published works have constantly shown the important aid that geology has received from the cautious inductions derived from the study of palæontology.

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After the other proceedings had been completed, and the Officers and Council had been elected, the President proceeded to address the Meeting.

## ANNIVERSARY ADDRESS OF THE PRESIDENT,

### SIR CHARLES LYELL.

Gentlemen,—You will have heard in the Report of your Council, that the finances of the Society continue to be in a flourishing state. In the course of the past year we have had to lament, as usual in so large a body, the death of several of our Members, and some of these it will be my duty more particularly to mention, as having distinguished themselves by original investigations, or by using the influence of their station or fortune in promoting the progress of Geology.

Christian VIII. King of Denmark was enrolled a Fellow of this Society in 1822. Two years before that time, when travelling in Italy, he had witnessed an eruption of Vesuvius, and had read a description of it to the Academy of Sciences at Naples; a communication published in their Transactions, and afterwards reprinted in Leonhard's Journal for 1822.

From an early age he had taken a lively interest in the progress of natural history, and when Crown Prince, formed at Copenhagen, at his own expense, a magnificent collection of shells, the number of species being estimated at not less than 12,000, exclusive of fossils. When I visited the Danish capital in 1835, he placed this museum and his library at my disposal, and I had then an opportunity of knowing that he kept up an acquaintance with the new species added from year to year to his cabinet, then in charge of an able conchologist, Dr. Beck, and that he was very desirous of making his museum useful to all zoologists. Nor was he inattentive to the points of controversy then agitated respecting the geology of Denmark. He questioned me closely as to my opinion, whether the strata of Faxoe, containing certain species of *Cyprea, Oliva, Mitra,* and other genera usually regarded as characteristic of the tertiary period, really belonged to that epoch, or to the cretaceous rocks. That the latter conclusion was correct I had satisfied myself, after exploring the cliffs of Moën and Seeland, as I have explained in your Transactions; and you are aware that the Faxoe beds, together with those of Maestricht and Sezanne near Paris, have been recently classed as an upper member of the great cretaceous system*.

When Christian VIII. succeeded to the throne, the cares and duties of an absolute monarch did not make him forgetful of his former love for natural history. He was always accessible to scientific foreigners and natives, and set on foot several publications, among which I may mention the 'Gea Danica' of Professors Steenstrup and Forchhammer. He also gave his patronage to a splendid botanical work on the palms of Mexico, by Professor Liebmann, and promoted liberally the geological expedition of Baron Waltershausen and Professor Bunsen to Iceland. He also took care that a good naturalist should accompany the voyage of the Galathea round the world; and when that expedition returned, he directed that the valuable collections, made by the officers in various countries, should be divided equally between the Universities of Copenhagen and Kiel. As Crown Prince, he had been elected President of the Academy of Sciences at Copenhagen, and when he attended their meetings, after his accession to the throne, he always declined to be received as king, taking his place simply as a member, or as any other President. After a reign of nine years, he died in January 1848.

The Earl of Auckland is well known to have zealously used his influence in England, and the political power which for some years he wielded as Governor-General in India, in the encouragement of various branches of science and natural history. When the Directors of the East India Company determined in 1844 to send out a geologist to survey the coal-fields of Bengal, Madras, and other parts of their Eastern possessions, I was consulted by Lord Auckland, then lately returned from the East, in regard to the best mode of organizing the undertaking, and was struck with his earnestness in forward-

* Geol. Trans. 2nd series, vol. v. p. 249.

ing the objects in view. He was intimately acquainted with all that had been previously done, towards opening out the mineral resources of India; indeed several of the previous surveys had been set on foot by himself; and when explaining to me his views, he justly observed that a Government should limit itself to the task of collecting and publishing correct information, and giving a right direction to private enterprise, without attempting to derive pecuniary profits, from its undertakings.

Sir Edward Ryan has shown me a pamphlet, entitled "Hints for collecting information, compiled for the expedition to China," which was planned by Lord Auckland in 1840, and intended to direct the attention of the officers employed on that service to the acquisition of knowledge of various kinds, both relating to science and the arts of life, and the manners, customs and languages of the people whom they might visit. In these instructions we trace the germ and to a great extent the model of that Manual of Scientific Inquiry, for which, several years later, when he became First Lord of the Admiralty, Lord Auckland obtained the editorship of Sir John Herschel. Such of you as have studied this most useful Manual are aware how largely the Fellows of this Society have contributed to its contents; the geology having been written by Mr. Darwin, the mineralogy by Sir H. De la Beche, the geography by Mr. Hamilton, the memoir on earthquakes by Mr. Mallet, and the zoology by Prof. Owen.

William Clift, Esq., for many years Keeper of the Hunterian Museum of the College of Surgeons, served several times in the Council of this Society between the years 1832 and 1837. From an early period many of the ablest and most active members of the Society were in the habit of consulting him on all questions bearing on fossil osteology, and we find frequent acknowledgements of his valuable services in the papers of Dr. Buckland, Dr. Conybeare and others. Dr. Mantell also, in his first paper on the Iguanodon, published so long ago as 1825, says, "I resolved to avail myself of the offer of Mr. Clift, (to whose kindness and liberality I hold myself particularly indebted,) to assist me in comparing the fossil teeth with those of the recent Lacertæ in the Museum of the Royal College of Surgeons. The results of this examination proved highly satisfactory, for in an Iguana which Mr. Stutchbury had prepared to present to the College, we discovered teeth possessing the form and structure of the fossil specimens."

Similar acknowledgements are made by the Dean of Westminster in his 'Reliquiæ Diluvianæ' (p. 35), and in his paper in our Transactions on the Pterodactyle (vol. iii. 1829); some of the minute particulars of the structure of that flying reptile having been illustrated by the drawings of Mr. Clift. The co-operation indeed of the Keeper of the Hunterian Collection is noticed in almost every memoir descriptive of fossil bones which appeared after the date of Sir Everard Home's first paper on the Ichthyosaurus in 1814, down to the period when Prof. Owen began to relieve his father-in-law from his duties as Curator of the Museum. Nor were the fruits of his skill and industry confined to British publications, for not a few of the figures in the 'Ossemens Fossiles' of Cuvier were executed by the same hand, and the great French anatomist often speaks of them in flattering terms, as "faites par Mons. Clift, dont le beau talent a déjà enrichi ce recueil de tant de planches non moins remarquables par leur exécution que par leur fidélité."

The first of Mr. Clift's own memoirs on organic remains was a description of "Some Fossil Bones discovered in Caverns in the Limestone Quarries of Oreston," printed in the Philosophical Transactions for 1823. His first contribution to the Geological Society was, "An Account of two new species of Mastodon and other Fossil Vertebrata, discovered by Mr. Crawfurd in Ava*." His second and last paper read to the Society was, "An Account of the Megatherium brought home in 1832 by Sir Woodbine Parish from South America†." Valuable as are these works, they convey no idea of the extent and variety of his labours in fossil osteology; for, regardless of personal distinction, he was singularly indifferent whether the results of his original research were given to the world in publications of his own or in those of his friends. He united a deep and tranquil enthusiasm for philosophical pursuits to great independence of character and simplicity of mind and manners, and he never seemed to need any other stimulus to excite or sustain his intellectual exertions than such as were afforded by the love of inquiry or the delight of arriving at new truths.

* Geol. Trans., 2nd ser. vol. ii. 1828. † Ibid. vol. iii.

Frederick Dixon, Esq. was educated at Eton, and completed his professional studies as a surgeon under Sir Astley Cooper. From the esteem in which his personal character, abilities and studious habits were held by his teachers and fellow-pupils, there is little doubt that his professional career would have been successful had he devoted himself to metropolitan practice. But he preferred the enjoyments and greater leisure afforded by a country residence, and retired early to reside on some property which he possessed at Worthing. There he continued to practise up to the period of his fatal illness in September last. His early tastes and habits of observation led him to study the geology and to collect the fossils of parts of Sussex adjacent to his residence ; and the peculiar skill with which he worked out organic remains from their native matrix, made his collection remarkable for the rarity, beauty and perfect condition of the specimens, especially of those from the chalk-pits of the Vale of Arun, and from the eocene beds of Bracklesham.

The number of new facts thus brought to light by Mr. Dixon determined him to publish a volume on the cretaceous and tertiary formations of Sussex, illustrated by figures of undescribed fossils, executed by the best artists whose skill he could command. He had made considerable progress in the preparation of this work at the period of his decease ; but as an author he was known only by a few papers on the historical antiquities of his neighbourhood, published in the Journal of the Sussex Archæological Society.

In the determination of his own rare and unique specimens of organic remains, he was so fortunate as to obtain the assistance of the highest authorities in different departments of palæontology ; and the appendix to his projected work was designed to contain a description of the fossil reptiles and mammals by Professor Owen ; of the fossil fishes by Sir Philip Egerton ; of the echinoderms and crustacea by Professor Edward Forbes ; of the fossil shells by Mr. Sowerby ; and of the fossil corals by Mr. Lonsdale : Mr. Dixon reserved to himself an account of the geological structure of his county, and of the localities of his fossil specimens, with other circumstances connected with their discovery. He expended large sums on the beautiful plates engraved for this publication, and you will learn with pleasure that Prof. Owen has most liberally undertaken to complete and publish this posthumous work. Mr. Dixon died at the age of fifty, on the 27th of September, 1849, deeply regretted by all those who knew him*.

Gentlemen,—It is now my duty, in accordance with the usual custom of my predecessors in office, to say something of the scientific labours of geologists during the past session. It is nearly twenty years since I announced, in the first edition of my 'Principles of Geology,' the conviction at which I had then arrived, after devoting some time to observation in the field, and to the study of the works of earlier writers, that the existing causes of change in the animate and inanimate world might be similar, not only in kind, but in degree, to those which have prevailed during many successive modifications of the earth's crust. I attempted to adapt the views which Hutton and Playfair had first promulgated, to a more advanced state of our science, and to extend their application, by showing, that should the same causes continue to act with unabated energy, for indefinite periods of the future, they must bring about revolutions not inferior in magnitude to those recorded in the monuments of past ages. After an interval of twenty years, during which Geology has been enriched by a vast accession of new facts, and when so many powerful minds, in every civilized country, have brought their intellectual energies to bear on the philosophy of our science, I may I think affirm that the idea of comparing the modern agents of change with those of remote epochs, as not inferior in power and intensity, appears even to the most sceptical a far less visionary and extravagant hypothesis than when I first declared my belief in its truth. As, however, there are not a few original observers, whose opinion I respect, who are still opposed to this doctrine, I cannot I believe do better on the present occasion than take a brief view of the bearing of some leading discoveries of modern date on this much-controverted question. I adopt this course the more willingly, because a perusal of the memoirs read before the Society during the past session, and the contemporary publications of other scientific bodies and authors in Europe and America, has convinced me that they are so varied and so overwhelming by their number and importance, as to make it impossible, within the limits of this anniversary Address, to give an analysis of the contents of each, still less

* A fuller account of Mr. Dixon's labours will be found in the 'Medical Times' for Dec. 8, 1849, of which notice I have largely availed myself.

to add criticisms and comments of my own. But in order to keep myself still further within due bounds, I shall not enter at present the field of palæontology, reserving for a future opportunity a comparison of the organic creation, in ancient and modern times, and the question whether the fluctuations of the living inhabitants of the globe have been regulated formerly by the same laws as now.

Among the points of geological interest relating exclusively to the inanimate world, none have given rise to a greater difference of opinion than the various causes suggested to account for the position of stratified and unstratified rocks in mountain chains. They are usually referred to the development of mechanical and volcanic forces of a paroxysmal character; but geologists who favour these views are by no means agreed whether the causes thus capable of modifying the earth's crust, were all of them in the beginning in a state of the highest intensity, and afterwards declined in energy, or whether they have been exerted again and again during short intervals of violent convulsion followed by long periods of repose. On these, and questions of a kindred nature, I shall proceed to offer some observations, well aware that I shall advocate opinions which I have long cherished, and on which I can scarcely fail to have a strong bias, but reminding you at the same time that they who defend conclusions opposed to mine have equal reason to doubt their own impartiality, and to suspect that they also may be influenced by old associations, and those strong prepossessions, with which nearly all the early literature of our science is imbued. It may be true that no geologist worthy of the name would contend at this time of day for the modern origin of our planet, or maintain the doctrine that it was created contemporaneously with man, although the multitude, including many of the educated classes, may, in their ignorance of the records of creation as written in the heavens and the earth, still fondly cling to such opinions. The cultivators of our science may be ready to grant the most indefinite duration to each successive geological epoch, yet they may still unconsciously derive a love of cataclysms and catastrophes, and faith in a primæval chaos out of which the present order of things was evolved, from an hereditary creed, not founded on facts, or strict inductive reasoning on natural phænomena.

As introductory to this subject, I cannot do better than recall your attention to the recently published memoir of Sir Roderick Murchison, on the structure of the Alps, Apennines and Carpathians, which deservedly occupies an entire Number of your Quarterly Journal*. It comprises a masterly summary of the labours of those who had gone before him, in a very difficult field of inquiry, as well as a luminous account of his own personal investigations, and should be studied by every one who is desirous of knowing what point the modern progress of geology has reached. On various important questions of which he treats, and in which I entirely agree with him, I cannot enter at present, but there is one leading conclusion established in his memoir which bears specially on the theory selected for discussion in this Address. He proves, as it appears to me in a satisfactory manner, that those stupendous movements to which the loftiest chain in Europe owes its complicated structure, and by which its component strata have been dislocated, fractured and contorted, belong to a very modern æra in the earth's history. In the long calendar of geological events, the Eocene period is the first which presents us with a fossil flora and fauna, both terrestrial and aquatic, of a very complete character, comprising mammalia both of the sea and land, of all the principal classes, now contemporary with man. It would doubtless be rash to assume that no plants or animals of equally high organization may not have pre-existed on this globe, for the recent progress of discovery in our science puts us on our guard against founding hasty generalizations on mere negative evidence. The fossil skeletons of saurians discovered in the coal-measures of Saarbrück near Treves are still fresh in our recollection, as are those footprints of the same age first detected by Dr. King, and which I have myself examined at Greensburg in Pennsylvania. We are waiting also with impatience for more minute details respecting some reptilian footprints of a still more ancient date, found by Mr. Isaac Lea in the old red sandstone at Pottsville, near Philadelphia; nor have we forgotten the tracks of numerous birds, observed in the red shales and sandstones of Connecticut, of a date nearly bordering on palæozoic times. Such facts, like the unexpected discovery of the Stonesfield marsupials, a quarter of a century ago, warn us against the presumption of taking for granted, that our present knowledge of the earliest occurrence of a particular class of fossils in stratified rocks, can be reasoned upon as if it afforded a true indication of the first appearance of a particular class of beings on the globe.

* Vol. v. Part i. 1848, December.

Nevertheless, with every reservation for the future enlargement of our ideas respecting the comparative perfection of the living creation in our own times and in the remoter ages, we may at least assert, that in the present state of our science the eocene fauna and flora may be contrasted with those of older date, in regard to the more complete manner in which they represent the animal and vegetable creation.

In the chronological classification of the materials composing the crust of the earth, it has been often asked, whether we ought to ascribe to the older tertiary epoch, or to the cretaceous system, the great nummulitic formation of the Alps, and other parts of Europe. This much-controverted question,—one, as I shall presently point out, of the highest theoretical interest, in reference to the hypothesis of the unabated intensity of the existing agents of change,—was declared by M. Boué, some years ago, to be the great problem of the day, and Sir R. Murchison has therefore devoted to its consideration a large portion of his memoir. M. Boué indeed announced in 1847 his own conviction that the nummulitic rocks belonged to the eocene or lower tertiary period, and remarked, in a paper read to the French Geological Society in that year, how much delight Alexander Brongniart would have experienced, had he lived to see one of his boldest and most startling generalizations thus crowned with success*. Al. Brongniart had in fact declared many years before, that the shells of the summit of the Diablerets, one of the loftiest of the Swiss Alps, which rises more than 10,000 feet above the sea, were referable to species characteristic of the eocene strata of the neighbourhood of Paris. He only felt considerable hesitation, he said, in assigning to them so modern a date, because the overlying limestones were so compact and homogeneous as to agree in lithological character with much older secondary rocks.

Several of the most animated discussions which have taken place in this room since 1825, have turned, as you will recollect, on this subject, especially when the fossil shells brought by Mr. Pratt from Biaritz in the Pyrenees were laid upon our table. A decided opinion was then expressed by many of us that the nummulitic series of that southern chain must be referred to the lower part of the eocene group, as it was made clear that the proportion of fossil species common to the Biaritz beds and the chalk was extremely small—

* Bulletin, vol. v. 2nd Series, pp. 69, 71.

much too few to imply a cretaceous age for the strata in question, or even a zoological passage from the cretaceous to the tertiary formations. They who have read with care the successive numbers of the 'Bulletin' of the Geological Society of France, are aware how much that body has been occupied with the same problem, and how steadily the evidence in favour of the same important conclusion has been gaining strength. M. d'Archiac, writing in 1847 on the fine collection of Biaritz shells submitted to his inspection by Mr. Pratt, observed that forty-eight, or one-fourth of the whole series, were identical with fossils of the lower eocene of the Paris basin, while the rest were all tertiary forms except four, which belonged to species of the chalk*. In a paper by M. Deshayes, read to the Geological Society of France in June 1844†, that able conchologist declared, after examining the Biaritz fossils, "that the whole of the nummulitic system must be classed as tertiary; an opinion confirmatory," he said, "of the results previously arrived at by M. Leymerie in the Corbières, and of M. Bertrand Geslin in the Alps." Lastly, I may observe, that you will find similar opinions recorded in the 'Bulletin,' either in the memoirs or verbal comments of MM. Deshayes, Charles Desmoulins, Raulin, Leymerie, Tallavigne, Delbos, Desor, Boué, Archiac, and Alcide D'Orbigny, all published in the course of the last six years. Whether a real transition from the cretaceous to the tertiary strata can be made out, is a point which has also been fully discussed, and how far the Maestricht beds are represented in the Pyrenees. It appears from the researches of MM. Desmoulins and Raulin, that some few of the characteristic fossils of Maestricht have really been found in that chain; but you will, I think, agree with M. Deshayes, that they are not enough to establish the existence of any true equivalent of the Maestricht group—that distinct and uppermost division of the chalk to which the Faxoe coralline limestone in Seeland, as well as the pisolitic strata of Sezanne near Paris, are referable.

When we consider that the age of the nummulitic formation of the Pyrenees, however clearly it may now be determined to be tertiary, has been regarded by so many able authorities as a subject of perplexity and debate up to so late a period, we cannot feel surprised that

* Bulletin, vol. iv. 2nd Series, p. 1006.

† Translated in Quart. Journ. Geol. Soc. 1845, p. 111.

MM. De Beaumont and Dufrénoy, in constructing their geological map of France many years before, should have referred these strata in the Alps, and in the regions bordering the Mediterranean, to an age anterior to the calcaire grossier of Paris, especially when we learn that even now M. Agassiz affirms, that out of 139 species of echinoderms described by him from the nummulitic beds of the Mediterranean, one species only is common to them and the calcaire grossier. The same geologist maintains that all the fish of Glarus and Monte Bolca, which according to the latest opinions must be classed as eocene, differ entirely from those of Sheppey *. Yet I am by no means disposed to question, on the ground of this want of agreement in the ichthyolites, that the Glarus slates are in truth tertiary, still less to doubt that the limestone of Monte Bolca belongs to the same period: I have always regarded the latter as eocene from the time when I visited that locality in company with Sir Roderick Murchison in 1828. You have seen also, in the classification of the three successive eocene formations established by Mr. Prestwich for the older tertiary deposits of Great Britain, that while each division is characterized by its peculiar assemblage of shells, a part only of the species pass from one division to another, and that the specific difference of the mammalia belonging to each division, and still more of the first, as determined by Agassiz, is extremely marked.

The researches, above alluded to, of Sir Roderick Murchison in the Alps in 1847, and the palæontological evidence of various eminent writers brought together by him in illustration of his views, have, I think, shown unequivocally, that, together with the nummulitic limestone, an enormous thickness of overlying strata of dark-coloured slates, marls, and fucoidal sandstones, provincially called Flysch, are separable from the cretaceous system of Northern Europe, and must also be regarded as lower eocene. His attempt however to make out a passage from the tertiary to the secondary series by means of an intervening group of marls, green sandstone and impure limestone, appears to me to be far less successful, since a true representative of the Maestricht beds is wanting in the Alps, or is very ill-defined, and no other equivalent assemblage of organic remains is enumerated sufficiently rich in forms, or intermediate in character, to fill up the wide gap between the eocene strata and the chalk.

* Bulletin, vol. v. pp. 414, 415.

I have dwelt thus at length on the age of the nummulitic series, because its recognition as a tertiary deposit draws with it consequences of the utmost theoretical importance, and is singularly confirmatory of a remark made by M. Desnoyers many years ago in his address to the French Geological Society, namely, "that the more the Alps are studied the younger they grow." This saying was elicited by the admission by competent observers, that certain schistose rocks of great thickness, containing dark writing slates, originally classed as "transition formations" by some of the followers of Werner, and regarded as of palæozoic age, were really secondary. Now we are called upon to go much further; for these same strata belong to the flysch, and therefore constitute what is by no means the base of the eocene system. To the English geologist who is old enough to remember when all the soft clays and loose sands overlying the chalk, some of them containing shells of species identical with those now living, were looked upon as very modern, and as the creations of yesterday, in comparison with the rocks of the higher Alps, it may well appear a startling proposition to learn that the clay of London was in the course of accumulation as marine mud at a time when the ocean still rolled its waves over the space now occupied by some of the loftiest Alpine summits. It will follow, moreover, as a corollary from the same data, as before hinted, that not only the upheaval of the Alps, but all the principal internal movements, dislocations, inversions and contortions of the strata, are subsequent to the origin of the nummulitic deposits, and had not therefore even commenced till great numbers of the eocene vertebrate and invertebrate animals had lived and died in succession.

If the development of so vast an aggregate amount of dynamical agency in times so modern in the earth's history had been confined to a single narrow zone of mountains, it would be a fact of no small significance as invalidating all theories which ascribe such magnificent displays of mechanical force to very remote epochs. But on extending our survey, we find some of the members of this nummulitic series, with their characteristic fossils, playing the same part in the Pyrenees, Apennines and Carpathians, and spreading over a large part of the globe of which the geology is best known. They are met with in full force in the north of Africa, as for example in Algeria and Morocco; they have been traced from Egypt into Asia Minor, and across Persia by Bagdad to the banks of the Indus. They occur not only in Cutch, but in the mountain-ranges which separate Sinde from Persia, and which form the passes leading to Cabul. They have been followed still further eastward into India, and may be said to enter bodily into the structure of all the continental lands and mountain-chains of the Old World.

Were we to endeavour to estimate the changes in physical geography which can be proved by the position of these marine eocene strata to have occurred since the commencement of the tertiary period, we should find them to be very inadequately expressed by stating that they equal in amount the conversion of sea into land of a continent as large and lofty as that of Europe, Asia, and the north of Africa. I endeavoured in 1834, in a map constructed for the 3rd edition of my 'Principles of Geology,' to show the extent of surface in Europe and part of Asia which had been covered by water, at some time or other, since the beginning of the eocene period. But, had I been then aware that a true pictorial representation of such modern revolutions in physical geography would have required the submergence of the Alps, Pyrenees, Apennines and Carpathians, and the insertion of a few insignificant islands only in their place, I might have thought such an illustration superfluous or without meaning, and have been satisfied by simply insisting on the post-eocene ubiquity of the ocean—not indeed by a simultaneous, but by a successive occupancy of the whole ground. But how small a portion even of the superficial remodeling of the earth's crust in recent times is expressed, by declaring that we can establish by direct proof or legitimate inference the upheaval out of the sea of all the land in Europe, Asia, and part of Africa! During the same tertiary periods there have been vertical subsidences as well as elevations of the same areas; and we have every reason to believe that the larger part of the globe (comprising nearly three-fourths of its superficies), which is covered by water, has undergone, in equal periods of time, oscillations of level not inferior in degree to those to which the continental spaces have been subjected. If therefore we were to confine our thoughts to the mere outward modifications in the shape of the land or bed of the sea, and all the changes of climate and fluctuations in organic life inseparably connected with movements which have amounted, in some cases, to more than two miles vertically in one direction, besides the lateral displacement of rocks and their denudation by water, the series of events would seem endless, and their magnitude not easily to be exaggerated. But it is evident that these superficial mutations are trifling in amount in comparison with revolutions which must have been going on simultaneously in the inferior parts of the earth's crust. The reality of these changes is certain, although their nature may be obscure; for we can rarely catch even a glimpse of the subterranean products of the eocene, miocene and pliocene epochs, because it requires far more time than the tertiary periods have as yet furnished, to allow the disturbing causes to uplift, depress, and rend open, or for the ocean to denude the incumbent rocks so as to make it possible for an inhabitant of the surface to behold them and appreciate their magnitude.

The Alps indeed, where the convulsions have been greatest, reveal to us some monuments of the vast chemical changes and re-arrangement of the component elements of rocks which have taken place since the deposition of the eocene strata, and we thus gain some insight into the nature of the transformation of mineral masses which must have been going on contemporaneously at greater depths. It appears, for example, that in some places granite has been intruded into the axis of the Alpine chain, and that in other places various granitiform compounds have been formed since the whole nummulitic formation was elaborated beneath the sea. "In passing," says Sir R. Murchison, "from east to west, from the Austrian into the Savoy Alps, the zone of metamorphism widens laterally, from the centre to the flanks of the chain, so as to affect even the younger secondary deposits, and in one or more tracts even the tertiary, some of the strata called flysch being converted into a crystalline state*." Instances are also adduced in the Bernese Alps (by the same author) of bands of granite or granitic schists in the midst of the flysch, demonstrating that the action of heat and vapours, or the causes commonly called plutonic, have changed even these modern deposits into gneiss, as well as into quartz rock and mica schist†.

To whatever geological period we may be disposed to assign the first origin or crystallization of the talcose granite and gneiss of Mont Blanc and other parts of the central nucleus of the Alps, we cannot doubt that they broke through the crust and were protruded into

* Quart. Journ. of Geol. Soc. vol. v. p. 164. † Ibid. vol. v. p. 213.

the atmosphere, or were laid bare by denudation, after the nummulitic limestone was formed, and consequently after the beginning of the eocene period. For my own part, I have little doubt that these granites are all tertiary, and that they may even have passed from a fluid or semi-fluid state to their present form at an epoch more modern than the eocene period. But although it is only in a few narrow strips of country, like the Central Alps, that nature discloses to us some of the nether-formed rocks of such modern geological æras, we cannot doubt that still greater modifications of the interior have extended downwards for many miles or leagues in depth beneath the Alps, and beneath every region, whether of land or sea, which has risen, sunk, or oscillated in level since the fossil shells and zoophytes of the lower eocene period were living in the sea. The imagination of the geologist strives in vain to form a just conception of the extent of these internal modifications of the crust, of which we are only beginning to interpret the outward signs. How much fracture and dislocation of solid rock must have taken place! how much heating and cooling, expansion and contraction, drying and baking, softening and re-solidifying of sedimentary strata! Over how vast an area, and to how great a depth, often hundreds of yards or several miles beneath the surface, have mineral masses been injected by lava, or dissolved by thermal waters, or corroded by acids, or permeated by steam, or impregnated with magnesia, sulphuric acid, or other substances introduced in a gaseous form! What obliteration has there not been of organic remains, and of the signs of stratification, in the course of the tertiary ages which have elapsed since the nummulitic strata and incumbent fucoid grits lay submerged beneath the ocean!

Sir Roderick Murchison has given a graphic description of the foldings, so sharp and so often repeated, of a grand succession of sedimentary strata in the Alps. Among other examples, he has cited one case of extraordinary inversion of large masses in the canton of Glarus, examined by himself and M. Escher, where a limestone of the Jurassic period containing ammonites is, on the one hand, "overlaid by a zone of talc and mica schist, having in parts quite the aspect of a primary rock;" while in another direction it is continuously superimposed for miles on beds of highly inclined flysch of eocene age*.

* Quart. Journ. of Geol. Soc. vol. v. p. 246.

It seems that in the course of the stupendous movements which have raised these modern beds to the height of 8000 feet above the sea, and caused portions of them to become crystalline or metamorphic, large masses of the solid Jurassic limestones of the Oxfordian age have been pushed bodily out of their place, and planted unconformably on the edges of strata of the nummulitic series. Our indefatigable colleague naturally shrinks from offering any explanation of so marvellous and anomalous a state of things, extending as it does over a considerable area. In attempting to estimate such gigantic movements, the powers of imagination, he says, are at fault; and "surely," he adds, "it is not unphilosophical to believe that in those days the crust of the earth was affected by forces of infinitely greater intensity than those which now prevail." In particular, he regards the apparent inversion of the tertiary molasse along the flanks of the Alps, and its great elevation, as "a clear demonstration of a sudden operation or catastrophe*."

Now, I shall first venture to remark, in regard to these theoretical views, that the Alps, when considered as a mountain-chain which has originated entirely since the commencement of the tertiary period, bear emphatic and irrefragable testimony to the fact, that the intensity of the causes which have disturbed the crust of the globe has not diminished in the tertiary as compared to the secondary or primary fossiliferous epochs. It may possibly be still contended, that the energy and violence of the movements were more general in those earlier epochs, supposed by some to have been close upon the confines of "the reign of Chaos and Old Night;" but it cannot be pretended that there are any proofs of a more magnificent development of the disturbing forces in any given region of equal extent, and accomplished in an equal lapse of time, at any period antecedent to the upheaval of the Alps. If, however, any one should maintain, that in the earlier ages the movements which upheave, depress and derange the position of strata were more general, and that they agitated simultaneously much wider horizontal areas, it will be easy to adduce the most overpowering evidence to the contrary. The wide extent in the United States of America, and in parts of Russia, of Carboniferous, Devonian and Silurian strata, which although upraised above the sea, continue almost as level as when the beds were first thrown

* Quart. Journ. of Geol. Soc. vol. v. p. 258.

down beneath its waters, clearly demonstrates the limitation of the agency to which great foldings and contortions of stratified rocks have been due to very confined spaces in each epoch. Were it otherwise, the multiplication of such extensive convulsions during a long succession of ages would have made it impossible to find any spot on the globe where the oldest rocks had escaped extreme derangement. It only remains therefore for the advocates of the paroxysmal hypothesis to assert that, although the disturbing forces have by no means grown feebler in the modern or tertiary times, as compared to periods when the oldest of the known strata were deposited, yet there have been brief æras of convulsion on a very grand scale, when the ordinary repose of nature was violently interrupted in particular regions (as in the Alps, for example) in a manner wholly different, in regard to the magnitude of the effects produced, from any which we have witnessed in historical times, or which ever occurred formerly during the ordinary and normal state of the globe.

That doctrines of this kind are popular, I am well aware; and if you desire to know how many modern writers have declared in their favour, I refer you to the excellent work which has just been published by one of our foreign members, M. d'Archiac, on 'The History of the Progress of Geology from the years 1834 to 1845.' He has executed conscientiously nearly half of the laborious and delicate task assigned to him by the Geological Society of France, and has given us a faithful digest of memoirs written in a variety of languages and scattered through the Proceedings and Transactions of numerous scientific bodies, or the periodical magazines and journals of almost every civilized country. A geologist of practical experience in the field, as well as of extensive erudition, was required to make a good classification of such complex materials, and justly to appreciate their relative value. In M. d'Archiac's pages every author of merit has been allowed an impartial hearing, and the expositor's own occasional criticisms are not obtruded too prominently on the reader's attention; when they are offered, they are so judicious as to aid us materially in understanding the faithful analysis he has given of the opinions of others. In the concluding part of his chapter on "Le terrain moderne," and when speaking of active volcanos, and in other places, he stoutly denies the adequacy of the causes which have modified the earth's crust in historical times to produce effects such as may enable us to explain geological monuments. "We must have recourse," he says, "to other causes, both organic and inorganic, of a more energetic and even paroxysmal character*."

On this subject I must make two preliminary remarks: First, that our present inability to decipher some of the monuments of past ages by a key derived from the effects of causes now acting, ought never to be adduced as an argument of much weight in favour of the paroxysmal theory; for it might with equal or greater propriety be urged as a reason for believing in the adequacy of existing causes, or their identity with those of former times, since no one doubts that we are ignorant of the nature of many subterranean and suboceanic changes now in progress. If therefore there was nothing obscure or mysterious in geological phænomena, if they simply presented to us a picture of objects as familiar as the lavas of Vesuvius or the calcareous tufas of mineral springs, or the newly-formed deposits of a delta seen at low water, we should be entitled to suspect a great want of analogy between the ancient and modern processes at work above and below the earth's surface. We should then be entitled to ask, where are the nether-formed and deep-sea formations of the olden time? Where are the signs of those changes brought about in the bowels of the earth corresponding to such as are now in progress in regions inaccessible to human observation? Why have not the causes which have upheaved mountains and deeply fissured the rocks, or which have denuded large areas, revealed to us ancient stratified and unstratified rocks, wholly distinct from any which we now see generated by ordinary volcanic action or formed in lakes and shallow seas. Secondly, it should be thoroughly understood that the decision of the question at issue can in nowise be determined by simply comparing the magnitude of the changes brought about in historical times with those of antecedent periods. It may be safely affirmed, that the quantity of igneous and aqueous action,—of volcanic eruption and denudation,—of subterranean movement and sedimentary deposition,—not only of past ages, but of one geological epoch, or even the fraction of an epoch, has exceeded immeasurably all the fluctuations of the inorganic world which have been witnessed by man. But we have still to inquire whether the time to which each chapter or page or paragraph of the earth's autobiography refers, was not equally

* Archiac, Hist. des Progrès, &c. tome i. pp. 209, 670.

immense when contrasted with a brief æra of 3000 or 5000 years. The real point on which the whole controversy turns, is the relative amount of work done by mechanical force in given quantities of time, past and present. Before we can determine the relative intensity of the force employed, we must have some fixed standard by which to measure the time expended in its development at two distinct periods. Dr. Whewell has justly observed, that "mechanical power retains its amount, however much it be distributed through time and divested of the character of extraordinary violence*,"—a principle which should never be lost sight of when we contrast the effects of the historical with those of antecedent epochs. It is not the magnitude of the effects, however gigantic their proportions, which can inform us in the slightest degree whether the operation was sudden or gradual, insensible or paroxysmal. It must be shown that a slow process could never in any series of ages give rise to the same results.

The advocate of paroxysmal energy might assume an uniform and fixed rate of variation in times past and present for the animate world that is to say, for the dying-out and coming-in of species, and then endeavour to prove that the changes of the inanimate world have not gone on in a corresponding ratio. But the adoption of such a standard of comparison would lead, I suspect, to a theory by no means favourable to the pristine intensity of natural causes. That the present state of the organic world is not stationary can, I think, be fairly inferred from the fact, that some species are known to have become extinct in the course even of the last three centuries, and that the exterminating causes always in activity, both on the land and in the waters, are very numerous; also, because man himself is an extremely modern creation; and we may therefore reasonably suppose that some of the mammalia now contemporary with man, as well as a variety of species of inferior classes, may have been recently introduced into the earth, to supply the places of plants and animals which have from time to time disappeared. But granting that some such secular variation in the zoological and botanical worlds is going on, and is by no means wholly inappreciable to the naturalist, still it is certainly far less manifest than the revolution always in progress in the inorganic world. Every year some volcanic eruptions take

* Quart. Journ. Geol. Soc. vol. iii. p. 231.

place, and a rude estimate might be made of the number of cubic feet of lava and scoriæ poured or cast out of various craters. The amount of mud and sand deposited in deltas, and the advance of new land upon the sea, or the annual retreat of wasting sea-cliffs, are changes the minimum amount of which might be roughly estimated. The quantity of land raised above or depressed below the level of the sea might also be computed, and the change arising from such movements in a century might be conjectured. Suppose the average rise of the land in some parts of Scandinavia be five feet in a hundred years, the present sea-coast might be uplifted 700 feet in fourteen thousand years; but we should have no reason to anticipate, from any zoological data hitherto acquired, that the molluscous fauna of the northern seas would in that lapse of years undergo any sensible amount of variation. If a botanist were asked how many earthquakes and volcanic eruptions might be expected, and how much the relative level of land and sea might be altered, or how far the principal deltas will encroach upon the ocean, or sea-cliffs recede from the present shores, before the species of European forest-trees die out, he would reply that such alterations in the inanimate world might be multiplied indefinitely before he should have reason to anticipate, by reference to any known data, that the existing species of trees in our forests would disappear and give place to others. In a word, the movement of the inorganic world is obvious and palpable, and might be likened to the minute-hand of a clock, the progress of which can be seen and heard, whereas the fluctuations of the living creation are nearly invisible, and resemble the motion of the hour-hand of a timepiece. It is only by watching it attentively for some time, and comparing its relative position after an interval, that we can prove the reality of its motion. If therefore in the coal-measures of South Wales or Nova Scotia we find the same fossil trees repeated through a mass of strata formed in shallow water 10,000 feet thick, we ought not to feel surprised, but merely conclude that formerly, as now, the rate of change in the vegetable kingdom was extremely slow, so that a stupendous mass of stratified sand and mud, as well as great revolutions in physical geography, might be slowly effected, without there being time for any important fluctuation to be brought about in the species of plants inhabiting the globe.

I have endeavoured to show in my 'Second Visit to the United

States*,' that a great oscillation of level has taken place in the valley of the Mississippi and its tributaries, by means, first, of a slow downward movement, and then of an ascending one, and that the whole was accomplished since the period when the freshwater and land-shells now inhabiting that great valley were already in existence. We ought not therefore to be surprised when we discover sea-beaches in Norway 700 feet high, in which the shells are identical with those now inhabiting the German Ocean; for we have already seen that the rise of land in Scandinavia, however insensible to the inhabitants, is rapid when compared to the rate of contemporaneous change in the testaceous fauna. Were we to wait therefore until the mollusca shall have undergone as much fluctuation as they underwent between the period of the liassic and upper oolite formations, or still more between the oolite and chalk, or between the Wealden and eocene strata, what stupendous revolutions in physical geography ought we not to expect, and how many mountain-chains might not be produced by the repetition of shocks of moderate violence, or by movements not even perceptible by man! I may take this opportunity of stating, in reference to the permanent effects of subterranean movements in our times, that in all likelihood we are always in danger of underrating their intensity, because we can only measure their amount on the sea-coast, whereas the adjoining mountain-chains seem generally to be more shaken by earthquakes, and probably undergo a greater change of level than the low countries.

Let us now return to the Alps, and inquire whether geologists who ascribe their origin to paroxysmal forces have been able of late years to bring to light any new facts in support of their favourite doctrine. On the contrary, if I mistake not, they have been more and more compelled to assign the time during which the disturbing power was exerted to a succession of distinct geological periods, in some of which the force must have operated very slowly, while in other cases where it was sudden it may probably have been intermittent, and consisted, as in ordinary volcanic action, of a repetition of shocks or explosions of moderate intensity. In illustration of these principles, I may first mention that some of the volcanic eruptions of the Alps, which produced the porphyry called melaphyre, broke out again and

* Vol. ii. chap. xxxiv.

again, as M. Favre has demonstrated, in the sea of the Jurassic period, and they were accompanied and followed by metamorphic action, occasioned by gaseous emanations. The tuffs and trap dikes of Monte Bolca and the Vicentine show that other volcanic eruptions poured out lava and ejected scoriæ into the waters of the eocene sea. Again, after this period, the protrusion, if not the formation, of the talcose granite, or protogene of the central nucleus of the Alps, occurred. The upheaval of nearly the whole mountain mass, from the waters of the eocene sea to an elevation of more than two miles above its level, happened subsequently to the deposition of all the nummulitic beds and the flysch. These latter deposits, thousands of feet in thickness, shared, after the commencement of the tertiary period, in all the movements, whether slow or convulsive, to which the Alpine rocks owe their curvatures, dislocations, and vertical or lateral displacement. The grand sinking-down of the nagelflue or conglomerate of the molasse, more than a-mile vertically, belongs again to a still later period, which did not begin till all the eocene movements had terminated, and was due to a gradual subsidence along the whole northern flank of the chain. At a still more modern æra, the entire upheaval of the same molasse took place, so that it reached at length its present altitude of 3000 or 4000 feet above the sea. Nor did the uplifting agency cease here, for it continued till the newer or subapennine tertiary beds were made to emerge. There are proofs indeed of the relative level of sea and land having been modified even after the erratic blocks were conveyed to their present sites, or subsequently to the glacial period of Northern Europe.

This assignment to a great number of distinct and separate periods of the work done by the moving and disturbing powers, is by no means the result of the study of the Alps exclusively. In other mountain-ranges it is now ascertained that the upheaving and depressing forces have been propagated in succession along the same parallel zones of country; and M. Élie de Beaumont has frankly confessed that he was in error when he first pronounced the Pyrenees to be a chain due to a single upthrow, "un seul jet," or "une chaîne élevée en une seule fois." He and M. Dufrénoy now go so far as to agree with M. Durocher, that in the Pyrenean chain, in spite of the general unity and simplicity of its structure, six, if not seven systems of dislocation, each chronologically distinct from the other, can be made out*.

In regard to the Alps, it appears from the observations of Leopold Von Buch, Sir Roderick Murchison, and others, that whatever be the major axis of the crystalline mass in the centre, such also is the prevailing direction of all the sedimentary deposits which lie on either side of the chain. Whether the axis be composed of granite, syenite, gneiss, mica-schist, marble, dolomite, or of any rock formed by eruption or by the metamorphism of pre-existing strata, there is obviously some connection between the position of the central crystalline nucleus and the dominant strike of the flanking deposits. It is as if the intrusion of the igneous matter at certain periods had not only raised the chain, but so injected and distended its central parts, as to force outwards the pliant strata on each side, and to cause them to fold themselves into parallel anticlinal and synclinal flexures.

The theory first proposed by Von Buch, of the conversion of mountain masses in the Tyrol and other parts of the Alps into dolomite, and of other limestones into gypsum, has been gradually embraced by the majority of the most eminent geologists who have carefully examined the great chain. The porous and cavernous nature of the dolomite are referred to by MM. É. de Beaumont and Morlot as a character implying the alteration of a compact rock into one of more open texture which had been permeated by gases†. "It is now more than twenty years," says De Beaumont, writing in 1847, "since I first advocated Leopold Von Buch's views, who attributed the gypsums and dolomites of the Alps to *épigénie*, or to the alterations of calcareous masses by mineral springs and gaseous emanations which came up from the interior of the earth at the time when the porphyries called melaphyre were formed‡. M. Frapolli, in reference to similar metamorphic action, has adduced numerous facts illustrative of the manner in which carbonates of lime may have been turned by sulphurous vapours into gypsum; and Sir R. Murchison reminds us that the well-known thermal waters of Aix do now actually change the ordinary Jurassic limestone into sulphate of lime; while, according to M. Coquand, another example of the like metamor-

* Bulletin, 2nd Series, vol. iv. p. 1368.

† Ibid. vol. vi. p. 318.

‡ Ibid. vol. iv. p. 1282.

phism is afforded by Mofettes, where the sulphuro-hydrous emanations turn the cretaceous limestone into gypsum along the lines of fissure which they permeate*. M. Favre, as before stated, has shown that the period when the porphyries called melaphyre were erupted agrees well with this hypothesis, and that the heat and gases disengaged during such volcanic outbursts might well have transformed the calcareous into magnesian rocks. Thus it is supposed that the carbonate of lime containing shells of the Jurassic epoch has been slowly transformed into magnesian carbonate, and perhaps an increase of volume was gradually acquired by the gypseous and dolomitic masses in proportion as they derived fresh accessions of mineral matter from below. If so it may have caused expansion, and have furnished an irresistible lateral pressure.

If in the central parts of the Alps we suppose heat to have accompanied the metamorphic action which has converted into gneiss and mica-schist, not only the Jurassic and cretaceous, but even certain eocene strata, this same heat must have caused many kinds of rock to expand, and might, in this manner, slowly give rise to the sideway thrust exhibited in the curved beds on either flank of the chain. It is now known that granite and sandstone, while solid, expand and contract, even under such a range of atmospheric temperature as the difference of a Canadian winter and summer produce. We must also take into account that highly inclined or vertical argillaceous strata, such as the flysch, would shrink when heated, and give off their water; while other rocks, ranged side by side, might be simultaneously expanding or partially melting, so as to occupy more room, and that the clays might thus be pressed into solid shales and acquire irregular and complicated curves. The irregularity and confusion would be greatly increased by local variations in the composition of the stratified deposits, whether in the direction of their strike or dip, and also by the unequal intensity of the heating and cooling processes, whether the central be compared with the lateral parts of the chain, or the superficial with the internal parts. Yet we cannot feel sure, that were such mighty changes now in progress in any range of mountains subject to earthquakes, such as the Andes or Himalaya, we could guess at the direction of the movement, for the contraction or expansion of mineral masses might be carried on as

* Bulletin, 2nd Series, vol. vi. p. 124.

slowly as the growth of a tree or the swelling of its roots in the soil.

M. de Beaumont, in his essay on volcanic and metalliferous emanations*, observes that, according to the experiments of Deville, the contraction of granite in passing from a melted or plastic to a solid state must be more than ten per cent. We have here then at our command an abundant source of depression on a grand scale at every geological period in which granitic rocks have originated. All mineralogists seem agreed that the passage from a liquid or pasty to a solid and crystalline state cannot, in such cases, have been instantaneous throughout voluminous masses; yet by suddenly crystallizing alone could it have given rise to the paroxysmal downthrow of overlying rocks. On the contrary, every hypothesis seems to proceed on the assumption that the crystallization of granite was an extremely gradual process. Many very instructive speculations on this head will be found in the writings of Scheerer, Frapolli, Fournet, Durocher, De Beaumont, and others, who have attempted to explain the reciprocal penetration of the crystals of quartz and felspar which enter into the composition of granites. These minerals, as is well known, have crystallized in an order independent of their relative fusibility, the quartz not only imprinting its form on the felspar, but sometimes itself receiving the imprint of the crystals of felspar. Gaudin and Fournet, in order to account for this fact, have shown that dissolved flint may cool without solidifying, and remain in a gelatinous state, and thus crystallize after the felspar and mica; while M. de Beaumont has suggested that electric action may prolong the duration of the viscosity of silex†.

The conglomerate of the molasse called nagelflue, before alluded to, and referred to the miocene, if not in part at least to a still later (pliocene) date, attains in some places a truly wonderful thickness, exceeding 6000 and even 8000 feet. It is very conspicuous in the Rigi and in the neighbourhood of Lucerne, as well as in the Speer near Wesen. The lower part of the group, containing terrestrial plants, fluviatile shells, and the bones of extinct land-quadrupeds, is considered by M. Escher as a freshwater formation, while some of the sandstones and marls of the upper members of the series contain

* Bulletin de la Soc. Géol. 2nd Series, vol. iv. p. 1312.
† Bulletin, 2nd Series, vol. iv. p. 1022.

marine shells*. To explain the origin of such a succession of pebbly strata, we are naturally referred, by Studer, Escher, Sir R. Murchison, and others, to a long-continued depression along the whole external northern face of the Alps. Numerous torrents are supposed to have issued from the islands which then occupied the site of the loftiest portions of the chain, and the continuity of the strata is explained by imagining them to have accumulated on a shelving shore like that of the present maritime Alps. At first the materials must have been arranged in beds which sloped away from their parent rocks of the Alps; yet after sinking successively to enormous depths, they have been brought up again, so as to dip towards the older rocks, as if they passed under them.

The first part of this grand subsidence of the sea-bottom was doubtless analogous to that now in progress on part of the coast of Greenland. But if the adjoining land participated in the same downward movement, it is difficult to conceive how it escaped being submerged, or how it could continue to retain its size and altitude so as to continue to be the source of such an inexhaustible supply of pebbles. We can scarcely avoid speculating on a contemporaneous slow upheaval of the mountains. There may have been an ascending movement in one region, and a descending one in a contiguous parallel zone of country, as the northern part of Scandinavia is now rising while the southern portion in Scania is sinking, or at least has sunk within the historical period. Perhaps the not uncommon occurrence, of deep sea in the immediate vicinity of bold coasts and mountain-chains, may be connected with extensive lines of fault, parallel to the shores, on the opposite sides of which, vertical movements may be taking place in contrary directions, or one side may be motionless, while the other is subsiding. In no other way does it seem possible to account for the proximity, throughout a long series of ages, of high land, and of a sea-bottom always going down so gradually as to remain for a long time the receptacle of annual tributes of rolled pebbles, and acquiring in the end a thickness of 5000 and 8000 feet. In regard to faults which have shifted rocks several thousand feet in a vertical direction, it is often too hastily assumed that they must have been produced suddenly; whereas the reverse is indicated by the fact that the walls of such faults are rubbed, polished and striated, as if they had been

* Murchison, ibid. p. 229.

subjected to friction long continued or many times repeated. The mass moreover of fragmentary matter usually included between the opposite walls of such rents is partly reduced to fine clay or dust, and partly filled with stones which have been superficially scored in various directions.

The minute study of the structure and organic contents of strata of various ages, has made us of late years more and more familiar with the hypothesis of a slow sinking of the ancient floor of the ocean going on while it was receiving repeated accessions of sediment. We must not forget that in all such cases a solid foundation of subjacent rock of unknown depth, and perhaps much older than the newly superimposed deposit, is undergoing simultaneously a change of position, and that rocks still lower are undergoing, whether by cooling or crystallizing, a change of structure. These very gradual movements are quite as remarkable in the palæozoic as in the tertiary periods. By consulting the 'Memoirs of the Geological Survey of Great Britain,' you will learn that in Wales, and the contiguous parts of England, a maximum thickness of 32,000 feet (more than six miles), of carboniferous, Devonian and Silurian beds, has been measured, the whole formed whilst the bed of the sea was continuously and tranquilly subsiding. In illustration of a movement of the same kind, I need scarcely remind you of the coal-measures of South Wales, with their numerous under-clays, each containing Stigmaria, a phænomenon to which Mr. Logan first drew our attention. Mr. Binney of Manchester has since proved to us that all these Stigmariæ, found in the floor of every coal-seam, are the roots *in situ* of fossil trees, chiefly of the genus *Sigillaria*, and that they are occasionally attached to their stems or trunks,—a conclusion fully confirmed by the more recent observations of Mr. Richard Brown on the coal-fields of Nova Scotia. Sir Henry De la Beche also, in his paper on the rocks of South Wales and the South-west of England, confirms these statements, and shows that subsidences of vast amount took place slowly during the accumulation of the palæozoic strata, the sea all the while remaining shallow, in spite of a depression of one or two miles. Still later, Professor John Phillips, in the second volume of the same 'Survey,' has pointed out analogous phænomena in the old red sandstone of the Forest of Dean; and these strata, 7000 feet thick, are described as having been formed in a sea of moderate depth. Fossil corals and shells imbedded as they grew, or ripple-marked sandstones and sandy or gravelly strata with subordinate diagonal layers, confirm these views. Such movements took place contemporaneously with the growth of organic matter, just as subsidence on a grand scale is now going on over vast areas in the Pacific and Indian Oceans,—a class of facts on which Mr. Darwin has founded his theory of atolls, or the origin of annular coral islands with lagoons. His theory, as you have probably observed, has been recently embraced and more fully elucidated by Mr. Dana, in his valuable chapters on the geology of the American Exploring Expedition under Capt. Wilkes.

The investigations of Professor Edward Forbes, on the laws governing the distribution of marine animal life, at various depths in the Mediterranean, have powerfully aided us in determining the conditions under which particular strata were formed, the depth of water being deducible from a careful study of the organic contents of each bed. Availing themselves of this key, Captain Ibbetson and Professor Forbes have shown how the lower cretaceous strata of the Isle of Wight have been deposited on a gradually sinking submarine bottom, while Mr. Prestwich has applied the same method of reasoning, with equal success, to the eocene strata of Alum and Whitecliff Bays in the same island*. In this instance it is remarkable, that after a depression of 1800 feet very slowly effected, there was still contiguous land inhabited by the Palæothere of Binstead and Hordwell and its contemporaries, as well as a freshwater estuary, implying that the movements in different parts of that region were either very unequal or opposite, or that they consisted of great oscillations of level. It would be easy to cite a variety of continental authorities in support of the same principle, but enough has been stated to entitle me to ask, whether the subsidence of mountainous masses, lying immediately beneath the floor of the ocean, brought about by such slow degrees, can possibly occur, without causing beneath many of the sunk areas, vast flexures of the strata, which as they sink for miles vertically must occasionally be forced to pack themselves into smaller spaces than those which they previously occupied. If this be true, the contortions and foldings of pliant beds, and the fracture and dislocation of the more unyielding rocks, have frequently been due to movements as gradual as those of various ages to which I have been alluding.

* Prestwich, Quart. Journ. Geol. Soc. vol. ii. p. 223.

The imagination may well recoil from the vain effort of conceiving a succession of years sufficiently vast to allow of the accomplishment of contortions and inversions of stratified masses like those of the higher Alps; but its powers are equally incapable of comprehending the time required for grinding down the pebbles of a conglomerate 8000 feet in thickness. In this case, however, there is no mode of evading the obvious conclusion, since every pebble tells its own tale. Stupendous as is the aggregate result, there is no escape from the necessity of assuming a lapse of time sufficiently enormous to allow of so tedious an operation. No intervention of a cataclysm or series of paroxysmal waves can avail us; and if the geologist could abridge the period, he would find that far from being a gainer, he had deprived himself of the only means ever yet suggested of explaining another set of geological monuments, relating to what we term denudation. It is not simply by fixed and permanent inequalities of level, in the land and sea, or by the alternation of dry and rainy seasons, or of summer heat and winter's frost, that the aqueous action of torrents, rivers, breakers, tides and currents acquires a sustained energy, capable of denuding wide areas, but by the gradual elevation or subsidence of continents and islands, occasionally accompanied by many minor oscillations of level. It is by reiterated slight variations in the position of a coast line, by the continual shifting of the points of attack, that every portion of the surface of the land is exposed by turns to denudation, and is prevented from ever settling into a state of equilibrium and cessation from waste. If earthquakes agitate the country from time to time, while it is rising or sinking, so as to block up valleys and cause temporary lakes and fissures, or the fall of river-cliffs and sea-cliffs, the power of aqueous destruction will be still further augmented.

In the first volume of the 'Memoirs of the Survey of Great Britain,' Professor Ramsay has shown that the missing beds, removed from the summit of the Mendips, must have been nearly a mile in thickness, and he has pointed out considerable areas in South Wales and some of the adjacent counties of England, where a series of palæozoic strata not less than 11,000 feet in thickness have been stripped off. All these materials have of course been transported to new regions; and when it is shown by observations in the same 'Survey' that the palæozoic strata are from 20,000 to 30,000 feet thick, we have a counterpart of older date of denuding operations on a scale of similar grandeur, for what has been carried away or borrowed from one space must always have been given to another. The gain must always have equalled the loss, and sediment deposited in one area must be the measure of the quantity of pre-existing rock cleared away elsewhere. The announcement of this principle may seem, perhaps, like insisting on a truism, but I find it necessary, because in many geological speculations I observe it is taken for granted that the external crust of the earth has been always growing thicker, in consequence of the accumulation of stratified rocks, as if they (and possibly the contemporaneous rocks of fusion, in progress far below) were not produced at the expense of pre-existing rocks, stratified and unstratified. Whether indeed the trap and granite of successive ages were formed by the melting of matter previously solidified, will be questioned by those who contend that the globe was originally a fused mass, and who also assume (still more gratuitously as appears to me) that geological monuments have reference to the period when the melted nucleus was passing to a more and more solid state. But even those geologists must admit that strata of the old red sandstone, or of any other ancient or modern rock of mechanical origin, imply the transportation from some other region, whether contiguous or remote, of an equal amount of solid material, so that the stony exterior of the planet has always grown thinner in one place whenever by accessions of new strata it has acquired density in another. The vacant space left by the missing rocks, after extensive denudation, may be less imposing to the imagination than a vast thickness of conglomerate or sandstone, or the bodily presence as it were of a mountain-chain, with all its inclined and curved strata; but the denuded tracts speak a clear and emphatic language to our reason, and like mountain masses of fossil nummulites, or of corals and shells, or seams of coal based on under-clays full of Stigmaria and surmounted by erect fossil trees, demand countless ages for their origin, and these ages supply the time in which continents and mountain-chains may rise and sink, without sudden, instantaneous or paroxysmal action.

I have already alluded to the slow crystallization and consequent contraction of granitic mixtures, and to the expansion of solid rocks by heat, and to the melting of stony masses, together with various metamorphic agencies, as the causes of slow and gradual movement, both vertical and horizontal. Formerly, when the stratified materials of the Alps presented to the eye of every observer a confused heap of ruin, before any general laws governing the lines of longitudinal fracture, or the parallel foldings of the strata, were caught sight of, it might be argued, that such chaotic disorder implied one or more paroxysmal outbursts of subterranean force, wholly different from ordinary volcanic or any other known agency. But Sir Roderick Murchison agrees with an eminent foreign member of this Society, Professor H. D. Rogers of the United States, and with several Swiss geologists of distinction, that the dislocations and lateral movements of Alpine strata have been obviously regulated by general movements, in which system and law can be discovered. Mr. Rogers, you will remember, declared in this room, when describing the structure of the Alps and Jura, that he recognized a striking analogy between the form of the flexures discernible in these European chains and those observed by him and his brother in the Appalachians of North America. In both cases the successive parallel folds have on one side a steep, short dip, while the other side of the anticlinal flexure is longer and less inclined. This longer side, in the Appalachians or Alleghanies, dips towards the belt of intrusive volcanic rocks on the south-east flank of the chain. So in the Alps, the steep, short dips do not face the crystalline nucleus, but the longer and less inclined ones, except where a curve has been so great that the whole are made to dip one way, the more steeply inclined side having become as it were more than vertical.

In the Alps, the anticlinal folds, where they are greatest, dip inwardly towards the central peaks, and therefore in opposite directions on each flank of the chain. In the Jura, the steep, sharp dips of each parallel fold are upon the side, facing the Alps, and hence Professor Rogers imagines that the subterranean undulations in the earth's crust, which, according to his theory, gave rise to these flexures, were propagated, not from the Alps, but from the district of the Vosges, or the country towards the north-west. To this theory Professor A. Guyot strongly objects, arguing that it is more probable, on the contrary, that the immediate cause of the uplifting of the Jura is to be sought in the upheaval of the Alps. "The elevation," he remarks, "of the anticlinal ridges of the Jura diminishes gradually and regularly in proportion as the Jura recedes from the Alps, the summits sinking from 5000 to 2000 feet. The minor chains also of which the system of the Jura is composed are not exactly in the direction of the system itself, but oblique in such a manner as to be parallel with the chain of the Alps." There is in fact an intimate relation between the two chains, and M. Guyot conceives that the movement has been the result of a contraction of the terrestrial surface in consequence of gradual cooling, and that the folding has been due to lateral pressure resulting from this contraction.

It is not my purpose to enlarge at present on the rival theories thus brought forward to solve a most difficult problem; and I confess myself unable at present to understand how, according to the hypothesis of Mr. Rogers, the grand flexures of the strata in mountain-chains can bear any intimate relation to great waves propagated through a subjacent reservoir of fluid matter. But if M. Guyot be correct in contending that a sinking-down of strata by gravity, owing to a slow contraction of part of the earth's crust below, can explain the flexures, we have then a cause introduced which might act as insensibly as the failure of support, so often witnessed in mines, especially after the removal of seams of coal. Such failure gives rise to what the miners call "creeps," which clearly prove that the sharpest bends and curvatures of yielding strata may be brought about by imperceptible degrees. Even if such an hypothesis be entitled, on pure mechanical principles, to equal favour, it should be preferred to one which appeals to extraordinary violence, for it must then be admitted that the "*dignus vindice nodus*" has not yet occurred.

I have already suggested that the talcose or protogene granites of the Alps may belong to the tertiary period. M. de Beaumont believes that they were not protruded into the atmosphere till they had already reached the region of perpetual snow. Whether there may be good grounds for such an opinion or not, it does not appear to me to follow that such granites may not have been solidified at a considerable depth in the bowels of the earth. No sufficient reason seems to have been advanced to prove that they ought to be regarded, as the French geologist seems to infer, almost as superficial products*. The limestones, sandstones and shales of the nummulitic and flysch series are of such enormous thickness, that tertiary granites may well be supposed to have crystallized beneath them, and then to have been exposed to view by breaking forth or bursting through

* Bulletin, 2nd Series, vol. iv. p. 1299.

the covering of sedimentary matter in the course of the enormous change of position which the Alpine eocene rocks have undergone. The question is one of the highest importance, because the French academician contends, that all the granites erupted in the earlier periods of the earth's history differed from those of later date, in being much more quartziferous; and he controverts the doctrine proposed by me in my 'Elements of Geology,' that the difference of mineral composition in the oldest rocks of this class now visible may reasonably and naturally be explained by imagining them to have originated at a great depth below the surface. On the contrary, M. de Beaumont supposes that granitic rocks charged with an excess of siliceous acid were formed at the surface in the older times, and he has even had the courage to present us with a diagram of Chaos, entitled "Chaos primitif," representing a scene by no means rude and disorderly, but where we behold two pyramidal mountains, from one of which the ordinary volcanic lavas and more volatile substances, such as sulphur, chlorine and aqueous vapour, are evolved; while from the summit of the other, granitic compounds, tin, fluor, and the more refractory and less volatile materials, are discharged*. It is suggested that the greater part of the metals which usually accompany tin were concentrated in the first envelope of the globe, but after the palæozoic epoch they were withdrawn from circulation, and like the primitive granites ceased to be emitted from the interior. The gases and vapours, from which the more ancient metalliferous compounds were sublimed, would, it is said, have been most deleterious to organic beings living in the air and ocean, so that their evolution in the sea and atmosphere in later times was discontinued.

For my own part, after having given the most patient consideration to these views, I see no sufficient grounds for believing that the same granitiform mixtures and metalliferous emanations may not have been disengaged in equal quantity at every successive geological period down to the most modern. We are taught by the activity of several hundred volcanos, that there must now be lakes and seas of melted matter in the interior of the earth, in every state, from one of perfect fusion to one of incipient crystallization; and as solid rock must thus frequently originate in great masses, under conditions different from that of lava poured out into the atmosphere, why should

* Bulletin, 2nd Series, vol. iv. p. 1322.

we not adopt as the most probable conjecture the idea that this matter is now, as of old, passing into granite, or into some of the granitiform compounds, more especially when we know that silex abounds in many modern lavas, and that certain obsidians and pumice do not differ materially in their component elements from granite.

I fully assent to the doctrine so ably advocated by M. É. de Beaumont, that a large class of metalliferous veins may simply be regarded as extinct mineral springs. They are fissures in which vapours, or thermal waters charged with various elementary bodies, have precipitated the materials of a refractory kind, or those which are the least easily retained in solution. The marked agreement between the contents of mineral springs and the emanations from active volcanos strongly supports this view. But why should we doubt that fissures now existing in solid rocks may in like manner communicate at one extremity with subterranean masses of fused matter, while at their upper end they terminate in mineral springs? and if so, why may not hot steam and gases and mineral waters be depositing at this moment, as actively as ever, that class of elementary bodies, whether metalliferous or not, which we find in the oldest veins? The steam or hot water will always part with these substances in the deeper parts of every fissure, and merely bring up to the surface the residuary salts which are more soluble and volatile. Hence mineral veins are marked by the habitual absence of alkalies, which are so readily dissolved in water.

When we consider the grand and reiterated movements of elevation and depression which have agitated the earth's crust since the palæozoic epoch, and the vast amount of volcanic action which can be shown to have been of subsequent date, it is evident that all those refractory bodies, said to have been "withdrawn from circulation," must have been from time to time re-melted, and therefore re-issued from the grand subterranean mint. Their circulation may always be confined to the interior of the earth, and they may never, except in very minute quantities, be disengaged superficially. If it be so, they must always be ancient in all future systems of geological classification; not because they originated at remote æras, but because time is required to uplift and expose them to view.

No illusion indeed is more likely to mislead us in our chronological speculations than the temptation to ascribe to antiquity appearances which are in reality characteristic of a deep subterranean or submarine origin. Volcanic rocks now forming at a certain distance below the surface, or sedimentary strata which are in progress in deep seas, can very rarely emerge and become visible to man till they have acquired a high antiquity relatively to most of the lavas and beds of mud, sand and pebbles which will be formed in the interval of time between the origin of such subterranean or submarine rocks and their exposure above ground. They cannot, except in a few very disturbed regions, like the Alps, emerge from the sea, or break out in the centre of a mountain-chain, till a series of grand revolutions of the earth's crust has occurred throughout many large areas. Lofty cones of lava and scoriæ will have been piled up, old rocks will have been denuded or displaced, bent or fractured, and new strata, thousands of feet thick, will have been formed, besides the occurrence of several important fluctuations in the organic world, before the nether-formed products of fire or water are brought into view. Whenever these do appear, their aspect will be strange and unfamiliar to human observers, such as might well belong to bodies formed in a part of the great laboratory of nature, to which man has no access. Such singularity in outward form and internal texture will naturally be referred to an origin connected with the beginning of things, if the mind be already prepossessed with a belief that we are studying the monuments of a planet, which has been passing from a chaotic or nascent state to one of order and maturity, especially if the peculiar rocks in question are found invariably to have claims to a high relative antiquity.

"Granitic eruptions," says M. de Beaumont, "have become more rare in the more recent epochs*;" and doubtless it is most true, that in the newer secondary and older tertiary formations, the granitic rocks become more and more exceptional; but had we lived in the carboniferous or Permian epochs, we might, I conceive, with equal justice have declared the only granites then visible to be extremely ancient. The more quartziferous varieties, together with a certain class of metalliferous veins, posterior in date to the vegetation of the coal period, such as are now known to the miners of Cornwall, or to those of the Ural Mountains, would then have been unformed, or at least invisible. The ages which have elapsed since the coal-measures

* Bulletin, 2nd Series, vol. iv. p. 1299.

were accumulated are so countless, as to have afforded ample time for the upheaval of much crystalline rock and metallic ores from great depths, and for the clearing away of superficial matter by aqueous denudation. To what an extent this subsequent denudation has been carried may be shown by adverting to the fact, that the masses removed must have more than equalled in volume all the sedimentary strata newer than the coal, for some part of the materials of such strata have been more than once ground down into sand or mud since that period and re-stratified.

Before concluding I shall say a few words on another very different topic, yet one which has a distinct bearing on the theoretical question discussed in this Address. Until the transporting power of glaciers and icebergs was better understood, no geological phænomena were oftener appealed to in support of violent earthquake-waves, sudden deluges, rapid and overwhelming currents of mud, and other extraordinary agencies, than the northern and Alpine erratics scattered over hill and dale, and having no obvious relation in their geographical distribution to the present drainage or physical outline of the countries where they abound. The hypothesis which has recently gained more and more favour, as best explaining the dispersion of such blocks, dispenses with all sudden and paroxysmal exertion of force; nay, more, it does not even call into play a succession of waves such as ordinary earthquakes can produce. The rate at which huge blocks of stone travel for centuries on the surface of a glacier, never halting day or night, summer or winter, appears rarely to exceed, according to the exact measurements of Professor James Forbes, half an inch per hour. When the icy mass, with its moraine and included boulders, reaches the sea, and becoming detached on the coast, gives birth to an iceberg, the frozen raft traverses wide spaces of the ocean at the rate of a few miles a day, so that its advance is usually inappreciable by human sight. I have seen hundreds of these floating bergs at once in the Atlantic on their way southwards; but no observer could determine their direction, or decide whether they were aground or in motion, unless he had opportunities of comparing their relative position from day to day. So large is the volume of ice submerged beneath the water, that the waves and swell of the Atlantic during a storm have no more power to communicate a rocking motion to one of them than if they were islands, or parts of the firm land.

Should geologists ever be convinced that some of the most gigantic curvatures of Alpine strata have been the result of intense pressure, so moderated in its application as to have been just sufficient to overcome the resistance opposed to it,—should any of them ever declare their belief that the motion had been as insensible as the unfolding of the petals of a flower,—it would not imply a more remarkable revolution in popular opinion than we have witnessed in reference to the glacial hypothesis. Nor even then might we be entitled to pronounce the process a slow one relatively to other natural operations, organic and inorganic, which were simultaneously in progress. In the fourth volume of our Quarterly Journal (p. 70), Mr. Hopkins, to whom you have this year awarded the Wollaston Medal, has published an excellent paper on the elevation and denudation of the Lake district of Cumberland and Westmoreland. He has undertaken, and, as it appears to me, with no small success, the very difficult task of restoring, in a series of diagrams, the successive steps by which the physical geography of the country attained its present condition, although the changes to be accounted for, consisting of the addition of several new sedimentary formations, and repeated alterations of level, and denudation of rocks, were numerous and complicated. In one part of his memoir he has suggested the possibility of the period during which the dispersion of erratic blocks took place, having extended far back in geological time, even as far as the oolitic period; an opinion which is, I think, at variance with a great weight of evidence derived from the study of the boulder formation both in Europe and North America. But in regard to the mode of transport, Mr. Hopkins has taught us, that if the bed of the sea were suddenly uplifted from 100 to 200 feet in vertical height, such an instantaneous upward movement would give rise to currents having a velocity of twenty-five to thirty miles an hour, and these currents might move blocks of great magnitude from place to place. Thus a current of ten miles an hour would be capable of propelling a block of five tons weight, and its force increasing in the ratio of the square of its velocity, a current of twenty miles an hour would move a block of 320 tons. The experiments of Mr. Scott Russell on the velocity of waves of translation, although made with much smaller waves, are supposed to bear out these views*.

* Hopkins, Quart. Journ. Geol. Soc. of Lond. vol. iv. p. 70, No. 13.

Now, adopting all the mathematical and hydrostatical calculations of Mr. Hopkins as correct, they prove, I think, the non-occurrence or extreme rarity in past times of earthquake-shocks more violent than such as we have experienced in the last ten centuries. For when we consider how many marine formations have been upraised, some of them from seas of considerable depth, and what a vast amount of upheaval and subsidence, estimated, as I have already reminded you, by miles vertically, has taken place, it seems clear that if currents and waves of such power as those contemplated by Mr. Hopkins had really been set in motion, there would have been erratic blocks in deposits of all ages, instead of their being confined to the close of the tertiary period. Had these mighty waves swept again and again over the floor of the ocean, and over the land in ancient periods, a drift or boulder clay with rounded and angular blocks would have been conspicuous in the Eocene, Cretaceous, Jurassic, Triassic, Permian, Carboniferous, Devonian and Silurian formations, and would have been most strikingly displayed in such of these epochs as have been of the longest duration. I have seen fragments of gneiss eight feet in diameter in the base of the Silurian series in Canada, in the group called by the New York geologists the Potsdam sandstone*; but I observed in the same place similar gneiss *in situ*, in the immediate vicinity, so that the blocks may have been detached from an undermined cliff of the Silurian sea-coast. In like manner, in the valley of the Bormida, in Piedmont, there are huge rounded masses of serpentine in the tertiary molasse; but similar rocks *in situ* pre-existed in the same region, so that blocks may have been derived from the destruction of cliffs close at hand. In Scotland, also, we see occasional fragments of large dimensions in the conglomerates of the old red sandstone, especially on the western coast, but in that case there is no ground for presuming distant transport. In no part of the geological series, except in that of very modern date, do we find an extensive deposit of drift, like that spread over Northern Europe and North America.

It may doubtless be objected, that by adopting the glacial hypothesis we concede the possibility of one natural agent, such as frost, acquiring at certain periods an intensity of action far greater than at others, and hence I may be asked, whether the energy of any other cause may not in an equal degree be subject to secular variation? I admit

* See Travels in North America, vol. ii. p. 126.

the force of the argument, if not pushed beyond its legitimate bounds. No one can contemplate future changes in physical geography without foreseeing that the varying altitude and extent of polar and equatorial lands may give rise to an intensity of solar heat or glacial cold, such as is not experienced now, and may never have been experienced on the earth; for the combinations of circumstances on which the climate of the globe most depend are so varied, that no one can define or guess how far heat, cold, moisture, and other conditions, may deviate from a mean state of things in the course of ages. But speculations of this kind belong equally to the future, the past and the present, and imply no inconstancy in the general condition of our planet, such as is assumed in the hypothesis of its passage from a chaotic to a fixed, stable and perfect state. Living as we do in an æra which has immediately followed the glacial epoch, we are able to comprehend the state of the northern hemisphere in European latitudes, when cold like that of the arctic and antarctic circles extended further from the poles towards the equator. We may also reason philosophically on the state of the globe during the carboniferous epoch, when there may have been little or no ice even at the poles. We may conclude that in those days a warmer, damper, and more uniform climate prevailed, when the Sigillaria, Lepidodendron, Caulopteris, Calamite, and other fossil plants flourished, and when there were reefs of coral in the adjoining seas. Such organic remains may betoken, as our Foreign Secretary, Mr. Bunbury, has argued, rather the absence of frost than, as many botanists once thought, an intense tropical heat. M. Adolphe Brongniart, in his admirable Essay on the genera of Fossil Plants, published in the year 1849 *, has questioned, and apparently with reason, the proofs hitherto adduced in favour of the existence of any true palms in the coal-measures, and Mr. Bunbury considers their absence as affording an additional argument to that derived from the universal preponderance of ferns in favour of a mild temperature in the atmosphere,—a warm, moist and uniform climate, not a tropical one. The flora, he says, of the London clay was of a much more tropical character. In this manner we may now reason philosophically on the remote carboniferous æra according to strict rules of induction; but had we lived in that

* Tableau des genres, etc. Dictionnaire Universelle d'Histoire Nat., Art. Végétaux Fossiles.

æra, and had been called upon to decipher the monuments of a glacial period of high relative antiquity,—had the phænomena of the drift constituted the first or oldest chapter then extant of the earth's autobiography, instead of happening to be, as it now is, the last and newest, we should have been in danger of indulging for ever in the most visionary and extravagant hypotheses. Ignorant of glaciers and icebergs, and perhaps of ice and snow,—unable to comprehend the nature of that mysterious power which had polished the surface of rocks over wide areas, or had engraved upon them long rectilinear and parallel furrows, we should have gazed upon these markings, and upon the confused and unstratified heaps of clay and loam, interspersed with boulders, and usually devoid of fossils, in stupid amazement and with feelings of despair. The enormous bulk of some erratics, which had travelled for hundreds of miles from their original sites, would have confounded us, and might well have tempted a geologist to dream of frightful catastrophes, and diluvial waves of prodigious velocity, which swept over the planet in its infancy, before it was fitted for the reception of the higher animals and plants, much less to become the home of man. If any one then doubted that there had been an æra of paroxysmal violence, or of primæval chaos, and wished to refer all geological appearances exclusively to the agency of slow and ordinary causes, he would have been asked to explain the position of fragments of granite, like those of Scandinavian origin, on the plains of Pomerania, or of protogene from Mont Blanc lodged on the summit of the Jura, and such an appeal in refutation of a theory apparently so visionary must have been triumphant.

But it is now time to conclude; and in taking leave of you, Gentlemen, I will venture to indulge the hope, that on some future occasion I may resume this theoretical discussion, which ought to embrace every department of geological inquiry, including that of palæontology, to which as yet I have been able to make but a few passing allusions.