, and in the chalk and tertiary formations, justifies the revival, which has been attempted since the microscopic discoveries of Ehrenberg, of the old and false dogma—*omnis calx e vermibus, omnis silex e vermibus, omne ferrum e vermibus.* Dr B. exhibited the plates of Ehrenberg's work on the animalcular constitution of chalk, (1839) in which he has described and figured specimens from twelve localities in Europe, Asia, and Africa, all of which are crowded with foraminiferous and other minute chambered shells, varying in size from 1–24th to 1–200th of a line, so that a million may occur in a cubic inch of chalk. In specimens from the north of Europe the quantity of inorganic earthy chalk exceeds that of the organic bodies; but in specimens from the south of Europe the animal remains largely predominate. Ehrenberg has made out seventy-one species of these shells, some calcareous, some siliceous, including twenty-two species of microscopic nautilites, nummulites, and cyprides,

and forty species of infusoria; with these are a few confervæ and other minute vegetables. The chalk of the south of Europe is without flints, but full of siliceous infusoria, whilst in the chalk of northern Europe there are abundant flinty nodules, but no siliceous infusoria, except in these nodules, as if they had been attracted to the nascent nodules from the fluid in which they floated. Dr Buckland noticed also the recent discovery by Mr Bowerbank of spicula, and of organic cellular and tubular structure, which he refers to parasitic spunges in the black substance of chalk-flints, which often incloses also alcyonic bodies and shells. Admitting, with Professor Ehrenberg and Mr Bowerbank, the large contribution which animal remains have supplied to the substance both of chalk and flint, Dr Buckland would refer the earthy portions of the chalk and the inorganic substance of the flint to segregation from the waters in which both the lime and flint were held in solution. To a similar segregation from water he would likewise assign the origin of the calcareous earthy matrix which invests the calcareous exuviæ of molluscous and radiated animals in the shelly, the encrinal, and coralline limestones of the silurian, Devonian, and carboniferous series, and which is still more obviously loaded with organic remains in the forest marble and coralline beds of the oolite formation and also in the calcaire grossier, the crag, and fahluns of the tertiary series. Dr Buckland next shewed the relations of the recent nautilus, sepia, and velella to the minute molluscous constructors both of recent and fossil foraminiferous microscopic shells, and stated how much the modern discoveries of microscopic shells and infusoria have added to the amount of animal remains that are known to have contributed to the formation of limestone. He illustrated the extent to which molluscous animals occur in our present seas by Captain Beaver's discovery, that two shoals marked in the charts as sand-banks, between the Cape of Good Hope and Mauritius, in Lat. 34° 30′ S., Long. 27° 30′ E., are dense masses of small medusæ, floating in water more than 150 fathoms deep; and, by Captain Scoresby's calculation of the number of medusæ in a cubic foot of water in the Greenland seas, exceeding 100,000. He also stated that the luminous appearance of the sea in summer nights is due to the presence of myriads of minute molluscous animals. Dr Buckland next spoke of the microscopic animalcules which fill our stagnant ditches and lakes of fresh water; the red and green colour of the water of certain shallow ponds in summer is in some cases due to swarms of infusoria, invisible to the naked eye; some of these are figured and described in Shaw's Miscellany. Recent observations have found the sediments of the lake of Neufchatel to be full of infusoria; so also is the mud at the bottom of every ditch and shallow pool. From the surface of this mud, whilst dry in summer, the desiccated infusoria are raised by the wind into the air, where they become mixed with rain, and fog, and snow, in all of which the microscope of Ehrenberg has detected them; they float with the atoms of dust we see twinkling in a sunbeam, and return to life on falling into water or other fluids fitted for their resuscitation; they are propagated by eggs, and by subdivision of the bodies of individuals; from one individual sixteen millions have been produced in twelve days. Many of these infusorial animalcules have been noticed by former observers, and are figured in the plates of the Encyclopédie Méthodique, but they have not till lately been shewn to be largely connected with geology. The almost universal presence of infusoria in lakes and ponds explains the occurrence of a stratum of polishing stone (*Polier schiefer*), composed entirely of siliceous shields of infusoria, fourteen feet thick, and occupying the bottom of an ancient lake of great extent, at Bilin, in Bohemia. Other genera of infusoria, which secrete to themselves shells or shields of oxide of iron, have been discovered by Ehrenberg, in the marsh ochre that is formed annually in the ditches, and even in cow tracks, on the meadows near Bilin. The iron secreted from the water by each animal to form its shield becomes a nucleus, attracting other iron from the same water that supplied it to the animal, so that the iron-ore is partly of animal and partly of mineral origin. The siliceous infusoria in chalk-flints seem to have been attracted to the alcyonic and spongiform bodies which often constitute the nucleus of these flints, at the same time that these nuclei attracted also unorganised silex from the waters that held in solution both silex and carbonate of lime. We find both these earths in warm springs that issue from the volcanic rocks ; *e. g.* water charged with carbonate of lime is now issuing from the trap-rocks of Clermont, in Auvergne, and deposits of siliceous stone are daily accumulating around the orifice of the geyser, in Iceland. Recent discoveries of marine infusoria in the sea-water, co-existing with microscopic molluscs, lead us to infer from analogy the high probability that similar animalcules were not less abundant in the ancient seas. We may therefore expect to discover fossil infusoria by the application of the microscope to thin slices of all siliceous and calcareous sedimentary rocks, that contain any other kind of marine or fresh-water remains. In this extension of the application of the microscope from the living to the fossil infusoria and foraminifers, we are commencing a new and important era in palæontology, which will demonstrate a wonderful and very extensive, but by no means *exclusive*, agency of animalcules in the formation of limestone. In the case of crystalline marbles, it is probable that if any organic remains were ever contained in them, they have been obliterated by heat.

2. *Kilbrickenite.*—Kilbrickenite, as Dr Apjohn proposed to call a new mineral from Kilbricken lead-mine, county of Clare, is obviously what Berzelius denominates a sulphur-salt, *i. e.* a combination of an electro-negative with an electro-positive sulphuret. But there are several other ores known to mineralogists, composed of the same proximate constituents, or including sulphuret of lead in association with the sulphuret of antimony. The subjoined list comprehends those which have been analyzed and described :—

Zinkenite, . .	S, Pb + S_2, Sb.
Plagionite, .	4 (S, Pb) + 3 (S_3, Sb.)
Jamesonite, . .	[5 (S, Pb) + S_3, Pb_2] + 4 (S_3, Sb) ?
Feather ore of lead,	2 (S, Pb) + S_3, Sb.
Boulangerite, . .	3 (S, Pb) + S_3, Sb.

A mere inspection of the formulæ is sufficient to shew that each mineral in this list is distinct in composition from that whose analysis has been given above. There is, however, an ore possessing a constitution perfectly analogous to the Irish mineral, namely, the *Sprödglaserz* of Mohs and Werner, or what Professor Jameson calls brittle silver-glance. The formula of this mineral Rose has shewn to be 6 (S, Ag) + S з, S b; so that it differs from the Kilbrickenite merely in containing silver instead of lead.—*Proceedings of the Royal Irish Academy, No.* 24. 1840.

On the Composition of Pyrope.—Dr Apjohn lately made a brief verbal communication to the Royal Irish Academy on the subject of the composition of *Pyrope.* This mineral, long confounded with garnet, is known to be distinguished from it by containing chrome, and by exhibiting not the dodecahedral, but the hexahedral form. The best analyses of it, however, which are by Kobel and Wachmeister, are obviously imperfect, of which no better proof can be given than that Gustavus Rose in his Crystallography, does not attempt to give the formula of the mineral, but contents himself with enumerating the different oxides of which it is composed. Under these circumstances, Dr Apjohn conceived that a re-examination of the constitution of pyrope would not be without interest. He therefore undertook its analysis; and the result has been that he has detected in it yttria, one of the rarest of the earths; one, in fact, which had previously been known to exist only in a few minerals of exceeding scarcity.—*Proceedings of the Royal Irish Academy, No.* 26. 1840.

ZOOLOGY.

Fossil Dugongs.—M. de Blainville, in name of a commission composed of MM. Alexander Brongniart, Cordier, and himself, read a favourabe report on a memoir presented by M. Jules de Christol, of which he gave an analysis at the time of its presentation. The memoir is entitled, *Recherches sur divers ossements fossiles attribués par Cuvier à deux Phoques, au Lamantin, et à deux espèces d'Hippotames, et rapportés au Metaxytherium, nouveau genre de Cétacés de la famille des Dugongs.*

M. de Blainville began by making a few general remarks, which were nearly to the same purport as those which we extract from his report.

“ One of the most celebrated naturalists that flourished in the end of the last century and the beginning of the present, Blumenbach, has expressed the opinion, in his Archæology of the Earth, that palæontological geology presents some resemblance to the history of the generality of mankind, that is to say, that it must go through mythological and heroic periods before it reach what can be truly called the historical. Palæontology has unquestionably been in the last-mentioned condition for nearly a century. At the same time it is not to be denied that, even in recent times, works on palæontology have been too often executed under the influence of a fanciful illusion, which rendered it difficult to discern the truth, even though the processes of demonstration appeared exact and almost mathematical. It ought not, therefore, to be surprising that we still find a certain number of geologists who, influenced by a respectful confidence for observers, rather than guided by a profound and personal examination of their observations, admit and propagate, in whole or in part, inferences evidently false, yet which necessarily flow from the premises obtained by this mode of procedure. Thus, for example, it was for a long while difficult to avoid the belief that fossil animals were found in Europe which did not exist in a living state any where but in South America; and yet all the facts, as they came successively to be more carefully examined, were opposed to this opinion; and the recent discoveries of MM. Clausen and Lund in the caverns of Brazil tend likewise to confirm, in relation to the fossil bones of the two continents, the observation of Buffon, that the living species are never the same in both, with the exception of the more northern parts.

"At the same time," adds the reporter, "amidst this distracting diversity of opinion, a few liberal minds, keeping free from fanciful or interested impressions, which are unhappily too common for the real advancement of the sciences, have begun to reconsider the grounds on which a very considerable number of assertions regarding palæontology, have been made to rest. Admitting less than ever that a single part of a skeleton, even though selected with that view, can enable one to reconstruct the whole, that the dental system is in necessary accordance with the digital system, or with any other portion of the skeleton, that a single tooth is enough to establish a genus, or that size alone can characterize species; they have introduced into the distinctive attributes the age, sex, and conditions favourable, or otherwise, to existence, and consequently to development. Proceeding on these principles, it is easy to perceive that the limits of variation in the different pieces of a skeleton are much wider than was at first supposed. This fact is confirmed by daily observation, and becomes more and more obvious in proportion as our osteological collections extend, and are made more complete. The consequence is, that affirmations in reference to palæontology ought to be made in a less decisive manner, and they will thus become more exact, by resting on a more rational investigation. Then, by taking into consideration the conditions derived from the natural history of living animals, we may perceive that certain of them, not to say all, when we have respect to the harmony of creation, may be influenced more or less, in consequence of the material and intellectual development of man, to such a degree, that some have disappeared during the historical period, and almost from under our eyes, from certain countries, and even from the surface of the earth, as has taken place with the dronte, or the dodo, in the class of birds."

M. Jules de Christol is, perhaps, one of the first palæontologists who has studied the subject in this light. He has shewn, many years ago, that the fossil bones on which G. Cuvier established his *Hippopotame moyen*, did not belong to a hippopotamus, but rather to an animal of the genus lamantin, or dugongs, which likewise live, it is true, in water, and are herbivorous, but do not belong to the same natural family. M. de Christol's new memoir may be said to be a continuation of that of which we have already spoken, and still refers to the correction of an error which escaped G. Cuvier, respecting bones ascribed by him to a species of seals, carnivorous marine animals, and which M. de Christol supposes likewise to be those of a dugong. We shall not revert to what has been already said on this subject. We shall only add the following remarks with which M. de Blainville concludes his report.

" M. de Christol's researches give prominence to this fact, among others, that at times more or less remote from the present, all the gulfs of Europe into which large rivers discharge their waters, produced a more or less distinct species of this family (the dugongs) ; as there is still found one in countries to which civilization has not transported multitudes of human beings, who are necessarily destructive to the harmony of creation, for example, in the Indian Archipelago, in the Red Sea, where the dugong is found, or in the Gulf of Kamtschatka, to which the Lamantin de Stelles is exclusively confined. Thus, in the vast embouchure of the Rhone, in the Gulf of Lyons, there existed a species re-established by M. de Christol, perhaps the same as that which lived in the ocean of Homer, or in the Gulf of the Nile. For it appears that the bones found by M. Lefèvre, on the other side of Cairo, in a calcareous ridge, probably belong to a species of lamantin rather than of phoca. In the Gulf of the Pô, one was likewise found, the bones having been discovered in the sub-Apennine hills in the neighbourhood of Montiglio in Montferrat, and of which M. Bruno has formed a genus under the name of *Cheirotherium*. The Gulf of Gascony, at the wide embouchure of the Garonne, also possessed a species, perhaps the same as that in the vicinity of Angers or the Gulf of Ouest, as there exists a living species in the present day at the mouth of the great rivers of Northern Africa, and another almost opposite in the Gulf of the Amazon in South America. The Gulf of the Rhine produced the *Dinotherium*, which was likewise found in the other European gulfs; and, perhaps at that period, another species occurred in the Gulf of St Lawrence, for we cannot suppose that it exists there still, and has hitherto escaped observation, unless the mastodon which evidently forms the passage of the terrestrial gravigrades or elephants to the aquatic gravigrades or lamantins, be the representative of this family in North America."

M. de Blainville proposed that the Academy should express its approbation of M. de Christol's researches, in order to encourage him to farther exertions in this investigation, which was agreed to.

5. *The Migrations and Manners of Lemmings,* Mus Lemnus. L. Lemnus norvegicus, *Ray.* By M. Martins.*—Olaüs Magnus, archbishop of Upsal, is the most ancient author who speaks of Lemmings. According to him, Wormius has devoted a monograph to them, in which he makes an effort to prove that they fall from the clouds. We are indebted for many interesting

* Read to the Société Philomatique of Paris, and printed in the "Institut."

observations respecting them to Samuel Rheen, Sir Paul Ryeaut, Linnæus, Hoegstroem, Pallas, Fabricius, and Zetterstadt. None of these authors, however, with the exception of Hoegstroem, have been present to witness a migration. M. Bravais and myself have been more fortunate; and I shall here give a short account of our observations. Many of the members of the Northern Scientific Commission, among others, MM. Gaimard and Sundevall, traversed Lapland in September 1838; and they never fell in with a single lemming. The following year, at the same period, we saw them in myriads on the plateau of Lapland. At Bossecop (Lat. 70°) they were rather scarce, and again became so when we descended below the limit of the zone of the white birch. They again became very common in the neighbourhood of Karasuando on the banks of the Muonio, but on the right side of this river, a little below Muonioniska (Lat. 67° 55′), they were truly innumerable, and it was impossible to look around one without seeing a great number at once, and all running in the same direction parallel with the river. This, then, was the commencement of the migration; the army was on march. On the plateau, on the contrary, they ran about hither and thither, without inclining to any particular direction. When they descended lower into the plain, their ranks became closer. Linnæus says, "They trace rectilinear parallel furrows, from two to three inches deep, and many ells distant from each other. They devour every thing in their passage, plants and roots, and nothing turns them aside from their course. If a man stand in their way, they slip between his legs. If they meet with a rick of hay, they gnaw their way through it, and pass along. If a rock oppose their progress, they run round it, and resume their rectilinear course. When a lake occurs on their route they cross it in a right line, whatever may be its breadth, and that very often at its greatest diameter. Should a boat be lying in the line of their passage, they creep over it, and again throw themselves into the water on the other side. Even a rapid river cannot arrest them; they throw themselves into the current, although they should all perish in it." All these details are confirmed by others, and we are assured that in 1833 they ascended in the boats as far as Dupvig, near Bossecop, and to Hernoesand in Sweden. When not migrating, lemmings inhabit burrows, either simple or ramified at one or more of their openings, dug in the little mounds of earth so common in Lapland, and which are generally produced by the stump of a pine having become reduced to vegetable earth. Constantin Gloger enumerates only five species of the Linnean genus *Mus*, which construct nests. These are *Mus messorius*, *M. musculus*, *M. agrarius*, *M. sylvaticus, and M. minutus*. To this list it is necessary to add *M. lemnus*. Its nest is cylindrical, about 18 centimetres long, by 6 broad, more capacious below than above, with an opening at its anterior extremity. One of those we brought with us was composed of the leaves of a grass which could not be recognised, mingled with fragments of *Betula nana*, *Empetrum nigrum*, *Vaccinium Vitis-idea*, *Cenomyce rangiferina*, *C. pyxidata*, *Cladonia deformis, and Stereocaulon tomentosum*. Lemmings are very courageous; they try to defend themselves against every opponent by whistling and barking; and they fight with fury among themselves. When two of them are put into a cage together, it is necessary that one should yield to the other. Bears, foxes, wolves, martins, ermines, dogs, birds of prey, and even rein-deer, destroy them in great numbers. Their animal temperature is rather high; a medium of four observations afforded me + 39° 5″ Cent.

In relation to the statement made by M. Martins, that rein-deer sometimes feed on lemmings, M. Roulin mentioned that an analogous fact is observed among many other species of herbivorous mammifera, which appear capable of readily accommodating themselves to an animal nutriment when a vegetable one fails them. Thus, during the passage of a cloud of migratory locusts, we see fowls, sheep, and cows searching for them eagerly, on purpose to feed on them. Perhaps some of the epizootical diseases which ravage our flocks may be ascribed to this cause. It is known, moreover, that in New Holland, sheep, at a certain period, eat their young. M. Roulin likewise remarked, that the lemming is not the only species of mammifera which sometimes appears in innumerable multitudes, and he mentioned, among the animals of France, the campagnols of Beauce, the rats of Bretagne, &c. Many other animals, and locusts in particular, in like manner follow a straight line in their migrations. Finally, the great battles in which the lemmings engage during their travels, by no means constitute a fact peculiar to this species; the troops of dogs, which are used in certain countries for drawing sledges, present us with a new example of it. When one of these dogs is struck by the driver's whip, it often happens that he bites his neighbour; the latter attacks a third, and so on, till the battle becomes general. M. Roulin concluded his remarks by saying, that the great sensibility of lemmings to cold may well be considered one of the causes which determine the migrations of these animals.

Whale Fishery in the Northern Seas.—For many years of late, remarkable changes have taken place in the circumstances of this species of fishery, which every day become more and more perceptible. The whale regions are no longer the same as formerly. Originally, the seas situate between Spitzbergen and Greenland were frequented: these were afterwards totally abandoned. Whalers gave the preference to Davis' Straits, Baffin's Bay, or the seas to the east of Greenland. At the present time, Davis' Straits appear on the point of being deserted in their turn. France no longer sends any ships there; Holland sends only one or two; England, which formerly sent upwards of 200 ships to these regions, in 1832 sent only 81. The number is now much less, and will still doubtless continue to diminish, for the results of the last expedition were very unsatisfactory. It is probable that the English will, ere long, renounce the dangers of so laborious and difficult a navigation, in order to devote themselves, like the French and the Americans, to the fishery in the more temperate regions of the south.*

SCIENTIFIC MEETINGS.

7. *British Association.*—The meeting of the British Associa-

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* We are informed by our friend John Mitchell, Esq., Belgian-Consul, Leith, that this year there will not be more than seventeen ships altogether fitted out for Greenland and Davis' Straits, viz. nine or ten ships for Greenland from Peterhead, principally for seals; two from Kirkcaldy to Davis' Straits; one from Hull for do.; two from Newcastle for do.; and two from Dundee for do. There are only two from Holland, and there are none now from France.—EDIT.

tion takes place this year at Plymouth, and will occupy the week commencing Monday July 12.

8. *Congrès Scientifique de France.*—The ninth meeting of this Association, which, in its plan and objects, resembles the British Association for the Advancement of Science, will be held at Lyons, and will occupy twelve days. The session will open on Wednesday September 1. 1841, in the great hall of the Palais des Terreaux. The Association will be particularly gratified by the attendance of men attached to Science, Literature, and the Arts from the British Isles.

NEW PUBLICATIONS.

Three Lectures on Agriculture, delivered at Oxford during the year 1841, *in which the Chemical Operation of Manure is particularly considered, and the Scientific Principles explained upon which their efficacy appears to depend.* By Charles Daubeny, M.D., F.R.S., &c., Sibthorpian Professor of Rural Economy in the University of Oxford. London, John Murray; and Oxford, J. H. Parker. 8vo. Pp. 106. 1841.

The following is a summary of the contents of these interesting Lectures:—

Lecture I.—Value of scientific knowledge in agriculture.—Immense national gain accruing from the smallest agricultural improvement.—Uses of botany and vegetable physiology in agriculture.—Uses of chemical knowledge in the same—For determining the origin of the elementary principles which enter into the composition of vegetables—For the preservation of animal manures—For discriminating the mode of their action upon plants—For ascertaining the cause of the exhaustion of soils.—Other lines of research interesting to the agriculturist upon which chemistry may throw light.—Concluding remarks.

Lecture II.—On the scientific principles by which the application of manures ought to be regulated.—Plan of cultivation adopted by the first settlers in a new country at the present time.—Distinction between prairies and wood-lands.—Similar method of culture pursued by the early colonists of antiquity.—Fallowing, when first resorted to.—Manuring, first noticed by Homer.—Rotation of crops, how far practised by the Romans.—Improvements introduced in modern times.—Increased productiveness of the soil since the time of Queen Elizabeth, as shewn by the growth of population.—Imperfections of our present knowledge with respect to the principles of husbandry.—Source of the various elementary bodies which enter into the constitution of plants considered.—1. That of the earthy and alkaline matters which they contain.—Experiments by Saussure and Daubeny.—Inferences from these facts as to the impropriety of over-cropping.—Variation of the earthy and alkaline contents of a plant according to the nature of the soil.—Why strontian cannot be substituted for lime in the structure of a plant.—Limits to the power of substituting one earthy or alkaline body for another. 2. Source of the carbon.—In what manner vegetable mould supplies carbon to plants.—Conclusion that the carbon of plants is derived directly from carbonic acid alone.—The hydrogen in plants traced to the decomposition of water.—Origin of the nitrogen which plants contain in the ammonia present in the atmosphere.—Proofs of the universal presence of this alkali, both in air and water.—Proofs of its presence in the juices of plants.—Differences between the different bodies in the power of decomposing ammonia.—Distinction between restorative and exhausting crops founded on this difference.

Lecture III.—Practical inferences from the principles above laid down: 1. The utility of diligent and frequent tillage.—Subsoil ploughing.—Spade husbandry. 2. Uses of manures. 3. Proper succession of crops. 4. Selection of manures.—Speculations as to the primary source of the carbon and nitrogen present in plants and animals. This section is published in the present number of the Journal. Appendix: On the increase in the productive powers of the soil of Great Britain within the last century.

List of Patents granted for Scotland from 22*d December* 1840 *to* 19*th March* 1841.

1. To Robert Cooper of Pebworth, in the county of Gloucester, gentleman, "improvements in ploughs."—24th December 1840.

2. To Henry Trewhitt of Newcastle-on-Tyne, in the county of Northumberland, Esq. being a communication from abroad, "certain improvements in the fabrication of china and earthenware, and in the apparatus or machinery applicable thereto."—24th December 1840.

3. To Charles Parker of Darlington, in the county of Durham, flax-spinner, "improvements in looms for weaving linen and other fabrics, to be worked by hand, steam, water, or any other motive power."—24th December 1840.

4. To John Werthiemer of West Street, Finsbury Circus, in the city of London, printer, being a communication from abroad, "certain improvements in preserving animal and vegetable substances and liquids."—24th December 1840.

5. To Edmund Leach of Rochdale, in the county of Lancaster, machine-maker, "certain improvements in machinery or apparatus for carding, doubling and preparing wool, cotton, silk, flax, and other fibrous substances."—28th December 1840.

6. To William Hickling Burnett of Wharton Street, Bagnigge Wells

Road, in the county of Middlesex, gentleman, "improved machinery for cutting or working wood."—28th December 1840.

7. To John Grylls of Portsea, "improvements in the machinery used for raising and lowering weights."—31st December 1840.

8. To Samuel Brown of Hoxton in the county of Middlesex, civil engineer, "improvements in making casks and other vessels of or from iron or other metals."—31st December 1840.

9. To William Henry Bailey Webster of Ipswich in the county of Suffolk, surgeon in the Royal Navy, "improvements in preparing skins and other animal matters for the purposes of tanning and the manufacture of gelatine."—31st December 1840.

10. To Colin Macrae of Cornhill, Perthshire, Scotland, gentleman, being a communication from abroad, "improvements in rotary engines worked by steam, smoke, gases, or heated air, and in the modes of applying such engines to useful purposes."—31st December 1840.

11. To Moses Poole of Lincoln's Inn in the county of Middlesex, gentleman, "improvements in drying woollen and other fabrics."—31st December 1840.

12. To Thomas Clark of Wolverhampton in the county of Stafford, iron-founder, being a communication from abroad, "certain improvements in the construction of locks, latches, and such like fastenings, applicable for securing doors, gates, windows, shutters, and such like purposes."—6th January 1841.

13. To Hugh Unsworth of Blackrod, near Bolton, in the county of Lancaster, bleacher, "certain improvements in machinery or apparatus for mangling, drying, damping, and finishing woven goods or fabrics."—7th January 1841.

14. To Henry George Francis, Earl of Ducie, of Woodchester Park in the county of Gloucester, Richard Clyburn of Aley, engineer, and Edwin Budding, engineer, of Dursley in the same county, "certain improvements in machinery for cutting vegetable and other substances."—8th January 1841.

15. To Thomas Spencer of Liverpool in the county of Lancaster, carver and gilder, and John Wilson of Liverpool aforesaid, lecturer on chemistry, "certain improvements in the process or processes of manufacturing metallic cylinders, and for engraving thereon, and for engraving on metals generally."—9th January 1841.

16. To John Mason of Rochdale in the county of Lancaster, machinist, and Alexander Stiven of Manchester in the same county, engineer, "certain improvements in machinery or apparatus to be used for turning and boring purposes."—13th January 1841.

17. To William Hill Darker sen., and William Hill Darker jun., both of Lambeth in the county of Surrey, engineers, and William Wood of Wilton in the county of Wilts, carpet manufacturer, "certain improvements in looms for weaving."—18th January 1841.

18. To John Aitchison of Glasgow in Scotland, and Archibald Hastie of West Street, Finsbury Square, in the county of Middlesex, "certain improvements in generating and condensing steam, heating, cooling, and evaporating fluids."—20th January 1841.

19. To Joseph Haley of Manchester in the county of Lancaster, engineer, an "improved lifting jack for raising or removing heavy bodies, which is also applicable to the packing or compressing of goods and other substances."—21st January 1841.

20. To James Jamieson Cordes and Edward Locke of Newport in the county of Monmouth, "a new rotary engine."—28th January 1841.

21. To Samuel Hall of Basford in the county of Nottingham, civil engineer, "improvements in the combustion of fuel and smoke."—28th January 1841.

22. To John Dickinson of Bedford Row, Holborn, in the county of Middlesex, Esq., "certain improvements in the manufacture of paper."—1st February 1841.

23. To William M'Murray of Kinlieth Mill near Edinburgh, paper-maker, "certain improvements in the manufacture of paper."—3d February 1841.

24. To William Henson of Allen Street, Lambeth, in the county of Surrey, engineer, "improvements in machinery for making or producing certain fabrics with threads or yarns, applicable to various useful purposes."—4th February 1841.

25. To Nathaniel Lloyd, pattern-designer, and Henry Rowbotham calico-printer, both of Manchester in the county of Lancaster, "certain improvements in thickening and preparing colors for printing calicoes and other substances."—9th February 1841.

26. To James Maclellan of the city of Glasgow, manufacturer, "an improved combination of materials for umbrella and parasol cloth."—9th February 1841.

27. To John Clarke of Islington in the county of Lancaster, plumber and glazier, being a communication from abroad, and partly by invention of his own, "an hydraulic double action force and lift pump."—9th February 1841.

28. To Charles May of Ipswich in the county of Suffolk, engineer, of the firm of J. H. and A. Hansome & Co., "improvements in machinery for cutting and preparing straw, hay, and other vegetable matters."—12th February 1841.

29. To James Johnson of Glasgow in North Britain, gentleman, "certain improvements in machinery for the manufacture of frame-work knitting commonly called hosiery, and for certain improvements in such frame work knitting or hosiery."—15th February 1841.

30. To George Holworthy Palmer of Surrey Square, in the county of Surrey, civil engineer, and Charles Perkins of Mark Lane, in the city of London, merchant, "improved constructions of pistons and valves for retaining and discharging liquids, gases, and steam."—16th February 1841.

31. To Miles Berry, of the office for patents, 66 Chancery Lane, in the county of Middlesex, patent agent, being a communication from abroad, "certain improvements in looms for weaving."—20th February 1841.

32. To Moses Poole of Lincoln's Inn, in the county of Middlesex, gentleman, being a communication from abroad, "improvements in tanning."—22d February 1841.

33. To William Orme of Stourbridge, in the county of Worcester, ironmaster, "improvements in the manufacture of cofered spades and other cofered tools."—23d February 1841.

34. To William Pierce of Islington, in the county of Middlesex, gentleman, "certain improvements in the preparation of wool and other animal fibres, both in the raw and manufactured state, by means of which the quality will be considerably improved."—24th February 1841.

35. To Theophilus Richards of Birmingham, in the county of Warwick, merchant, being a communication from abroad, "improvements in cutting or sawing wood."—26th February 1841.

36. To Francis Sleddon junior, of Preston, in the county of Lancaster, machine-maker, "certain improvements in machinery, or apparatus for roving, slubbing, and spinning cotton, and other fibrous substances."—2d March 1841.

37. To Hugh Lee Pattinson of Bensham Grove, near Gateshead, in the county of Durham, manufacturing chemist, "improvements in the manufacture of white lead."—3d March 1841.

38. To Charles Cameron, Esq., lately captain in her Majesty's 18th regiment of foot, and presently residing at Mount Vernon, in the county of Edinburgh, "certain improvements in engines to be actuated by steam or other elastic fluids."—3d March 1841.

39. To Paul Hannuic of Paris, but now of Manchester, in the county of Lancaster, solicitor, being a communication from abroad, "certain improvements in the construction of governors or regulators applicable to steam-engines, and to other engines used for obtaining motive power."—3d March 1841.

40. To Charles de Bergue of Sydenham, Kent, gentleman, being a communication from abroad, "certain improvements in machinery for making reeds used in weaving."—3d March 1841.

41. To William King Westly of Leeds, in the county of York, flax machinist, "certain improvements in carding, combing, straightening, cleaning, and preparing for spinning hemp, flax, and other fibrous substances."—6th March 1841.

42. To Robert Urwin of South Shields, in the county of Durham, engineer, "certain improvements in steam engines."—9th March 1841.

43. To Walter Richardson of Regent Street, in the county of Middlesex, and George Mott Braithwaite, also of Chelsea in the county of Middlesex, gentleman, being a communication from abroad, "improvements in tinning metals."—11th March 1841.

44. To John Rand of Howland Street, in the county of Middlesex, gentleman, "improvements in preserving paints and other fluids."—16th March 1841.

45. To Thomas William Booker of Melin, Griffith works, near Cardiff, iron-master, "improvements in the manufacture of iron."—16th March 1841.

46. To Charles Edwards Amos of Great Guildford Street, in the Borough of Southwark, "certain improvements in machinery or apparatus used in the manufacture of paper."—18th March 1841.

47. To William Hancock junior, of King Square, Goswell Road, in the county of Middlesex, accountant, "an improved description of fabric suitable for making friction-gloves, horse-brushes, and other articles requiring rough surfaces, and the method of manufacturing the same."—19th March 1841.

48. To Frederick Steiner of Hyndburn Cottage, near Accrington, in the county of Lancaster, Turkey-red dyer, being a communication from abroad, "improvements in looms for weaving, and cutting asunder double [piled] cloths, and a machine for winding weft to be used therein."—19th March 1841.

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