blem which she can solve, if she is allowed to make the attempt. The gradation in form between man and other animals, a gradation which we all recognise, and which, therefore, need not startle us because it is presented under a new aspect, is but a slight and, as appears to me, unimportant feature, in looking at the great subject of man's origin. Even if we had no Divine record to guide us, it would be most unphilosophical to attempt to trace back the history of man without taking into account the most remarkable facts in his nature: the facts of civilization, art, government, writing, speech—his traditions—his internal wants—his intellectual, moral, and religious constitution. If we will look backwards, we must look at all these things as evidences of the origin and end of man's being. When we do thus comprehend in our view the whole of the case, it is impossible for us, as I have elsewhere said, to arrive at an origin homogeneous with the present state of things; and on such a subject the geologist may be well content to close his own volume, and open one which has man's moral and religious nature for its subject.

In order to complete the notice of the contributions to foreign geology, I must mention Mr. Roy's account of Upper Canada: in which country he conceives that he has detected terraces which exhibit the beaches of the lakes when the level of their surface was more elevated than they are at present. I must refer also to Mr. Bollaert's paper on alluvial accumulations containing large masses of silver ore in Peru. And, finally, I have to direct your attention to the very curious information respecting the geology of South America, which we have received from Mr. Darwin. In a communication made to us, he gave a very striking view of the structure of a large portion of that continent; and, as I have already had occasion to observe, he has brought to this country the remains of various fossil animals of entirely new kinds, of exceeding interest to the zoologist as well as the geologist. I need only remind you of the gigantic mammifer which has been reconstructed in idea by Mr. Owen, upon the evidence of a fossil skull, and has been named by him the *Toxodon Platensis*. This animal, although a *Rodent*, according to its dental characters, in other respects manifests an affinity to the *Pachyderms*; and also to the *Dinotherium*, and to the *cetaceous* order. Many other fossil animals have been discovered in South

America; and all, from their magnitude, fitted to excite our wonder, when we compare the diminutive size of the present races of animals which inhabit that country. The animal remains found by Mr. Darwin comprise, besides the Toxodon, which extraordinary animal was as large as a hippopotamus,—(2, 3, 4, 5, 6.) the Megatherium, and four or five other large Edentata;—(7.) an immense Mastodon; —(8.) the Horse;—(9.) an animal larger than a horse, and of very singular character, of which a fragment of the head has been found; —(10, 11, 12.) parts of Rodents, one of considerable size;—(13.) a Llama, or *Guanaco*, fully as large as the Camel.

But I should very ill convey my impression of the great value of the researches of Mr. Darwin, by any enumeration of special points of geology or palæontology on which they have thrown light. Looking at the general mass of his results, the account of which he has been kind enough to place in my hands, I cannot help considering his voyage round the world as one of the most important events for geology which has occurred for many years. We may think ourselves fortunate that Capt. Fitz Roy, who conducted the expedition, was led, by his enlightened zeal for science, to take out a naturalist with him. And we have further reason to rejoice that this lot fell to a gentleman like Mr. Darwin, who possessed the genuine spirit and zeal, as well as knowledge of a naturalist; who had pursued the studies which fitted him for this employment, under the friendly guidance of Dr. Grant at Edinburgh, and Professor Henslow and Professor Sedgwick at Cambridge; and whose powers of reason and application had been braced and disciplined by the other studies of the University of which the latter two gentlemen are such distinguished ornaments. But some of the principal of these results may be most conveniently mentioned, when we pass from mere descriptive geology, to that other division of the subject which I have termed Geological Dynamics. And this I now proceed to do.

GEOLOGICAL DYNAMICS.

This term is intended to express generally the science, so far as we can frame a science, of the causes of change by which geological phænomena have been produced. Without here speaking of any classification of such changes, I may observe that the gradual eleva-

tion and depression, through long ages, of large portions of the earth's crust, is a proximate cause by which such phænomena have been explained: and this class of events, its evidence, extent, and consequence, is brought before our view by Mr. Darwin's investigations, with a clearness and force which has, I think I may say, filled all of us with admiration. I may refer especially to his views respecting the history of coral isles. Those vast tracts of the Pacific which contain, along with small portions of scattered land, innumerable long reefs and small circles of coral, had hitherto been full of problems, of which no satisfactory solution could be found. For how could we explain the strange forms of these reefs; their long and winding lines; their parallelism to the shores? and by what means did the animals, which can only work near the surface, build up a fabric which has its foundations in the deepest abysses of ocean? To these questions Mr. Darwin replies, that all these circumstances, the linear or annular form, their reference to the boundary of the land, the clusters of little islands occupying so small a portion of the sea, and, above all, the existence of the solid coral at the bottom of deep seas, point out to us that the bottom of the sea has descended slowly and gradually, carrying with it both land and corals; while the animals of the latter are constantly employed in building to the surface, and thus mark the shores of submerged lands, of which the summits may or may not remain extant above the waters. I need not here further state Mr. Darwin's views, or explain how corals, which when the level is permanent fringe the shore to the depth of twenty fathoms, as the land gradually sinks, become successively encircling reefs at a distance from the shore; or barrier reefs at a still greater distance and depth; or when the circuit is small, lagoon islands:—how, again, the same corals, when the land rises, are carried into elevated situations, where they remain as evidences of the elevation. We have had placed before us the map, in which Mr. Darwin has, upon evidence of this kind, divided the surface of the Southern Pacific and Indian oceans into vast bands of alternate elevation and depression; and we have seen the remarkable confirmation of his views in the observation that active volcanos occur only in the areas of elevation. Guided by the principles which he learned from my distinguished predecessor in this chair, Mr. Darwin has presented this subject under an aspect which cannot but have the most powerful

influence on the speculations concerning the history of our globe, to which you, gentlemen, may hereafter be led. I might say the same of the large and philosophical views which you will find illustrated in his work, on the laws of change of climate, of diffusion, duration and extinction of species, and other great problems of our science which this voyage has suggested. I know that I only express your feeling when I say, that we look with impatience to the period when this portion of the results of Captain Fitz Roy's voyage shall be published, as the scientific world in general looks eagerly for the whole record of that important expedition.

And I cannot omit this occasion of mentioning with great gratification, the liberal assistance which the Government of this country have lent to the publication of the discoveries in natural history which Mr. Darwin's voyage has produced. The new animals which he has to make known to the world will thus come before the public described by the most eminent naturalists, and represented in a manner worthy of the subject and of the nation. I am sure that I may express the gratitude of the scientific world, as well as my own, for this enlightened and judicious measure.

I may here notice Mr. Darwin's opinion, so ably exposed in a paper read before us, that the change by which a variety of materials thrown on the earth's surface become vegetable mould, is produced by the digestive process of the common earth worm.

I will here also advert to Mr. Fox's paper on the process by which mineral veins have been filled up. This he conceives might be produced by the circulation or ascension of currents of heated water from the deeper parts of the original fissures. The discovery of the causes of the formation and filling of metallic veins, one of the earliest subjects of geological speculation, will remain probably as a problem for its later stages, when our insight into the laws of slow chemical changes is far clearer than it is at the present day.

If, from these proximate causes of change of which I have spoken, we proceed to those ulterior causes by which such events as these are produced;—to the subterraneous machinery by which islands and continents appear and vanish in the great drama of the world's physical history;—we have before us questions still more obscure, but questions which we must ask and answer in order to entitle ourselves to look with any hope towards geological theory. Of late

ADDRESS

DELIVERED AT

THE ANNIVERSARY MEETING

OF THE

GEOLOGICAL SOCIETY OF LONDON,

On the 16*th of* FEBRUARY 1838;

AND

THE ANNOUNCEMENT OF THE AWARD

OF THE

WOLLASTON MEDAL AND DONATION FUND

FOR THE SAME YEAR.

BY THE REV. WILLIAM WHEWELL, M.A., F.R.S.,

PRESIDENT OF THE SOCIETY.

LONDON:

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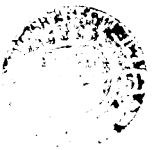
1838.



AWARD

OF THE

WOLLASTON MEDAL AND DONATION FUND

FOR 1838.

(Extracted from the Proceedings of the Geological Society, Feb. 1838.)

The Wollaston Medal was awarded to Mr. Richard Owen for his services to Fossil Zoology in general, and in particular, for the part already published of his Description of the Fossil Remains collected by Mr. Darwin in the voyage of H.M.S. Beagle.

In delivering the Medal to Mr. Owen, the President addressed him as follows:

Mr. Owen,

I have peculiar pleasure in presenting to you this Medal, awarded to you by this Society for your services to Fossil Zoology in general, and, in particular, for the description of the Fossil Mammalia collected by Mr. Darwin. I trust it will be a satisfaction to you to receive this our testimony of the success with which you have cultivated that great science of comparative zoology, to which you have devoted your powers. I trust it will add to your satisfaction to consider that the subject which we more peculiarly wish to mark on this occasion, in the study of Fossil Zoology, is one to which the resources of your science were applied, while the subject was yet new, by that great man, John Hunter, whose Museum and whose reputation are so worthily assigned to your care. I trust also that this Medal thus awarded to you at the outset, if I may so say, of an enlarged series of investigations, will convey to you the assurance that, in your progress in such researches, you carry with you our strong interest in your en-

deavours, and our high esteem of your powers and your objects; and will convince you that in all your successes, you may reckon upon our most cordial sympathy in the pleasure which your discoveries give.

Mr. Owen acknowledged his sense of the distinction conferred upon him in the following terms:—

I wish, Sir, that I had words adequately to express my deep sense of the honour I have now received; but I feel assured that you will grant to me the sincerity of my brief acknowledgments. The study of the animal organization has always abundantly repaid me by the pleasure which naturally flows from the contemplation of the marvellous skill with which, in the complete frame of existing species, structures are modified and designed in relation to particular ends; and from the perception of a subordination of the various instruments to one general plan. But since I have pursued anatomical investigations in connection with fossil remains, I have been rewarded by new and extrinsic pleasures. I trace to this source my connexion with the Geological Society, and the possession of some most valued friendships; and now, Sir, my obligations to the Society, and to Palæontology are increased ten-fold by the unexpected honour I have this day received at your hands. I receive this testimony of your good opinion as a strong stimulus to future endeavours. I cannot permit myself to regard it as a reward for the inadequate contributions which I have hitherto been able to make to the history of lost species; and I pledge myself to lose no available time or opportunity which may be applied to further that branch of geological science which has the extinct animals of this planet for its immediate object.

ADDRESS

TO THE

GEOLOGICAL SOCIETY,

Delivered at the Anniversary, on the 16th February 1838,

BY THE REV. WILLIAM WHEWELL, M.A. F.R.S.,

PRESIDENT OF THE SOCIETY.

GENTLEMEN,

YOU have heard in the Reports just read, statements which show that the Society is in a state of healthy progress both in respect to its numbers and its funds. The total number of Fellows of the Society, exclusive of Honorary and Foreign Members, at the close of the year 1836, was 709. At the close of the last year it was 738, the increase being 29, after deducting 18 Members deceased or resigned.

A Part of the Transactions has recently been published, which is worthy of its predecessors in the interest of its matter, and which is not inferior to them in its appearance and illustrations. I believe it will be found that improvements have been introduced, especially in the colouring of the maps.

Our collections have also gone on increasing, and have, as in previous years, derived great additional value from the labour and knowledge bestowed upon them by our excellent Curator. But your Council has found itself compelled to attend to the great, and I may say intolerable amount of labour which has fallen upon Mr. Lonsdale, and certain alterations in the Society's arrangements, directed to the object of remedying this evil, are now in progress or in contemplation.

When they are completed I shall have the satisfaction of announcing them to the Society.

The Council have awarded the Wollaston Medal, as you have already been informed, to Mr. Richard Owen, for his general services to Fossil Zoology, and especially for his labours employed upon the fossil mammalia collected by Mr. Darwin in the voyage of Captain Fitz Roy. I need not remind you, Gentlemen, how close are the ties which connect the study of living and of fossil animals; how much light the progress of comparative anatomy throws upon the interpretation of geological characters; and what important steps in our knowledge of the past condition of the earth are restorations of the animal forms which peopled its surface in former times, but have long vanished away. Since the immortal Cuvier breathed into our science a new principle of life, the value of such researches has ever been duly appreciated; and the award of the Wollaston Medal last year is an evidence how gladly your Council take that method of congratulating the successful cultivators of such studies. I am sure that all who are acquainted with Mr. Owen's labours will rejoice that we have in this manner marked our sense of his success. His earlier researches, those for instance on the Nautilus, have been of exceeding use and interest to geologists. And the first part of his description of the fossil mammalia, collected by Mr. Darwin in South America, contains matters of the most striking novelty, interest, and importance. We have there the restoration, performed with a consummate skill, such as fitly marks the worthy successor of Hunter and the disciple of Cuvier, of two animals, not only of new genera, but occupying places in the series of animal forms, which are peculiarly instructive. For the one, the Toxodon, connects the Rodentia with the Pachydermata by manifest links, and with the Cetacea by more remote resemblances; and thus contributes to the completion of the zoological scale just in the parts where it is weakest and most imperfect: while the other animal, the Macrauchenia, the determination of which is considered by anatomists as an admirable example of the solution of such a problem, appears to be exactly intermediate between the horse and the camel. But this creature is also interesting in another way, since it closely resembles, although on a gigantic scale, an animal still existing in that country and peculiar to it, the Llama. Thus, in this as in some other instances, the types of animal forms which distinguish a certain

region on the earth's surface are clearly reflected to our eyes as we gaze into the past ages of the earth's history, while yet they are magnified so as to assume what almost appear supernatural dimensions. The Llama, the Capybara, and the Armadillo of South America are seen in colossal forms in the Macrauchenia, the Toxodon, and the Megatherium. I will not omit this occasion of stating that the profound and enlarged speculations on the diffusion, preservation, and extinction of races of animals to which Mr. Darwin has been led by the remains which he has brought home, give great additional value to the treasures which he has collected, and make it proper to offer our congratulations to him, along with Mr. Owen, on the splendid results to which his expedition has led and is likely to lead. Mr. Owen and Mr. Darwin are engaged in the restoration of other animals from the South American remains in their possession, and I am able to announce that two or three other new genera have already been detected. I am sure I am conveying your feeling, Gentlemen, as well as my own, when I express a cordial hope that these two naturalists, so fitted by their endowments and character to advance the progress of science, may long go on achieving new triumphs; and may have the satisfaction—higher even than that which they derive from the honours we so willingly bestow—of finding the great principles which it is given to them to wield, becoming every year more powerful instruments of discovery; and of seeing, as they pursue their researches, light thrown upon the darkest and widest of the vast problems which they have proposed to themselves.

I will now say a few words concerning a few of the most conspicuous of the names which have been obliterated by death from our list during the year.

Among the members of our body, whom we have lost, there is one whom we cannot but mention with more than common emotion, endeared as he was to many of us by private friendship, and admired by all for his talents, his knowledge, and his services. Dr. Edward Turner, Professor of Chemistry in the London University, filled the office of our Secretary for five years, and subsequently was two years Vice-President, which situation he held at the time of his death in February 1837. Several of you may remember, Gentlemen, that our last anniversary meeting was in some measure clouded by the recollection of this then recent calamity; and that many of

the Fellows of the Society, on that occasion, expressed their intention of testifying their respect and regard for the departed by attending his funeral. Of Dr. Turner's private virtues, and of the charm of his society, I must not here speak. I will not allow myself to dwell upon the admirable clearness and precision of his thoughts as expressed in conversation,—upon the delightful openness and candour of his character,—upon the kind and gentle cheerfulness of his demeanour, the genuine fruit of a deep habitual religious feeling. But I may take this occasion to say, that in him chemistry suffered a loss, not only great,—for that all would at once say,—but much greater and more difficult to repair than may at first sight appear. Dr. Turner entertained a conviction (I am stating the result of many interesting conversations which I have held with him) that the time was come when the chemist could not hope to follow out the fortunes of his science, and to read in her discoveries their full meaning, without being acquainted with the language, and master of the resources of mathematics. Acting upon this enlightened view, he did not hesitate to encounter the great labour and exertion of a course of study in the higher mathematics; and he succeeded entirely in making himself a good mathematician. And he was one of the very few who, in our country, labour at a branch of chemistry which is of the highest importance to us as geologists; but which,—we may suppose from its laborious and intricate nature,—appears to repel our most active chemists; I mean that portion of chemistry which is connected with mineralogy.

Yet this department is, in truth, more inviting than it may at first appear. No doubt in it clear mathematical conceptions are necessary, and perhaps some little training in mathematics; but there is good promise that the labour which this line of investigation demands will be rewarded. I am fully persuaded that there is no portion of the frontier line of our knowledge of which we can so certainly say, "Here we are on the brink of great discoveries." Had Dr. Turner been spared to us some years longer, I know no one who was more likely to have had a principal share in such discoveries. Two papers of his, in the Philosophical Transactions,* show that he was able to

* On the Composition of Chloride of Barium, 1829; Researches on Atomic Weights, 1833.

deal with the atomic theory in a mode which combines the resources of the skilful analytical chemist with the rigour of the mathematical reasoner; a combination which the right prosecution of that theory requires, but which has not always been found in its cultivators.

Dr. Turner lectured on chemistry at the London University from its first foundation in 1828; he was there surrounded by students, whose affection he gained by his kindness, as well as their admiration by the clearness of his teaching. He also gave a course of lectures on geology, in conjunction with Dr. Grant and Mr. Lindley, each of those gentlemen taking a division of the subject with which he was most familiar. Dr. Turner was snatched from science at the early age of thirty-nine, having been born in the island of Jamaica in 1796. He studied anatomy at Edinburgh, and chemistry at Göttingen, under the able chemist Friedrich Von Stromeyer, to whom he dedicated his Elements of Chemistry; a work which has had, as it well deserves, a very wide circulation among students.

In William Farish, B.D., Jacksonian Professor of Natural and Experimental Philosophy in the University of Cambridge, the Society has lost an honorary member, elected as such soon after its original foundation, namely in November 1808, and one of a number of our countrymen who were at that period placed upon the honorary list. Professor Farish never employed himself peculiarly in geological pursuits as we now understand the term; but it is to be recollected, that within a few years of the date of his election, which I have mentioned, the investigation of the earth's structure made a rapid progress, and, in consequence, assumed a more fixed and technical form. Professor Farish's scientific studies were mainly directed to the arts, manufactures, and machinery of the empire; on these subjects he delivered courses of lectures full of interest and instruction; and he was thus led to describe our mines, and the mode of working them.

But no reference to particular portions of Professor Farish's labours can convey a just notion of the impulse which he gave to the progress of scientific knowledge within his own sphere of influence, by the habit of seizing, with an active and vivid apprehension, upon prominent parts of modern science, and conveying them, in a manner singularly clear and simple, to his audience. For a long course of years his lectures were more efficacious than any other circumstance in stimulating the minds of men in his university to philosophical thought on physical

subjects; and to this day these lectures are never mentioned by those who attended them at that period, without admiration and pleasure. His merit was well recognised by the university in which he spent his life. He received the highest mathematical honours of that body on taking his degree of B.A. in 1778, was elected Professor of Chemistry in 1794, and Jacksonian Professor in 1813; and at the institution of the Cambridge Philosophical Society in Nov. 1819, he was its first president.

I cannot refrain from adding, that although I have here to speak of him principally as a man of science, such pursuits were in his case little more than episodes, in a life the main action of which was directed to the ends of religion and benevolence. In his duties, as a minister of Christianity, he was most zealous and indefatigable; and every attempt to relieve the misery, the ignorance, the unjust restraints of any portion of mankind, found in him a strenuous advocate and ready agent. His childlike simplicity, genuine kindness of heart, and untiring religious earnestness were such as well suited his kindred with Bernard Gilpin, "the Apostle of the North," from whom, through his mother, he derived his descent. He was born at Carlisle in 1759, and died at the age of 78.

Henry Thomas Colebrooke, member of the Supreme Council of Calcutta, was one of those extraordinary men whom our Indian empire has produced; and who show the animating effects of the great scene in which they are there placed, by the variety of subjects to which they extend their attention, and by the vigour with which they combine speculative and practical employment. Mr. Colebrooke went to India as a writer in 1782, and about 1792 began to attend peculiarly to Sanscrit literature. A little later we find him beginning to enrich the Asiatic Researches with a series of memoirs on the religion, the literature, and, above all, the science of the Hindoos. In this department his labours on the Zodiac of the Indians*, and on their notions of the Precession of the Equinoxes and the motions of the Planets†, are highly deserving of notice; as were at a later period the account of the Indian Algebra, given in his translations of the Lilawati and Vijaganita. But Mr. Colebrooke was also ready to contribute a share in sciences with which we are more nearly concerned,

* Asiat. Res., vol. ix. † Ibid., vol. xii.

He took a lively interest in the correction of errors respecting the physical geography of India, and was one of the first to declare (in 1815) his opinion that the Himalaya mountains were higher than the Andes, an opinion soon afterwards fully confirmed. He also was one of the first to enter upon a subject, to which we may now look with the greatest hope. The first part of vol. i. of our New Series of Transactions (published in 1822) contains two papers by him, one upon the geology of the valley of the Sutledge, which had been explored by Lieut. Gerard; the other upon the north-east of Bengal, where Mr. D. Scott had noticed various rocks, and, among the rest, a deposit which contained fossils, resembling, as he conceived, those of the London clay. I shall have occasion, in the course of this address, to refer to a recent repetition of this observation of an identity between the fossils of the east of India and those of the London and Paris basin. I may observe that these, and other contributions to Indian geology by other writers, contained in the volume of which I spoke, and a preceding one, induced the Secretaries of that time to insert a map, on which the localities of these observations were indicated; and to express in the volume a hope, that these were merely an earnest of the information which might be expected from the activity of British subjects in that quarter.

Among our foreign members deceased within the year, I regret much to have to mention one, to whom is due, in no small degree, a revolution in the mode of treating the subject of geology, which has taken place in our own times, and the formation of a new branch of geology. This revolution consists in the endeavour, now so familiar to us, to identify geological with recent changes, instead of classifying the great past changes in the surface of the earth which its structure discloses to us, as separate from the newer and slighter modifications of which history and tradition gives us evidence; and the study of the discernible causes of change to which we are thus led, I shall have occasion to speak of under the name of Geological Dynamics. You are well aware that Mr. Lyell is the person who has, with a bold and vigorous hand, moulded the whole scheme of geology upon this idea; but the power which he had of doing this was derived in no small degree from Von Hoff's admirable survey of the evidence of those changes which can be proved by tradition. The extent and universality of the facts thus brought into notice, might

well forcibly strike a philosopher already seeking to apply such a principle to geology; and Mr. Lyell has always been forward to acknowledge his obligations to M. Von Hoff. Indeed the idea of such an identification of geological with historical changes was by no means new; it had been both expressed and acted on by Deluc; and must have been present to the minds of those persons who framed the question which gave rise to Von Hoff's book. This question was proposed in 1818 by the Royal Academy of Science of Göttingen. "Considering," they said, "that we have, in the crust of the earth, evidence of great revolutions, which have happened at different times, in different portions, and of which the period and duration are unknown, we are led to ask whether certain more partial alterations may not lie within the domain of tradition, and give us the means of knowing at what period they took place, and what time the formation of certain portions of the earth's crust required; whereby some light may be thrown on those changes which lie beyond the limits of history."

M. Von Hoff's work,—"The history of those natural changes in the earth's surface which are proved by tradition"—appeared (the first part) in 1822, and had the Academy's prize assigned it. This part of the work contained an account of the changes due to the agency of water; and by the wide range of reading and study which it included, and the philosophical manner in which its copious materials were arranged, well justified the distinction which it received. The view presented in it of the great changes which have gone on from the beginning of historical times,—the yielding or advancing of coasts, the disappearing of islands, the union of seas,—appear to give a new face to the globe. But the portion of the judgement of the Academy which the author most valued was, that in which they said that he had used the sources of his information *conscientiously*. In 1824 appeared the second part, containing the history of volcanos and earthquakes; and, although the previous labours of Humboldt and Von Buch had done much to connect and generalise facts of this kind, Von Hoff's labours were an important step: "At least," he himself says, "he was not aware that any one before him had endeavoured to combine so large a mass of facts with the general ideas of the natural philosopher, so as to form a whole." Among other large views, we may see much which, as to kind of change supposed, agrees with the opinions of Mr. Darwin, of which I shall have to speak;

for instance, Von Hoff conceives that the island of Otaheite is undergoing a gradual elevation out of the sea.* Finally, the third volume of this work appeared after an interval of ten years, in 1834; in which he considers other causes of change; as rising and sinking of the land; alterations of rivers and seas; the operations of snow and ice; and also the geological results to which the whole survey had led him. In this volume he expresses his pleasure at the appearance of Mr. Lyell's work, which had taken place in the intervening period, and by which he had found much new light thrown upon his own speculations.

In the interval of time between the publication of the second and third volumes, M. Von Hoff published "Geological Observations on Carlsbad," (1825) and "Measures of Heights in and near Thuringia" (1833). In this last work he not only gave a great number of his own barometrical measurements, but discussed all extant measures of the heights of points in Thuringia, to the amount of above 1100. He also employed himself in meteorological observations.

Karl Ernest Adolph Von Hoff, Knight of the order of the White Falcon, and invested with several offices of honour and dignity at the Ducal Court of Gotha, died at Gotha the 24th of May last. He was 66 years of age, having been born in the same city Nov. 1, 1771.

Besides the history I have mentioned, which must always continue to be a classical work on the subject of which it treats, he was at the time of his death employed in compiling a continuation of his Notices of Earthquakes and Volcanic Eruptions; and also a new work, which was considered to be an important one, and was to be entitled "Germany according to its Natural Conditions and Political Relations."

In attempting a rapid survey of the contributions to geological knowledge which have come under our notice during the past year, I may perhaps be allowed to advert to a distinction of the subject into Descriptive Geology and Geological Dynamics; the former science having for its object the description of the strata and other features of the earth's surface as they now exist; and the latter science being employed in examining and reducing to law the causes which may have produced such phænomena. We appear to be directed to

* Part II. Pref. p. xiv.

such a separation of our subject by the present condition of our geological studies, in which we and our predecessors have accumulated a vast store of facts of observation, and have laboured with intense curiosity, but hitherto with very imperfect success, to extract from these facts a clear and connected knowledge of the history of the earth's changes. Nearly the same was the condition of astronomy at the time of Kepler, when the accumulated observations of twenty centuries resisted all the attempts of that ingenious man and his contemporaries to construct a science of physical astronomy. But though checked by such failures, they were not far from success; and when for the next succeeding century philosophers had employed themselves in creating a distinct science of Dynamics, the science of physical astronomy, full and complete, made its appearance, as if it were a matter of course; and thus showed the wisdom of separately cultivating the study of causes, and the classification of facts.

DESCRIPTIVE GEOLOGY.

If we begin with geological facts, our attention is first drawn to that district on the earth's surface within which the facts have been subjected to a satisfactory comparison and classification, which may be considered, in a general way, as including England, France, Italy, Germany, and Scandinavia. The language which the rocks of these various countries speak has been, in a great measure, reduced to the same geological alphabet. The questions of the determination of any member in one country, or the identification of similar members in two countries, are, for the most part, problems admitting of a definite and exact solution. In countries out of this district, on the other hand, we have not only to explore but to classify. We have to divine their geological alphabet;—to decipher as well as to read. We have not only to discover of what British rocks the observed ones are the equivalents, but we have to ascertain whether there be an equivalence; and where this relation vanishes, we have to discover what new resemblances and differences of members are most worthy our notice. The great difference in the nature of the geologist's task in these two cases seems to me to make it desirable to employ the familiar division of *Home* and *Foreign* Geology in a wider sense than has hitherto been common, including in the former all that re-

gion of Europe which has had its order of strata well identified with our own; this distinction then I shall employ.

1. *Home (North European) Geology.*—If we attempt, in this part of our subject, to follow an order of strata, we must begin with the oldest stratified rocks, though they are undoubtedly the most obscure; for the same reason which compels the historian of states to begin with the dim twilight of their savage or heroic times; namely, because at the other extremity of the series there is no boundary; since the events of past ages and their records form an unbroken series, leading us to the unfinished occurrences and works of to-day. Going then as far back as the historian of the earth can discern any light, and, for reasons which may hereafter be spoken of, shaping our course by the stratified rocks alone, we should first have to ask what addition has been made during the past year to our acquaintance with those formations which have generally been called *transition*. And here, gentlemen, many of you well know, that if I had had to address you at a period a little later, I might have hoped to be able to point out, among the labours of our members, some which may be considered as events of primary importance in this part of our knowledge;—steps which may be described as a new foundation rather than a mere extension of this portion of European geology;—a separation and arrangement of transition rocks, which is likely to become the type and classical model of that part of the geological series, as Smith's arrangement of the oolites became the type of that portion of the strata. I speak of Professor Sedgwick's views on the Cambrian rocks, which occupy the north-west of Wales, and Mr. Murchison's on the Silurian formations which cover the remainder of the principality and the adjacent parts of England. Mr. Murchison's work, which cannot but be one of first-rate value and interest, will, I trust, be in our hands in a few weeks; and I should grieve to think that Professor Sedgwick will be not only so unjust to his own reputation, but so regardless of the convenience and expectations of geologists, as to withhold from the world much longer the views which his sagacious and philosophical mind has extracted from the accumulated labour of so many toilsome years, on a subject abandoned to him mainly from its difficulty and complexity.

Turning then to the researches which have been laid before us upon the earlier stratified rocks, I am first led to notice the important

memoir of the two gentlemen I have just mentioned, upon the structure of North Devonshire. According to the views of these gentlemen, founded upon an extended examination of the county, this portion of England forms a great trough, having an east and west position, in which a series of culmiferous beds rest at their northern and southern extremities upon older rocks. The plants found in the culmiferous beds are said to be all identical with species which are abundant in the coal-fields of the central counties of England, and of the South Welsh coal basin: and it was at first conceived that these plants differed essentially from the scanty and imperfect remains of vegetables which are found in the older rocks. More recently, however, the same fossil plants which occur in the culm measures are said to have been detected in the subjacent strata. Before this fact was known, the identity of the fossils and the resemblance of mineralogical character seemed irresistibly to prove the culm-bearing beds of Devon to be the same formation with the culm or coal-bearing measures of Pembrokeshire on the opposite side of the Bristol Channel. How far this apparent anomaly admits of explanation, and in what manner it is to be allowed to modify the conclusion previously drawn, we may perhaps most properly consider as questions hereafter to be decided. The rocks which support the culmiferous formation on the north are conceived by Messrs. Sedgwick and Murchison to be a series, of which the last ascending term is probably of the date of the lowest portion of the Silurian system. On the south the culmiferous strata rest partly upon the granite, and partly upon the oldest slate rocks of Devon and Cornwall.

The same general view of the nature of the transverse section of Devon, and of the age of the culm, has been presented, perhaps I ought to say adopted, by the authors of two other papers upon the same region which have been brought before us,—Mr. Austen and Mr. Weaver;—and also, at least so far as the section is concerned, by the Rev. D. Williams in a communication made to the British Association in September last. Nor am I aware that it has been dissented from by any one who has examined the county in question since this view was made generally known. Resting on the concurrence of so many able observers, I should conceive, therefore, that we may look upon this view as *established*, so far as the time which has elapsed allows us to use the term. No truths should be termed incontestable

till a considerable period has been left for the antagonists to show themselves and to try their force.

Although this view has thus so good a claim to acceptance, you are aware, gentlemen, that it is entirely different, both as to the form of the section and the age of the members, from that which was entertained up to the time when these gentlemen turned their attention to the subject. Their opinion respecting Devonshire being adopted, along with the views of the same eminent geologists respecting Cumberland and North and South Wales, one-third of our geological map of England will require to be touched with a fresh pencil.

Nor is this wonderful. It is rather a matter of extraordinary surprise, that when the rest of the geological map of England is again drawn, there are scarcely any but microscopic alterations which require to be made. No higher evidence can be conceived of the vast knowledge and great sagacity of its author.

Such modifications we must ever expect to have to make of a first approximation; and I should think it a misfortune to our researches if we should attempt to elude this necessity by giving up the key of all our geological knowledge of our country,—the doctrine that there is a fixed order of strata, characterized mainly by their organic fossils. If we have not advanced so far as to prove this, what have we proved? If our terms do not imply this, what is their meaning? Is it not true, in our science as in all others, that a technical phraseology is real wealth, because it puts in our hands a vast treasure of foregone generalizations? And if we evade the difficulties which may occur in the application of this phraseology to new cases, by declaring that our terms are of little importance, is not this to deprive our language of all meaning and all worth? Do we not thus refuse to recognise as valuable the tokens which we ourselves circulate, and plainly declare ourselves bankrupts in knowledge? When certain strata of Devon have thus been identified with the coal measures of other regions, can we still term them grauwacke? Either this term implies members having a definite place in our series of strata, or it does not. If it do, it is certain that these strata have not that place. If it do not, it conveys no geological knowledge at all. But if it be used to imply a rejection of such series, it involves a denial of all

geological knowledge hitherto asserted concerning the older rocks of this county.

The transition downwards from the culmiferous beds of Devon to the older strata on which they rest, is, according to almost all who have studied the subject, wrapt in great obscurity. In this obscurity, if it be true that the fossil plants of the culm measures are found also in the subjacent rocks, there is nothing which need make us mistrust the clear and positive part of our knowledge. And even if this be so, it will not be the less necessary to separate the culmiferous from the subjacent Silurian and Cambrian systems, by a different name in our lists, and by a different colour in our geological maps, if they are to represent the present state of our information.

The interest of this question has induced me to dwell upon it longer than I had intended, and I must on that account be very brief in my notice of many other communications. I may observe that the very nature of several of these indicates very remarkably the European character which our geology has assumed, since they have for their object the identification of some members of the recognised series of England, and of France, or Germany. Thus Mr. Murchison and Mr. Strickland have attempted to show, by the evidence of organic fossils, now for the first time adduced on this point, that the red saliferous marls of Gloucester, Worcester, and Warwick shires, with an included bed of sandstone, represent the keuper or *marnes irisées* of Germany; and that the underlying sandstone of Ombersly, Bromsgrove, and Warwick is part of the bunter sandstein or grès bigarré of foreign geologists. They are thus led to conclude that though the muschelkalk, which intervenes between these formations in Germany, is absent in the new red system of England, and of a large part of France, its other members may be identified over the whole of the north of Europe.

Proceeding from the new red to the oolite system, we have a memoir from Mr. Pratt containing an examination of the geological character of the coast of Normandy, which necessarily implies a comparison of this series of rocks with those of England. The identification is found to be complete, as had already been believed; but Mr. Pratt has made some alteration in the received doctrines on this subject; for instance, the Caen stone, which is usually considered to repre-

sent the great oolite, he finds to resemble in its fossils the inferior oolite.

Ascending still, we have to notice Mr. Clarke's elaborate geological survey of Suffolk, which, of course, refers entirely to the chalk and overlying beds. With regard to the crag of this district, I may remark that M. Desnoyers, in a communication made to the Geological Society of France, has endeavoured to identify this formation with the *Faluns* of the Touraine. M. Deshayes had referred the latter to the *Miocene*, and the crag to the *Pliocene* formations of Mr. Lyell. The point is one of great interest, since it involves the question of the value and right mode of application of the test of the relative number of recent species, on which Mr. Lyell's classification, or at least his nomenclature, is founded. I conceive that in a matter of arrangement any arbitrary numerical character must lead to violations of nature's classifications; and can only be considered as an artificial method, to be used provisionally till some more genuine principle of order is discovered.

Mr. Clarke, in his survey, has noted as one division of the diluvium of his district, a clay of a yellowish or bluish hue, containing rolled pieces of chalk. This deposit is of great extent and thickness in East Anglia and the neighbouring parts; and is worth notice, since this deposit is one main cause of the geological confusion and obscurity in which that region is involved. In the neighbourhood of Cambridge this diluvial deposit is called the *brown clay*; and I can state, from my own experience, that the recognition of it as a separate bed at once rendered the stratification clear, where it had long been unintelligible.

Before quitting our stratified rocks, I may notice the communications respecting some of their fossils which we have received, particularly that of Mr. Williamson on the fossil fishes of the Lancashire coal field, and the establishment of the new genus Tropæum, separated from the Hamites of the green sand by Mr. Sowerby.

In attempting to pursue a stratigraphical order, we are compelled to reserve for a separate head the notice of unstratified rocks, since their age and history are only known by the mode in which they interrupt and disturb the rest of the series. We have not had many communications respecting European rocks of this character; but we cannot but be struck by the subversion of ancient ideas which result

from the investigations of Messrs. Murchison and Sedgwick. They have shown that the granite of Dartmoor, and consequently that of Cornwall, formerly considered as one of the earliest monuments of the primeval ages of the earth's history, is posterior to the deposit of the culm measures.

Advancing to newer phænomena, we find the evidences of change still unexhausted. We cannot but reflect how familiar those views of the elevation and depression of portions of the earth's surface are become, which were at first considered so strange and startling. This is remarkably shown by the number of communications concerning raised beaches which we have recently received. When we visit places where these occur, and look at the winding shore, where the sea line is faithfully followed or distantly imitated by terraces, sands and pebbles a little above it, we wonder that we should so long have been blind to this kind of evidence. Such raised beaches have been described during the past year, by Mr. Prestwich, as occurring in the Murray Frith; by Mr. Austin, in the valley of the Axe, the Exe, and the Otter. Dr. Forchammer has given us the evidence of recent elevation in the island of Bornholm; Mr. Trevelyan has given us similar evidence for the coast of Jutland, and the islands of Guernsey and Jersey.

Mr. Morris's paper, describing a series of dislocations in the chalk cliffs to the south of Ramsgate, marked by shifts in a bed of tabular flint, may perhaps be considered as also affording evidence of violent elevation. But since a small derangement of the conditions of support of any stratum might occasion dislocations of the scale of those here described, it would probably be hazardous to consider them as otherwise than local accidents.

Among descriptions of the most recent geological phænomena, I must notice Mr. Clarke's paper on certain peat marshes and submarine forests, which occur near Poole in Devonshire; and in his investigation of the causes which have produced the results now visible, we may see by how easy a gradation descriptive geology passes into the other portion of the subject, the study of the processes by which change is produced.

Finally, in concluding this survey of our descriptive home geology, I notice, with great pleasure, Mr. Burr's communication of his notes on the geology of the line of the proposed Birmingham and Glou-

cester Railway. In a country like this, in which the order and boundaries of the strata are, for the most part, well ascertained, an additional accuracy of measurement, of great value to us, may be supplied by the operations of civil engineers employed on canals, roads, and the like works. With this persuasion, and acting with the advice of the Council, I wrote letters to a great number of engineers, begging them to communicate to us the levels and sections which they might obtain in the course of their professional employments; and I am happy to see so excellent an example as Mr. Burr's paper supplies, of the advantage which may be derived from materials of this class.

2. *Foreign (South European and Trans-European) Geology.*—In proceeding beyond the Alps, and still more as we advance beyond the shores of Europe, we can no longer, so far at least as geologists have hitherto discovered, trace that remarkable correspondence of the strata of different countries which we can study so successfully in our *home circuit*. With the mountain masses of those more distant regions we are, it would seem, hardly authorised as yet in making any more detailed distinctions than the general one of secondary and tertiary strata; the latter including the strata in which we trace an approach to the existing species of animals, and the former implying a general comparison with our chalk, oolites, and lower strata. Perhaps we may further distinguish in most countries which have been visited, a great mass of transition slates; but the establishment of such divisions must be the business of geological observers.

We have had several valuable additions to this portion of our knowledge, including, as we must do, Greece and its islands in this foreign district. That the Apennine limestone is the predominant mass of the Morea, had been made known by the researches of MM. Boblaye and Virlet. Mr. Strickland and Mr. Hamilton have told us that the same rock forms a large mass of the island of Zante and other islands in that sea, and of the neighbourhood of Smyrna. They find also tertiary beds, as on the south side of the bay of Smyrna; on the east side of the island of Zante; and at Lixouri in Cephalonia, where the tertiary beds are remarkable for the number and beauty of their fossils, some of which have been identified with species existing in the Mediterranean. Dr. Bell, who travelled from Teheran to the

shores of the Caspian, has given us an account of the rocks which he observed in Mazanderan. From the statements made by him, we are led to believe, that a more continued and detailed observation of the country would give the true geological order of the deposits in this region; which might then, perhaps, serve as a connecting link between western Asia and India.

It is among the favourable omens for the geology of India, of which we now see many, that a temperate spirit of generalization has recently been applied to the examination of her soil; a spirit which contents itself with such a general reference of the foreign to the home strata as we have described, till by its own labours it has earned the right of asserting some closer correspondence. If to deny the value of our geological terms within the home district, where they mark an order which has been repeatedly verified, would be a suicidal scepticism in geologists, there would be a rashness and levity no less fatal in applying them to distant regions where no order has yet been ascertained.

Captain Grant in his account of Cutch, and Mr. Malcolmson in his description of a large portion of the Indian peninsula, have not ventured to call the strata which they have examined by the names which describe European formations. We may trust that, hereafter, the admirable activity and resource which our countrymen display in that wonderful appendage of our empire, will enable them to communicate to us a genuine Indian arrangement of secondary strata. In the mean time, Mr. Malcolmson has most laudably employed himself in determining the age of the wide-spread igneous rocks of the peninsula of India, with reference to the contiguous strata. And Dr. McCleland, who was associated with Mr. Griffith in the scientific deputation sent under Dr. Wallich into Upper Asam, has, among other geological observations, noted a raised bed, at 1500 feet above the sea level, in which none of the species are identical with those of the Bay of Bengal on the one hand, or the secondary strata on the north of the Himalaya on the other; but in which a resemblance was at once recognised with the species of the Paris basin.

This resemblance between the extinct animal population of regions so remote from each other, is in itself remarkable enough. It is stil more curious to observe, that the same coincidence of the ancient animals of France and India has recently been detected in another

case; and what makes the circumstance still more remarkable is, that the animal was not only new in both countries as a fossil genus, but involved a transgression of the supposed boundaries of fossil forms. Not only had no human bones been found in genuine strata, but as it had been generally held, no traces of those creatures which most nearly imitate the human form. This rule now no longer holds good; for during the past year the bones of monkeys have been discovered both at Sansan, in France, in the Sewalik Hills in the north of Hindostan, and more recently under the city of Calcutta.

That this is a highly interesting and important discovery, no one who attends to the signification of geological speculations can doubt. I do not know if there are any persons who lament, or any who exult, that this discovery tends to obliterate the boundary between the present condition of the earth, tenanted by man, and the former stages through which it has passed. For my own part I can see no such tendency. I have no belief that geology will ever be able to point to the commencement of the present order of things, as a problem which she can solve, if she is allowed to make the attempt. The gradation in form between man and other animals, a gradation which we all recognise, and which, therefore, need not startle us because it is presented under a new aspect, is but a slight and, as appears to me, unimportant feature, in looking at the great subject of man's origin. Even if we had no Divine record to guide us, it would be most unphilosophical to attempt to trace back the history of man without taking into account the most remarkable facts in his nature: the facts of civilization, art, government, writing, speech—his traditions—his internal wants—his intellectual, moral, and religious constitution. If we will look backwards, we must look at all these things as evidences of the origin and end of man's being. When we do thus comprehend in our view the whole of the case, it is impossible for us, as I have elsewhere said, to arrive at an origin homogeneous with the present state of things; and on such a subject the geologist may be well content to close his own volume, and open one which has man's moral and religious nature for its subject.

In order to complete the notice of the contributions to foreign geology, I must mention Mr. Roy's account of Upper Canada: in which country he conceives that he has detected terraces which exhibit the

beaches of the lakes when the level of their surface was more elevated than they are at present. I must refer also to Mr. Bollaert's paper on alluvial accumulations containing large masses of silver ore in Peru. And, finally, I have to direct your attention to the very curious information respecting the geology of South America, which we have received from Mr. Darwin. In a communication made to us, he gave a very striking view of the structure of a large portion of that continent; and, as I have already had occasion to observe, he has brought to this country the remains of various fossil animals of entirely new kinds, of exceeding interest to the zoologist as well as the geologist. I need only remind you of the gigantic mammifer which has been reconstructed in idea by Mr. Owen, upon the evidence of a fossil skull, and has been named by him the *Toxodon Platensis*. This animal, although a *Rodent*, according to its dental characters, in other respects manifests an affinity to the *Pachyderms*; and also to the *Dinotherium*, and to the *cetaceous* order. Many other fossil animals have been discovered in South America; and all, from their magnitude, fitted to excite our wonder, when we compare the diminutive size of the present races of animals which inhabit that country. The animal remains found by Mr. Darwin comprise, besides the Toxodon, which extraordinary animal was as large as a hippopotamus,—(2, 3, 4, 5, 6.) the Megatherium, and four or five other large Edentata;—(7.) an immense Mastodon; —(8) the Horse;—(9.) an animal larger than a horse, and of very singular character, of which a fragment of the head has been found; —(10, 11, 12.) parts of Rodents, one of considerable size;—(13.) a Llama, or *Guanaco*, fully as large as the Camel.

But I should very ill convey my impression of the great value of the researches of Mr. Darwin, by any enumeration of special points of geology or palæontology on which they have thrown light. Looking at the general mass of his results, the account of which he has been kind enough to place in my hands, I cannot help considering his voyage round the world as one of the most important events for geology which has occurred for many years. We may think ourselves fortunate that Capt. Fitz Roy, who conducted the expedition, was led, by his enlightened zeal for science, to take out a naturalist with him. And we have further reason to rejoice that this lot fell to a gentleman like Mr. Darwin, who possessed the genuine spirit and

zeal, as well as knowledge of a naturalist; who had pursued the studies which fitted him for this employment, under the friendly guidance of Dr. Grant at Edinburgh, and Professor Henslow and Professor Sedgwick at Cambridge; and whose powers of reason and application had been braced and disciplined by the other studies of the University of which the latter two gentlemen are such distinguished ornaments. But some of the principal of these results may be most conveniently mentioned, when we pass from mere descriptive geology, to that other division of the subject which I have termed Geological Dynamics. And this I now proceed to do.

GEOLOGICAL DYNAMICS.

This term is intended to express generally the science, so far as we can frame a science, of the causes of change by which geological phænomena have been produced. Without here speaking of any classification of such changes, I may observe that the gradual elevation and depression, through long ages, of large portions of the earth's crust, is a proximate cause by which such phænomena have been explained: and this class of events, its evidence, extent, and consequence, is brought before our view by Mr. Darwin's investigations, with a clearness and force which has, I think I may say, filled all of us with admiration. I may refer especially to his views respecting the history of coral isles. Those vast tracts of the Pacific which contain, along with small portions of scattered land, innumerable long reefs and small circles of coral, had hitherto been full of problems, of which no satisfactory solution could be found. For how could we explain the strange forms of these reefs; their long and winding lines; their parallelism to the shores? and by what means did the animals, which can only work near the surface, build up a fabric which has its foundations in the deepest abysses of ocean? To these questions Mr. Darwin replies, that all these circumstances, the linear or annular form, their reference to the boundary of the land, the clusters of little islands occupying so small a portion of the sea, and, above all, the existence of the solid coral at the bottom of deep seas, point out to us that the bottom of the sea has descended slowly and gradually, carrying with it both land and corals; while the animals of the latter are constantly employed in building to the surface,

and thus mark the shores of submerged lands, of which the summits may or may not remain extant above the waters. I need not here further state Mr. Darwin's views, or explain how corals, which when the level is permanent fringe the shore to the depth of twenty fathoms, as the land gradually sinks, become successively encircling reefs at a distance from the shore; or barrier reefs at a still greater distance and depth; or when the circuit is small, lagoon islands:—how, again, the same corals, when the land rises, are carried into elevated situations, where they remain as evidences of the elevation. We have had placed before us the map, in which Mr. Darwin has, upon evidence of this kind, divided the surface of the Southern Pacific and Indian oceans into vast bands of alternate elevation and depression; and we have seen the remarkable confirmation of his views in the observation that active volcanos occur only in the areas of elevation. Guided by the principles which he learned from my distinguished predecessor in this chair, Mr. Darwin has presented this subject under an aspect which cannot but have the most powerful influence on the speculations concerning the history of our globe, to which you, gentlemen, may hereafter be led. I might say the same of the large and philosophical views which you will find illustrated in his work, on the laws of change of climate, of diffusion, duration and extinction of species, and other great problems of our science which this voyage has suggested. I know that I only express your feeling when I say, that we look with impatience to the period when this portion of the results of Captain Fitz Roy's voyage shall be published, as the scientific world in general looks eagerly for the whole record of that important expedition.

And I cannot omit this occasion of mentioning with great gratification, the liberal assistance which the Government of this country have lent to the publication of the discoveries in natural history which Mr. Darwin's voyage has produced. The new animals which he has to make known to the world will thus come before the public described by the most eminent naturalists, and represented in a manner worthy of the subject and of the nation. I am sure that I may express the gratitude of the scientific world, as well as my own, for this enlightened and judicious measure.

I may here notice Mr. Darwin's opinion, so ably exposed in a paper read before us, that the change by which a variety of materials

thrown on the earth's surface become vegetable mould, is produced by the digestive process of the common earth worm.

I will here also advert to Mr. Fox's paper on the process by which mineral veins have been filled up. This he conceives might be produced by the circulation or ascension of currents of heated water from the deeper parts of the original fissures. The discovery of the causes of the formation and filling of metallic veins, one of the earliest subjects of geological speculation, will remain probably as a problem for its later stages, when our insight into the laws of slow chemical changes is far clearer than it is at the present day.

If, from these proximate causes of change of which I have spoken, we proceed to those ulterior causes by which such events as these are produced;—to the subterraneous machinery by which islands and continents appear and vanish in the great drama of the world's physical history;—we have before us questions still more obscure, but questions which we must ask and answer in order to entitle ourselves to look with any hope towards geological theory. Of late years an opinion has taken root among us, that the dynamics of geology must invoke the aid of mathematical reasoning and calculation, as the dynamics of astronomy did, at the turning point of its splendid career. Nor can we hesitate to accept this opinion, and to look forwards to the mathematical cultivation of physical geology, as one of the destined stages of our progress towards truth. But we must remember, that in order to pursue this path with advantage, we have, in every instance, two steps to make, each of which demands great sagacity, and may require much time and labour. These two steps are, to *propose* the proper problem, and to *solve* it. Last year an important example of this kind was brought under your notice by my predecessor. The supposition that there are, beneath the crust of the terrestrial globe, liquid or semiliquid masses which exert a pressure upwards, leads to the inquiry what phænomena of fissure, disruption, and dislocation, this subterraneous strain would produce. The answer to this inquiry must be given by mathematical reasoning from mechanical principles; and Mr. Hopkins, who proposed, and to a considerable extent solved this problem, has put forth a set of results, with which, so far as they are definite and decisive, it will be highly important to compare the existing phænomena of disturbed geological districts. The same assumption, of an incandes-

cent mass existing deep below the earth's surface, has led two other distinguished members of our body to another train of speculations; which, however, though highly interesting, I should be disposed to consider as only the enunciation of a problem, requiring no small amount of mathematical skill for its solution. I speak of the speculations of Professor Babbage and Sir John Herschel, concerning the subterraneous oscillations of the isothermal surfaces of great temperature. They remark that such oscillations will arise, when thick and extensive deposits take place on any parts of the surface of the earth, (as for instance at the bottoms of seas,) because such deposits increase the thickness of the coating over a given subterraneous point; and thus removing the cooling effect of the surface, bring a high temperature to a place where it did not exist before. The deposited strata might thus be invaded by violent heat advancing from below; and there might result both changes of position arising from extension and contraction, and a metamorphic structure in the rocks themselves. It is highly instructing to have this chain of conceivable effects pointed out to us; but we may venture to observe, that in order to render the suggestion of permanent use, it will be necessary to express, in some probable numbers, the laws of the result as affected by the conductivity of the earth's mass, the rate and thickness of the deposit, and other circumstances. For instance, we know that a deposit of one thousand feet thick would be quite insufficient to occasion a metamorphic operation in its lower strata. Would then a deposit of ten thousand or of twenty thousand feet call into play such a process? To answer questions like these, of which a vast number must at once occur to our minds, we have many experimental data to collect, many intricate calculations to follow out. And it would be easy to point out problems of a still more abstruse kind, in which we no less require aid from the mathematician, before we can proceed in our generalizations. May we not hope to see some fortunate man of genius unveil to us the mechanics of crystalline forces? And when that is done, can we doubt that we shall have a ray of new light thrown upon those extraordinary phænomena of slaty cleavage in mountain masses which have lately been brought under our notice? Or, recollecting the experiments of Sir James Hall, (a striking step in geological dynamics,) may we not hope then to learn how those crystalline forces are stimulated by heat; and thus follow the meta-

morphic process into its innermost recesses? These and a thousand such questions lie before us;—tangled and arduous inquiries no doubt, but connected by their common bearing upon one great subject;—'a mighty maze, but not without a plan.' And through this maze we must force our way in order to advance towards any sound geological theory. The task is one of labour and difficulty; but I well know, gentlemen, that you will not shrink from it on that account. Those who aspire to the felicity of knowing the causes of things, must not only trample under foot the fears of a timid unphilosophical spirit, which the poet deems so necessary a preparation, but they must look with a steady eye upon difficulty as well as violence. They must regard the terrors of the volcano and the earthquake, the secret paths by which hot and cold and moist and dry ran into their places, the wildest rush of the fluid mass, the latent powers which give solidity to the rock,—as operations of which they have to trace the laws and measure the quantities with mathematical exactness. And though there can be no doubt that the greater part of us shall be more usefully employed in endeavouring to add to the stores of descriptive geology, than in these abstruse and difficult investigations, yet we must always receive, with great pleasure, any communications containing real advances in the mathematical dynamics of geology, from those whose studies and whose powers enable them to lay an effectual grasp upon these complex and refractory problems.

I have but a single word to add in conclusion. This Society has always been an object of my admiration and respect, not only from the importance and range of its scientific objects, the wide and exact knowledge which it accumulates, the philosophical spirit which it calls into play, the boundless prospect of advance which it offers; but also for the manner in which its meetings and the intercourse of its members have ever been conducted; the manly vigour of discussion, tempered always by mutual respect and by good manners; the deep interest of all in the prosperity of the Society, to which, whenever the hour of need comes, private differences of opinion and resentments have given way. To be placed for a time at the head of a body which I look upon with such sentiments, I must ever consider as one of the greatest distinctions which can reward any one who gives his attention to science. I trust, by your assistance and kind sympathy, gentlemen, I shall be able to preserve the spirit and temper

which I so much admire;—to hand that torch to my successor burning as brightly as it has hitherto done. And there is one consideration which will make me look with an especial satisfaction upon such a result. I have not myself the great honour of being one of the members of the Society who are connected with it by an early interest in its fortunes, and by long participation in its labours. I may consider myself as only belonging to its second generation. Now if there be a critical and a perilous time in the progress of a voluntary association like ours, it is when its administration passes out of the hands of its founders into those of their successors. It is like that important and trying epoch when the youth quits the paternal roof. I will say however, gentlemen, for myself and for my fellow-officers, some of whom are in the same condition, that our best cares shall not be wanting that the Society may suffer as little as possible by this change. And among our grounds for hope and trust, the main one is this: that though the offices of the Society may be in younger hands, the parental cares of its founders are not withdrawn. We have to discharge our office with the aid and counsel of those excellent persons to whom the prosperity of the Society up to the present time has been owing. Surrounded by such men, knowing their generous and ready sympathy for the attempts and exertions of their followers and disciples, I feel a cheerful confidence in the future destinies of the Geological Society; and a persuasion that it will not only preserve but extend its influence as a bond of scientific and social union among its members.

THE END.

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