Address delivered at the Anniversary Meeting of the Geological Society, on the 21st of February 1834, by GEORGE BELLAS GREENOUGH, *Esq. President.*

GENTLEMEN,

You have learned from the Report of the Council that the Society has considerably gained in number since the last Annual Meeting. So large an accession of members shows the growing popularity of our science, and is at once a gratifying reward of your past exertions and a sure presage of your further success.

You have also been informed that during the same period the losses of the Society have been unusually numerous. Several of the deceased, whose main objects in life, if not alien, were connected but remotely with those of our institution, conferred upon it, notwithstanding, by their enlightened encouragement, important advantage: but the merits of the poet, the historian, the statesman, the warrior, though recorded in the annals of a grateful country, must not here be dwelt upon. To the memory of those only who have been closely allied to us, as fellow-labourers, will you desire that I should pay, individually, the well-earned tribute of our common regret.

The late Dr. Babington, whom we have been accustomed to look to with a respect almost filial, attached himself in early life to the study of chemistry and mineralogy. In the year 1795, he published a Systematic Arrangement of his collection of minerals purchased of the Earl of Bute, the finest, perhaps, which at that period existed in England; and in 1799, his New System of Mineralogy, which may be considered a continuation of the former work. These works, now superseded by others, which the introduction of improved modes of inquiry and the application of new instruments have rendered more perfect, evince much patient research and an exact knowledge of the state of mineralogy at that time. Active in the cultivation of science himself, Dr. Babington was quick to discern and eager to encourage merit in others. With a view to enable Count Bournon, of whom he had been a pupil, to publish his elaborate monograph on carbonate of lime, Dr. Babington, in 1807, invited to his house a number of gentlemen the most distinguished for their zeal in the prosecution of mineralogical knowledge. A subscription was opened and the necessary sum readily collected. This object having been accomplished, other meetings of the same gentlemen took place for the joint purpose of friendly intercourse and mutual instruction. From such small beginnings sprang the Geological Society; and among the names of those by whose care and watchfulness it was supported during the early and most perilous crisis of its history, that of Dr. Babington must always stand conspicuous.

But while Dr. Babington employed his leisure in the study of chemistry and mineralogy, he gradually rose into eminence as a physician, and at last became occupied with the care of a numerous family, and subjected to all the labour and responsibility of extensive medical practice. During many years, he was disabled from

pursuing his favourite sciences with that unremitted attention which alone leads to original discovery; and accordingly our Transactions do not contain any communication from his pen: no man, however, more steadily cheered us in our progress or more heartily rejoiced in our success. In the year 1822, he was elected to the presidency of this Society, an office which he accepted in deference to the earnest wish of the Members, and held for two years at great personal sacrifice. His conduct in this chair afforded to us ample opportunity of observing the native goodness and kindliness of his heart, the urbanity of his manners, the evenness and cheerfulness of his temper, and the aptitude with which he exercised every liberal feeling.

During the presidency of Dr. Babington, and at his suggestion, was established the practice of submitting to immediate discussion the papers read at the table of the Society. Apprehensions were entertained by some persons at that time, that the collision of argument and the desire of personal distinction might interfere with the love of science or break the bonds of social intercourse,—that we might learn to contend less for truth than for victory. I appeal to you, Gentlemen, whether the brighter anticipations of Dr. Babington have not been amply justified by experience; whether our discussions, continued now during twelve years, have not been strongly characterized by a love of truth; whether the bonds of friendship have not been more closely cemented by them. Our conversations have been animated, but never intemperate; they have encouraged the timid, assisted the investigator in discovering the object of his research, and given additional value to every paper in our Transactions.

Dr. Babington was a Vice-President during the years 1810, 1811, 1812, 1813 and 1814, and a Trustee from 1811 to 1821. His donations to our library and museum were extensive, and from subscriptions set on foot to promote the objects of the Society his name was never withheld.

Dr. Babington retained to the latest period of his life a keen relish for the attainment of knowledge, and made considerable sacrifices to enable himself to keep up with its rapid progress. After descending from this chair he took private lessons in geology of Mr. Webster. So late as the winter of 1832–3 he enrolled his name at the University of London as a student of chemistry, and there attended with the utmost punctuality a course on that science of seven months' duration; he afterwards in the same spirit, and in his 77th year, once more applied himself seriously to geology, and went over the collection of fossils in our museum. I can scarcely imagine a more gratifying spectacle than that of a veteran in the labours of professional duty, thus returning to the pursuits which he had loved when young, and seeking relaxation, not in ease and repose, the allowable luxuries of old age, but in the indulgence of an enlightened passion for knowledge.

I need not apologize for these extended comments; they are more than justified by the occasion. The duties which your benefactor owed to the Society he cheerfully and fully performed. May

his memory kindle in us a feeling not merely of gratitude but of emulation!

Dr. Berger, who died in the early part of last year, was a native of Switzerland, and had been employed in geological study for some years previous to 1813, when he sought in England an asylum from the foreign oppression which in those days of revolution had visited his country. In 1816, at the request of some of his friends in this Society, he agreed to devote himself for three years to geological investigations in the British Islands; and an annual sum was insured to him during that period by a subscription of some of our members. The north-west coast of Ireland was suggested for his first examination, and there, as might perhaps have been foreseen, the movements of a foreigner, who spoke our language imperfectly, and whose occupation must have appeared to the inhabitants mysterious, if not dangerous, at first excited doubt and obstruction, which, though not unamusing, were attended with some embarrassment, and called for the interference of his friends. He laboured with great zeal and assiduity, in that interesting field of inquiry, till his health unfortunately gave way. His papers and collections were therefore incomplete; and his attention appears to have been given perhaps too much to the investigation of details not immediately connected with the proper and immediate business of the geologist. His merit, however, must be judged of, not by reference to the present state of knowledge and the methods of inquiry now pursued, but to the condition of the science at that time. The facts he accumulated were valuable. "A Memoir on the Dykes of the north-east coast of Ireland," by himself, appears in the third volume of our Transactions; his remaining papers were put into the hands of the Rev. William Conybeare, who subsequently went over the same country with Dr. Buckland; and we are indebted to the labours of Dr. Berger, extended and illustrated by these geologists, for one of the most valuable memoirs in the earlier volumes of our Transactions. The late years of Dr. Berger's life were passed in his native country, in bad health: he died at Geneva in 1833.

In perusing at the distance of so many years the record of the arrangement by which Dr. Berger's services were obtained for this Society, and the names subjoined*, I have been much struck by the delicacy with which his personal feelings were consulted, and have looked back with pride and exultation to the early history of our institution. I cannot be surprised at the success which has attended your exertions, when I call to mind the noble and disinterested spirit by which the first steps in your progress were directed. On no occasion since I have known the Geological Society, (and I have known it from its birth until the present hour,) have the Members hesitated to contribute, with the most liberal devotion, both personal labour and pecuniary support, whenever the *probable* advancement

* The paper bears, with the names of other Members who still remain, the signatures of the late Dr. Babington, Dr. Marcet, Mr. Francis Horner, Mr. Morgan, Dr. Wollaston, Sir Joseph Banks and Mr. Ricardo.

of science appeared to call for them. I mention this with double satisfaction, because I am convinced that this good spirit still subsists amongst us with undiminished vigour.

Dr. Alexander Turnbull Christie imbibed in the class room of Professor Jameson a taste for geology, which he afterwards improved in India, as far as opportunity allowed, under many discouraging circumstances. On his return to Europe he applied himself to the science with great earnestness; he studied the best works, courted the society of their authors, familiarized himself with the contents of collections, and practised in the open air the most approved methods of investigation. He became the pupil of M. Brongniart at Paris, and the companion of M. de Beaumont and M. von Buch in the Alps. His studies were by no means confined to geology; they embraced every department of natural history. The climatological and geographical distribution of plants was a subject to which he paid much attention. Having provided at his own expense the best instruments for the purpose, he returned to India with the design of instituting there a continued series of barometric, hygrometric, and other experiments, as well as of exploring the physical structure of that vast region, and of determining the relations of its rocks to those of Europe. On his way he visited Sicily, and transmitted to the Society an account of some of the younger deposits of that island, and the phænomena that accompanied their elevation. He wrote also a description of some bone caves near Palermo, and of tidal and other zones observed on limestone along the shores of Greece. These notices will be found in Jameson's Journal. Dr. Christie died prematurely of a Jungle Fever, while crossing the Nilgherry hill in November 1832.

Mr. Lansdown Guilding, though not himself engaged in the pursuit of geology, added several valuable specimens to our collection, and materially assisted the progress of some other branches of natural history, especially in connexion with the West Indies.

Sir Charles Giesecke was born at Augsburg in 1761. He was originally intended for the church; after various changes of occupation and a life of some adventure, he devoted himself in about his fortieth year to mineralogy, and studied under Werner at Freyberg in 1801. He subsequently travelled with mineralogical views in several parts of the North of Europe; in 1806 he entered into the service of Denmark and repaired to Greenland, leaving at Copenhagen a valuable collection of books and minerals, which were destroyed during the bombardment of that city. In Greenland he formed acquisitions of great interest in various departments of natural history, but foreseeing the probability of their capture on the passage to Europe, he with great resolution and perseverance went a second time over the ground he had examined, and remained in that desolate region till his object was accomplished. In the mean time the vessel which contained his first treasures was

taken, and the cargo sold by auction at Leith. The minerals attracted but little general notice, in part, I have been informed, from their being packed in moss and sea-weed, and perhaps also from the very circumstance of many of the species being unknown. Mr. Allan purchased nearly the whole collection, which upon examination proved to contain a great number of new and rare substances of the highest mineralogical interest, cryolite, sodalite, allanite, with mixed groups of striking variety and novelty; and all in such abundance that most of the cabinets of England (when collectors, if not more numerous, were at least more active than I fear they are at present,) were supplied from this source. Mr. Giesecke himself accidentally arrived at Leith in 1813, not long after Mr. Allan had published an account of his purchases, and with great generosity contributed to the improved catalogues and descriptions of specimens which subsequently appeared. He was soon after appointed Professor of Mineralogy to the Royal Dublin Society, and went to reside in Ireland, where he spent the remainder of his life. About this period also he was honoured with an Order of Knighthood by the King of Denmark; but having now passed his fiftieth year, his health was broken, and much of the energy lost which distinguished his early life. He lived to the age of 72, and died at Dublin in March 1833. Sir Charles Giesecke meditated, after his return from Greenland, an extensive work upon that country; he published a brief account of it in Dr. Brewster's Encyclopædia, but the larger work was deferred till the voyages of Ross and Parry had deprived the subject of the interest of novelty. His meteorological observations appeared in the Edinburgh Philosophical Journal for 1818; and he gave to Mr. Scoresby, for his work on the Greenland Coast, the use of his maps and other materials. The Edinburgh Philosophical Journal for 1822, contains an account of his discovery of the geological situation of Cryolite. His only publications on the mineralogy of Ireland are, I believe, a brief notice of the geological situation of Beryl in the county of Down *, and an account of an excursion to the counties of Galway and Mayo†.

Mr. Alexander Nimmo was a civil engineer of high reputation. He was born in Fifeshire in 1783, and at a very early age showed a strong propensity to physical and mathematical inquiry. One of his first public employments was a survey of some of the bogs in Ireland, on which he delivered a report to the Commissioners in 1811, containing some general observations on the geological character of part of Roscommon, Kerry, Cork and Galway. He was afterwards engaged in various works of great importance, principally in Ireland. He was the author of several articles in Brew-

* Annals of Philosophy, 1825. New Series, vol. x. pp. 74 & 75; republished from the Dublin Philosophical Journal.

† Annals of Philosophy, 1826; republished from the Dublin Philosophical Journal.

ster's Edinburgh Encyclopædia, on subjects connected with his profession. One of his latest and most valuable literary productions, on the publication of part of which he was engaged at the period of his death in January 1832, was a Chart of the Irish Channel, with sailing directions for the coast of Ireland, a performance probably connected with a paper which he laid before the Royal Irish Academy "On Geology as applicable to the Purposes of Navigation."

Mr. David Scott was one of the numerous class of officers in the service of the East India Company who have found means to combine with the most exemplary discharge of their official duties, a constant attention to the interests of literature and science. He was the second son of Archibald Scott, Esq., of Montrose, and died prematurely in India in 1831, at the age of 45, having passed through many offices of high trust with distinguished credit, and holding at the time of his death the situations of Civil Commissioner in Bungpoor and other districts, and agent to the Governor General in the North-east of Bengal. His exertions and success in discharging his official functions, and in promoting the welfare of the country in which he was placed, by diffusing education, were highly appreciated, and a monument has been erected to his memory by the Supreme Government of India. Mr. Scott possessed great knowledge in several branches of science not immediately connected with this institution, and lost no opportunity of attending to geological research. Our Transactions are indebted to him for the substance of a valuable paper communicated by Mr. Colebrooke*, "On the Geology of the North-eastern Border of Bengal," in which is described a remarkable deposit on the left bank of the Burrampooter river, containing an assemblage of fossils that bear an extraordinary likeness to those of the London clay. "Among the remains of fishes," Mr. Colebrooke states, "bony palates and the fins of the Balistes are common to the Indian clay and to that of Sheppey; and the shells of Cooch-behar bear a strong generic, if not specific, resemblance to the marine formations above the chalk in France and England." This communication contains also some valuable facts respecting a succession of strata, like those of our coal-fields, in the Tista and Subuk rivers; and in the same volume, is an extract from a letter written by Mr. Scott, describing the situation of a limestone and clay containing Nummulites at Robagiri, a village in the North-east of Bengal. Such resemblances, though they are far from establishing the contemporaneous formation—much less the continuity—of the groups in which they occur, are interesting, from the proof they furnish, of the operation of similar causes in very distant parts of the former surface of the globe.

On the accounts of the past year put into your hands today, I will make but one observation. From the report of the Auditors it ap-

* Series II. vol. i. pp. 132—140.

pears that the balance of disposable property in favour of the Society, taken at a very moderate estimate, is £2010, while the total amount of the compositions of all the compounders in the List of Fellows since the foundation of the Society does not exceed £2394. The difference is less than £400. If, then, the value of the collections, library, and furniture belonging to the Society be taken into account, our actual property considerably exceeds the claims of all our compounders, our current income being wholly disposable and free.

Wollaston Medal.—The product of the Wollaston Fund during the past year has been awarded to Mr. Agassiz of Neufchatel, in promotion of his work on the "General History of Fossil Fishes."

The first part of Mr. Agassiz's publication has but recently reached England, and the Council have availed themselves of the earliest opportunity of giving support to an undertaking of great geological importance. The author's qualifications for this work were so highly appreciated by the late Baron Cuvier, who had himself been engaged in a similar project, that on seeing Mr. Agassiz's collection of drawings, and hearing a statement of his views, and the results at which he had arrived, that profound naturalist at once transferred to Mr. Agassiz the whole of his materials. The approval of Cuvier is fully sanctioned by the portion of the work which is now before the Society. In deciding on the present award, the Council have acted strictly in compliance with the bequest of Dr. Wollaston. The work of Mr. Agassiz is intimately connected with the objects of this Society; it demands for its completion great labour and expense. It is still in progress, and its publication has been ably commenced with a full assurance of the author's competency to the fulfilment of the task he has begun.

In his prospectus, Mr. Agassiz solicits the contribution of specimens from all quarters; and I cannot better close the announcement of a testimony of approbation which I trust will be gratifying to his feelings, than by requesting the Fellows of the Geological Society to aid the progress of this important work, by giving or lending to its author any drawings and specimens of fossil fishes which they may either possess or obtain. The transmission and return of these loans can be easily effected through the medium of the officers of this house.

The History of Geology has been recently treated by several authors, especially by Mr. Conybeare and Mr. De la Beche, in a manner which would render any observation from me on that subject at once superfluous and imprudent. The communications read at our general meetings have been fixed in your memory by the discussions to which they have given rise, and the published abstract of their contents. Still, however, it may be well to enumerate these communications, that you may measure the exertions made

here since the last Anniversary, and the effect they have had on the state of geological knowledge.

Miscellaneous.

The experiments of Sir James Hall mark an important epoch in science. It was with great delight, therefore, that we received from Captain Basil Hall, R.N., a collection of the products of these experiments, and some of the instruments with which they were conducted. Among the latter is a machine for regulating high temperatures, accompanied by an account of its properties and mode of acting.

Mr. Gardner, the well known geographer, has drawn our attention to the curious fact, that of the land on the surface of the globe only $\frac{1}{77}$th part has land at its antipodes.

Sir David Brewster has communicated to us his interesting observations on the properties of the diamond, from which it would appear to be of vegetable origin,—the cavities whence these properties are derived being found in amber, but not in any product either of igneous fusion or of aqueous solution.

Home Geology.

Dr. Mitchell has laid before the Society a detailed account of the geology of Harwich in Essex, of the Reculvers in Kent, of Quainton and Brill in Buckinghamshire. Mr. Dadd has described the Vale of Medway and its neighbourhood. Dr. Fitton, who published in the early part of the year, a geological sketch of the vicinity of Hastings, has supplied us with an account of some instructive sections recently exposed to view at St. Leonard's. Mr. Woodbine Parish has sent to us portions of the Iguanodon and Lepidosteus from the well known "White Rock," situate in the same district, and now almost destroyed. Our knowledge of the inland Extent of the Wealden Formation has been enlarged by a paper of Mr. Strickland, accompanied by specimens of Paludina from the ferruginous sand of Shotover hill.

Mr. Strickland has also rectified the boundaries of some of the strata near Bewdley, and traced a line of fault from the north of Bredon Hill to Little Inkbarrow.

Sir Philip Egerton has supplied us with further information in respect to the lower portion of the Connaught Coal-district. Beneath the coal at Kulkeagh in the county of Fermanagh is a shale 600 feet thick, with subordinate layers of black marlstone and clay-iron ore towards the top, and a thin stratum of micaceous grit near the bottom. All the beds are replete with ammonites, orthocera, producta, encrini, corals and calamites. This deposit lies on sandstone separated by the mountain limestone from another bed of shale marked by characteristic fossils, and the entire system therefore appears to bear a strong resemblance to the lower portion of the carboniferous beds in the South-west of England.

In the carboniferous strata of Coalbrookdale, Mr. Prestwich has

described a heterogeneous assemblage of plants and shells both of fresh- and salt-water species. A band of ironstone, nearly in the centre of this series, contains four genera of Trilobites: in the same coal-field Mr. Anstice has recognised two genera of insects. On the opposite side of the Severn, Mr. Murchison has found at Pontesbury, Uffington, Le Botwood and other places, a band of compact limestone, between two beds of coal, resembling the lacustrine limestone of central France, and containing freshwater shells. These discoveries may throw light on those which have been since made at Burdie-house and elsewhere in the neighbourhood of Edinburgh.

The structure of other coal-fields has been illustrated by Mr. Murchison, Mr. R. J. Wright, and Mr. England.

After careful examination of the Old red Sandstone, Mr. Murchison has proposed to divide it into three parts: the uppermost, characterized by quartzose Conglomerate; the middle, by Cornstone; the lowermost, by Flagstone. The cornstone and marlstone of the middle group contain undescribed genera of crustacea; and in the tilestone beneath are found some defences of fish, together with a few remains of testacea.

Mr. Murchison has employed three summers in examining a range of country situate between Shrewsbury and Caermarthen; and the geological positions as well as the mineral and zoological characters of the several rocks which border England and Wales are now determined with as much exactness as those of any portion of the secondary system. Taking the old red sandstone as a line of departure, the rocks beneath are disposed in descending order as follows:

1. The Ludlow series, divisible into three parts, the upper, middle and lower. To the middle belong the well-known limestones of Amestry and Sedgley: the upper and lower consist of sand, marl, or flagstone, having some fossils peculiar to each, and others in common. The thickness of the whole is estimated at 1000 feet.

2. The Dudley or Wenlock series, consisting of limestone: its thickness may be taken at 2000 feet.

3. The Hordesley or May Hill series, composed of party-coloured sandstone, conglomerate and impure calcareous flagstone: it is said to attain a thickness of 2500 feet.

4. The Built or Llandilo series, a black flagstone, characterized by the *Asaphus Buchii*.

5. The Longmynd or Linley series, consisting of coarse roof slate, sandstone and conglomerate; no fossils have been discovered in it.

It is well known that Professor Sedgwick has studied with equal assiduity the rocks which lie beneath those I have mentioned. When his observations are published, the Society will have a type of the whole of the transition rocks of Wales. The rocks described by Mr. Murchison are, for the most part, exceedingly well characterized by their fossil contents. Some of the shells which he has discovered, appear to have escaped the notice of antecedent observers; but the genera, if not the species, of others, may occasionally be found in the works of Hisinger and other continental writers. If, then, the transition as well as the secondary and tertiary beds can be identified

over great tracts of country by their fossil remains, let us hope that a clue is now at hand, by which we may find our way through that vast assemblage of beds, which, not in England only, but in Scotland, Ireland, Germany, Russia, Sweden, and North America, has hitherto presented to the observer a mere scene of confusion.

In Mr. Murchison's paper we find also, traced with exactness, several hitherto unexplored lines of disturbance, producing sometimes, as in the Abberley Hills, a complete inversion of dip. The rocks which border the old red sandstone, acquire in some places an anticlinal dip, and reappear in parallel ridges far westward of their natural site, insomuch that the Ludlow series is met with even in Montgomeryshire. Mr. Murchison has examined in detail the trappean and porphyritic rocks to which these disturbances are for the most part assignable, but the description of them has been reserved for communications not yet before us.

Professor Sedgwick has transmitted to us a notice on the granite of Shap in Westmoreland. From recent excavations it appears that veins of this granite penetrate the adjoining strata, from which he infers that it is of posterior date.

Mr. De la Beche, one of our Vice-Presidents, acting under the direction of the Board of Ordnance, has produced a geological map of the county of Devon, which, for extent and minuteness of information and beauty of execution, has a very high claim to regard. Let us rejoice in the complete success which has attended this first attempt of that honourable Board to exalt the character of English topography by rendering it at once more scientific and very much more useful to the country at large.

Organic Remains.—Every succeeding year brings to light new fossil animals which cannot be assigned to existing genera. Dr. Riley, deeply skilled in physiology and comparative anatomy, has given us an account of an animal so extraordinary, that naturalists differ even respecting its class. After careful examination, he considers it a cartilaginous fish, partaking of the character both of the Rays and the Squales. Here then is another instance of a link, now wanting to connect existing genera, having formerly existed.

Towards the close of the last session Mr. Channing Pearce exhibited to the Society a matchless collection of Apiocrinites found at Bradford in Wiltshire. To the description of this fossil as given by the late Mr. Miller, Mr. Pearce adds that the column was occasionally ten inches long. He has found in the great oolite, three species of Apiocrinites, differing in the form of their body, and the thickness of its component plates.

Foreign Geology.

Europe.—The structure of the South of Spain has been illustrated by Colonel Silvertop and Captain Cook. From the joint labours of these gentlemen we learn, that the country between the Sierra Morena and the Mediterranean consists of lofty ranges of

granite, slate, serpentine and limestone, succeeded either by red sandstone or by vast beds of secondary, compact, dolomitic limestone. We also learn from them that the valleys and plains which border the shore of the Mediterranean, are composed of tertiary strata; but we are indebted solely to Col. Silvertop for pointing out to us, on the authority of M. Deshayes, that the tertiary deposit of Malaga and the districts adjacent belongs to the Pliocene, while that of the basins of Baza and Alhama belongs to the Miocene epoch.

Mr. Lyell has laid before us an account of the lignite formation of Cerdagne in the Eastern Pyrenees. This lacustrine deposit reposes in horizontal beds on granite and hornblende and argillaceous schist at the height of 3000 and 4000 feet above the level of the sea. The shells procured are too imperfect to determine its age.

A memoir on the neighbourhood of Bonn was presented last year by Mr. Horner. After describing the characters of the grauwacke, trachyte, basalt, brown coal, gravel and löss, the author compares the age of these with that of analogous formations in other parts of Europe, and of one another. The beds of grauwacke as they contain Terebratulæ and other shells he refers to the upper part of that system; he considers the brown coal more recent than the plastic clay, some of its plants and shells having been identified with specimens found at Aix en Provence. The löss, which reposes on a thick bed of gravel, and contains existing land shells, together with bones of extinct quadrupeds, is considered the latest deposit, and attributed to the bursting of a lake in the upper part of the Rhine. From the beds of trachytic tuff being interstratified with brown coal, and from the occurrence of a bed of basalt above it, Mr. Horner infers that volcanic operations took place during, and even subsequently to, the deposition of the lignite. Having thus established the comparative age of the brown coal, he also determines that of the volcanic rocks.

The tertiary coal or lignite near Gratz, in Styria, is interesting on account of its organic remains. In the memoir of Professor Sedgwick and Mr. Murchison on the Eastern Alps, the strata of this deposit, which are nearly horizontal, are shown to rest on "an inclined system of secondary green-sand." Imbedded in the coal are various vegetable remains, shells of a Cypris, scales of fishes, and fragments of bones of Mammalia and Tortoises. Professor Anker of the Joanneum, has sent to the Society an account of these, together with the drawing of a jaw, which Mr. Clift conceives to have belonged to a Hyæna.

Mr. Pratt, ignorant of the prior researches of Dr. Christie, carefully examined, in the year 1832, the caves of Monte Grifoni near Palermo; and having ascertained the height to which the perforations of lithodomi extend in each, infers that the change of level was not effected by one movement, but by several.

Asia.—Much information has been received from the East during the past year. Mr. Burnes, distinguished for his travels in India, Persia and Toorkistan, has presented to the Society his geological memoranda of the countries lying between the mouth of the Indus

and the Caspian Sea. Mr. Burnes, though he did not travel for the express purpose of studying geology, carefully and faithfully noted whatever attracted his attention. In reading his account of these hitherto almost unknown regions, we cannot but be struck with the resemblance of their geological structure to that of Europe. The central axis of the Hindoo Koosh is composed of granitic rocks, succeeded by various schists, conglomerates, variegated marls, limestones and sandstones. Besides this mighty system, some portion of which cannot be identified with European strata for want of fossils, there is a vast range of salt (previously noticed by Mr. Elphinstone), of coal, and, near the mouth of the Indus, nummulitic limestone.

In a late number of Jameson's Journal is part of a memoir on the structure of the Valley of Ovelipore* by Mr. Hardie, one of our recently elected Fellows.

This valley had previously been noticed by Captain Dangerfield†; but Mr. Hardie has been the first to describe a singular Indian formation which occurs there, called Kunkur. It is rarely, if ever, stratified; it forms a bed, seldom exceeding a few feet thick, which mantles over the irregularities of the country. It is sometimes imperfectly oolitic; at others globular, botryoidal or nodular; in some places a compact limestone; in others it resembles chalk: not unfrequently it contains round and angular fragments of rocks. No animal or vegetable remains have been noticed in it. The author carefully distinguishes Kunkur from modern tufaceous deposits, but assigns to it a similar origin.

AMERICA.—Captain Colquhoun and Mr. Burkart have presented to us a specimen of native iron from Zacatecas, and memoranda on this and similar masses found in Mexico.

Captain Bayfield has communicated to us a paper on the shores of the River and Gulf of St. Lawrence from the Saguenay to Cape Whittle. The information contained in this memoir completes our knowledge of the north coast of the St. Lawrence‡; and from the previous labours of Mr. Green in the district of Montmorency§; of Lieutenant Ingall in the country bordering the rivers St. Maurice and aux Lievres‖; of Captain Bonnycastle in Upper Canada¶; of Dr. Bigsby**, Captain Bayfield†† and Dr. Richardson‡‡, on the shores of Lakes Ontario, Erie, Huron and Superior; and of Dr. Richardson in the overland expeditions to the Arctic Seas, we have a

* The city of Ovelipore is in lat. 24° 25′ N. long. 73° 44′ E.

† See Sir John Malcolm's Central India.

‡ See on the country between the St. Maurice and the Saguenay, Trans. Quebec Society, vol. ii. p. 216. On the Saguenay country and St. Paul's Bay, *ibid.* vol. i. p. 79; vol. ii. p. 76. On Quebec, Proceedings Geol. Soc. No. 5, p. 37.

§ Quebec Trans. vol. i. p. 181. ‖ *Ibid.* vol. ii. p. 7.

¶ *Ibid.* vol. i. p. 62.

** Proceedings Geol. Soc. No. 3, p. 23. Trans. Geol. Soc. Series II. vol. i. p. 175. Journal Royal Institution, vol. xviii. pp. 1, 228.

†† Quebec Trans. vol. i. p. 1. ‡‡ Appendix, Expedition to Polar Seas.

general account of the geological structure of the whole country between the mouths of the Mackenzie and Copper Mine rivers and the Gulf of St. Lawrence. The researches made during the expeditions of Captain Ross, Sir Edward Parry and Sir John Franklin, have also given us a general insight into the nature of the formations which constitute a large portion of the shores of the Western Polar Seas. Why should I repress the feeling of patriotic pride which rises within me on contemplating how vast a range of the western continent has thus, in the brief period of a few years, been brought within the pale of our science almost entirely by the exertions of English officers? Great is the gratitude we owe them; yet have their services not been wholly without reward. The taste for scientific research which sprung up in the minds of these gallant men, spontaneously, as it were, and without the aid of regular systematic culture, has been to many of them a welcome relief from the toil and monotony of professional duty; while to others it afforded pleasurable occupation in the solitude of trackless deserts, under exposure to all the rigour of an arctic climate, in the absence of European indulgences, and even under the terrible apprehension of impending starvation.

The district surveyed by Captain Bayfield is bounded by hills, composed of granite, sienite and trap rocks, which enter so largely into the structure of the two Canadas. Clay, sand and gravel, apparently recent, occupy the coast. The Mingan, the Esquimaux and Anticosti Islands are of limestone, containing fossils like those of Lake Huron. But the most interesting feature in this communication is the evidence it affords of a change in the relative position of land and water. In the Mingan Islands is a series of shingle terraces, agreeing in character with the recent beach, the most distant being 60 feet above the level of the highest tide. The author describes, with great care, the different vegetation of each terrace, the one furthest from the shore being covered with trees, the nearest almost barren; parallel to the shore, in this island, natural columns of limestone have been scooped out by the action of water at different periods; the levels of the water-worn portions agree with those of the terraces, and the depth of the scooped parts, with the rise of the present tidal wave of the St. Lawrence. Captain Bayfield has noticed similar terraces on the adjacent mainland and in the neighbourhood of Quebec, and thinks the phænomena indicate successive elevations of the land rather than successive depressions of the water.

Among the subjects which have for some years past engaged the thoughts of geologists, none perhaps has excited so general and intense an interest as the Theory of Elevation. I shall avail myself, therefore, of the present occasion to lay before you a connected statement of the scattered facts and opinions upon which it rests.

On entering upon this subject, it is necessary to understand distinctly what is meant by Elevation. Definitions have recently been decried, I think unwisely. The formation of definitions, it has been said, and the establishment of unerring distinctions

are among the last, and not the first steps of systematic knowledge. Equally true, and far more salutary is the lesson that science cannot be advanced by equivocation. As in trading concerns fixed weights and measures are necessary guards against fraud, so in philosophical investigation words of definite meaning are indispensable securities against sophistry and self-delusion. Euclid did not end, he began with defining. Mathematical certainty has no other basis than mathematical precision, and the greater part of those absurdities which from time to time attach themselves to all other branches of knowledge derive their subsistence from ambiguity of language and a dearth of definition.

A torrent brings down a quantity of alluvial matter, and the plain on which it rests is said to be *elevated*.

An opening occurs in the earth; ejected ashes, scoriæ and lava accumulate around it; a Monte Nuovo is formed; and the area it occupies is said to be *elevated*.

By the persevering labour of polypi, a coral reef gradually attains the surface of the ocean; and the fabric so constructed is said to be *elevated*.

A porous rock covers a rock that is not porous; the rain filters through the superincumbent bed; springs break out in the subjacent; and at last, for want of support, the porous rock, originally horizontal, acquires an inclined posture, one end being directed upwards, the other downwards; and the whole is said to be *elevated*.

An earthquake takes place at the mouth of a river; the sea is violently affected; a bar is formed at the entrance of a harbour from the washing in of new alluvion, or from some obstruction to the escape of the old; where a ship floated, a barge is aground; and the land is said to be *elevated*.

Such instances of Elevation are common and incontestible; but elevation of this kind is quite different from that which forms the subject of my present inquiry.

By the term *Elevation*, I mean only the removal of any given object from a lower level to a higher level; consequently it is necessary, before I speak of an object as *elevated*, that I should be prepared to show two things: first, the level at which it has stood; secondly, the level at which it stands.

That I might form a right opinion of the theory, the merits of which I am about to investigate, I have endeavoured to determine the site, the number and the magnitude of those multifarious objects to which the attribute of elevation is continually applied. The attempt has proved unsuccessful: they are indefinite in place, in form, and in dimension. That Mountains should be elevated is not surprising, but we are familiarized also with Valleys of elevation*. In ancient times an Island (Delos, for example,) would alternately

* Valleys of this nature are properly called by Mr. Scrope "valleys of "elevation and subsidence," or more concisely, "anticlinal valleys." See Scrope on Volcanoes, p. 213.

emerge from, and plunge beneath, the sea. Extensive Provinces, nay, entire Kingdoms, now perform the same feat. The existence of Craters of Elevation is by some still considered doubtful; but it is an accredited fact that Mountains and Mountain Chains have risen, either *per saltum* or *per gradus*. All the Strata have been raised; and all Unstratified Rocks would doubtless have been raised also, but that some have risen of themselves. The Bed of the Sea has been elevated again and again. Continents too have been raised, though "by an operation distinct from that which raised the Pri-" mary Strata."

The arguments advanced in favour of these doctrines are derived either from observation, or from induction.

It is stated by Von Hoff, that in the year 1771 several tracts of land were upraised in Java, and that a new bank made its appearance opposite the mouth of the river Batavia. The authorities cited for the effect of this and several other earthquakes mentioned in the same place by this author, are Sir Stamford Raffles, John Prior's Voyage in the Indian Seas, and Hist. Gen. des Voy. tom. ii. p. 401. Mr. Lyell has cited the first of these only, but no such fact is noted in either edition of the work of Sir Stamford Raffles. The other authorities adduced by Von Hoff I have been unable to consult; but from the Appendix to the Batavian Transactions (which contains an apparently authentic account of all the recorded earthquakes that have taken place in Java during a century and a half,) it would seem, that in the year 1771, in which the uprising is said to have happened in that island, there was no earthquake at all.

The Earthquake of Chili in 1822 has been so much* insisted on, that it requires detailed consideration. Of this event an account by Mrs. Graham is inserted in our Transactions. I am deeply sensible of the honour that lady conferred on the Society by her obliging compliance with the request which elicited her narrative, and it is only the importance of its contents which could induce me to subject them to the test of rigid examination.

According to this account "it appeared on the morning after the "earthquake, that the whole line of coast from north to south, to the "distance of above 100 miles, had been *raised* above its former level." But by what standard was the former level ascertained? who on the morrow of so fearful a catastrophe could command sufficient leisure and calmness to determine and compute a series of changes, which extended 100 miles in length, and embraced (according to a statement in the Journal of Science,) an estimated area of 100,000 square miles? How could a range of country so extensive be surveyed while the ground was still rocking, which it continued to do on that day, and for several successive months? What was the average number of observations per square mile? Who made, checked and registered them? By what means did the surveyors acquaint themselves with what had been the levels and contour before the

* Bakewell's Geology, edition 4, pp. 98. 504. Lyell, vol. i. pp. 401. 455. De la Beche's Manual, edition 2. Scrope on Volcanoes, p. 209.

catastrophe took place, by which, as we are told, all the landmarks were removed, and the soundings at sea completely changed?

Mrs. Graham states that by the dislodgement of snow from the mountains, and the consequent swelling of rivers and lakes, much detritus was brought to the coast; and further, that sand and mud were brought up through cracks to the surface. Amid so many agents it should not be easy to assign to each, its share in the general result.

That fishes lay dead on the shore may prove only that there had been a storm. In her published travels, Mrs. Graham represents them as lying on the beach, which may very well have been thrown up, as the Chesil bank has been, by a violent sea. Some muscles, oysters, &c., still adhered, she says, to the rocks on which they grew; but we know not the nature or dimensions of these rocks, whether fixed or drifted. The occurrence of a shelly beach above the actual sea-level is an observation which must not be lost sight of. I propose to speak of it hereafter: in the mean time be it recollected, that these beaches are said to occur along the shore at *various* heights, along the summit of the highest hills, and even among the Andes.

Neither in the paper of Mrs. Graham, nor in the anonymous account published about the same time in the Journal of Science, can I find any paragraph to justify the position (which, from the seductive character of the work* in which it appears, may, if not now assailed, soon be deemed unassailable,) that a district in Chili, one hundred thousand miles in area, "was *uplifted* to the average height "of a foot or more; and the cubic contents of the *Granitic Mass* "added in a few hours to the land." By what means we get the average I do not know. Mrs. Graham says the alteration of level at Valparaiso, was about three feet; at Quintero, about four feet: but *the granitic Mass!* has the geological structure of Chili been sufficiently examined to assure us that Granite extends over one hundred thousand square miles?

In the well-known work of Molina, a Jesuit who passed the greater part of his life in Chili, and wrote a natural history of that country, I find no ground for supposing that in any earthquakes which took place there from the time the Spaniards first landed on its shores to the date of his publication, any similar phenomena had been noticed. Moreover, the statement of Mrs. Graham, and of the writer before alluded to, respecting the *Elevation of land* which occurred during the earthquake of 1822, has not been confirmed by Captain King, nor by any naval officer or naturalist who has since visited that region, though many have visited it who had heard the circumstance, and who would willingly have corroborated it if they could. But they saw no traces of such an event; and the natives with whom they conversed, neither recollected nor could be induced to believe it.

The 16th number of the "*Mercurio Chileno*," a scientific Journal, contains an account of this earthquake, by Don Camilo Enriquez, which I have not been able to procure. A later number refers to this account, and to another published in the *Abeija Argentina*, a work

* Lyell, vol. i. p. 473.

of considerable reputation, which, by the kindness of Mr. Woodbine Parish, I have been enabled to consult. The account there given of the earthquake of 1822, is strongly recommended to the reader, "as a sensible straight-forward description of what actually took "place, without the high colouring in which ignorance and terror "and exaggeration are apt to indulge."

No notice is here taken of the permanent *Elevation of the Land*, and the account concludes thus:

"The earth certainly cracked in places that were sandy or "marshy; I saw cracks too in some of the hills, but mostly in "the low nook where much earth had run together; the sea was "not much altered,—it retired a little, but came back to its old "place. Don Onofri Bunster, who, on the night of the earthquake, "was walking on the shore at Valparaiso, in front of his house, had "a mind to go up on the hill, but could not, so great was the quan- "tity of falling dust and stones: he repaired to his boat therefore, "and with some difficulty got aboard; this done, he made obser- "vations on the motion of the sea; on sounding, the depth was "thirteen fathoms; he heaved the lead a second time, and the "depth was no more than eight fathoms: this alternate ebbing and "flowing lasted the whole night, *but did not the slightest harm on "shore.*"

These are the only cases I remember to have met with, in which the testimony of eye-witnesses has been adduced to prove the Rise of land by Earthquakes. That such Rise may have taken place, at different times, without being recorded, perhaps even without being observed, is not very improbable; but if I am to pronounce a verdict according to the evidence, I believe there is not as yet one well authenticated instance in any part of the world, of a non-volcanic Rock having been seen to rise above its natural level in consequence of an Earthquake.

Before I quit this subject, it may not be amiss to mention, that on comparing the times at which the successive shocks took place in Chili, as given by Mrs. Graham, and the other authorities to which I have had occasion to refer, the discrepancy is extraordinary.

I have already intimated in a few words, my opinion as to the sense in which land can be said *to be elevated by means of Volcanoes*. Of these, Vesuvius is perhaps the most constantly observed; and among the innumerable authors who have described its effects, from the time of Pliny down to the present day, not one pretends that the Apennine limestone, close at hand, has been in the least raised by that volcano. We shall do well to bear this in mind, when we have occasion to consider the height at which tertiary shells are found on Etna. That those shells belong to beds thrown up by Etna, is a doctrine founded upon induction, not upon experience. As far as experience goes, we have no reason to think that Etna, in its most violent paroxysms, will ever raise those tertiary strata above their present level.

Leaving these scenes of paroxysmal violence, let us next inquire, whether there may not be going on, in the calmest seasons and in

the stillest countries, a *chronic and almost imperceptible impulsion of land upwards.*

As early as the time of Swedenborg, who wrote in 1715, it was observed that the level of the Baltic and German Ocean was on the decline. About the middle of the last century an animated and long-continued discussion took place in Sweden, first as to the cause of this phenomenon, and then as to its reality. Hellant, of Tornea, who had been assured of the fact by his father, an old boatman, and who afterwards witnessed it himself, bequeathed all he had to the Academy of Sciences, on condition that they should proceed with the investigation: the sum was small, but the bequest answered the purpose. Some of the members of the Academy made marks on exposed cliffs and in sheltered bays, recording the day on which the marks were made, and their then height above the water. The Baltic affords great facility to those who conduct such experiments, as there is no tide, nor any other circumstance to affect its level, except unequal pressure of the atmosphere on its surface and on that of the ocean: this produces a variation which is curiously exemplified at Lake Malar near Stockholm. As the barometer rises or falls, the Baltic will flow into the lake, or the lake into the Baltic. The variation resulting from the inequality of atmospheric pressure, however, is trifling. In sheltered spots, mosses and lichens grow down to the water's edge, and thus form a natural register of its level. Upon this line of vegetation marks were fixed, which now stand in many places two feet above the surface of the water.

In the year 1820-1, Bruncrona visited the old marks, measured the height of each above the line of vegetation, fixed new marks, and made a Report to the Academy. With this Report has been published an Appendix by Halestrom, containing an Account of Measurements made by himself and others along the coast of Bothnia. From these documents it would appear, 1. That along the whole Coast of the Baltic the water is lower in respect to the land than it used to be. 2. That the amount of variation is not uniform. Hence it follows, that either the Sea and Land have both undergone a *change of level*, or the Land only; a change of level in the Sea onl will not explain the phenomena.

A quarter of a century has now elapsed since Mr. von Buch d clared his conviction that the surface of Sweden was slowly rising the way from Frederickshall to Abo, and added that the Rise mig probably extend into Russia. Of the truth of that doctrine the presumption is so strong, as to demand, that similar experiments and observations should be instituted and continued for a series of years in other countries, with a view to determine whether any change of level is slowly taking place in those also. The British Association for the Advancement of Science have already obeyed the call. A committee has been appointed to procure satisfactory data to determine this question as far as relates to the coasts of Great Britain and Ireland, and I cannot but hope that similar investigations will also be set on foot along the coasts of France and Italy, and eventually be extended to many of our colonial possessions.

The inductive arguments in favour of the *Elevation of land*, what-

ever the size, and whatever the amount of Rise, are founded chiefly on the following circumstances: 1. The height of sedimentary beds and marine bodies, whether corresponding or not to those of adjacent seas, or of the actual globe. 2. The height of terraces resembling sea beaches. 3. The height of ripple marks. 4. The change of posture which horizontal strata undergo in the neighbourhood of "unstratified rocks." 5. The various heights at which the same rocks occur in different parts of their course. 6. The anticlinical posture of strata frequent in, though not confined to, mountain chains. 7. The arched or domed configuration of some strata. 8. The occurrence of coral, apparently recent, high above the present surface of the sea. 9. The position of ancient buildings, viz. the temple of Serapis at Puzzoli, &c. I have not time to consider these arguments in detail; each deserves to form the subject of a separate treatise. Some of them prove not Elevation, but only change of level, which Subsidence would explain equally well. Some prove local disturbance, whereby one portion may have been thrown up, the other down. Some again afford a fair presumption of real *local* Elevation or Ascent. Most of them are good to a certain point: all are continually overstrained; and I am frequently astonished to observe how prodigious the weight, how slender the string that supports it.

The assigned *Causes of Elevation* are exceedingly various. One author raises the bottom of the sea by earthquakes; another, by subterranean fire; another, by aqueous vapour; another, by the contact of water with the metallic bases of the earth and alkalis. Heim ascribes it to gas; Playfair, to expansive force acting from beneath; Necker de Saussure connects it with magnetism; Wrede, with a slow continuous change in the position of the axis of the earth; Leslie figured to himself a stratum of concentrated atmospheric air under the ocean, to be applied, I suppose, to the same purpose.

It is impossible within the narrow limits of this discourse, that I can enter into the merits of these and other hypotheses seriatim. I must therefore throw them into two classes, the first of explosive forces, the second of sustaining forces; they are one and the same in Plutonic language, but still it will be convenient to separate them.

That explosive forces exist, or may exist, under the surface, no one can deny; but I cannot adopt the opinion (however high the authority from which it comes,) that "in volcanic eruptions we find a power "competent to raise *Continents* out of the ocean." The force we find in volcanic eruptions is limited in time, place and action; it fuses bodies of easy fusibility; it tosses up those that are refractory, and thus forms either a current of lava or a shower of stones, scoriæ and ashes. What resemblance is there between this operation and the rise of a continent? With more propriety might it have been said that in a molehill we behold the action of a cause competent to raise mountains.

If by *Continent* is meant a whole Continent, and nothing but a Continent, its rise, provided this happened only once, would seem difficult to understand; but to me still more incomprehensible is the confident assurance we continually receive from writers of high

and deserved reputation, that this event has happened again and again. Before we admit the Submersion of a continent, we must admit either that at a period immediately preceding that catastrophe, there existed under the land a cavity large enough to contain the continent about to be submerged, or that during the process the subjacent beds shrunk in consequence of a reduction of the temperature, and to such an extent that the contraction in a vertical line equalled the distance from the level of the highest tops of the continent to that of the surrounding ocean. In like manner, before we can admit the Elevation of a continent, we must admit either that, at a period immediately preceding that catastrophe, there happened an inroad of sustaining matter equal in thickness and in extent to the Continent about to be uplifted, or that during the process the subjacent beds expanded in consequence of an increase of temperature, and to such an extent that the expansion in a vertical line equalled the distance from the level of the highest tops of the continent to that of the surrounding ocean. These therefore are the events which we are taught to credit, as having taken place again and again, notwithstanding the tendency which caloric has to diffuse itself, and the apparently unaltered dimensions of the fissures and local caverns by which the strata are so often separated or intersected.

I will not expend more of your time in arguing against such doctrines. All men are more or less lovers of the marvellous, but few, I think, will upon reflection approve such marvels as these.

Solids, fluids and aeriform substances exist, we know, in the interior of the earth, and expand by heat, which exists there likewise. All of these, therefore, are fit *Agents of Elevation*, subject to certain conditions.

Dr. Daubeny attributes the liquefaction of lava, the throwing up of ashes, and all other phenomena of disturbance attendant on volcanic eruptions, to the Action of Water upon the Metallic Bases. This cause is not opposed to experience, and appears well proportioned to the effect, which is sudden, violent, occasional, temporary, accompanied by heat and by flame. To me, at least, it seems far more satisfactory than the explanation of those who ascribe the effect to the Elastic Power of Subterranean Fires, repressed in one place and relieved in another, or to the Undulations of a Heated Nucleus.

A heated *Central Nucleus* is a mere invention of fancy, traceable, I believe, to no other source than the hope of obtaining a good argument from the multiplication of bad ones. To the Huttonian and every other geological sectary who relies on this postulate, I say, be cautious; "*incedis per ignes dolosos.*"

The only observation I recollect to have met with in favour of central heat is, that the deepest mines are the warmest—be it so! Might not a geologist by parity of reasoning argue thus?—In travelling from Rome to Chamonix, the country becomes continually more and more mountainous; some of the peaks of Chamonix are from ten to fifteen thousand feet above the level of the sea. Imagine, therefore, what they must be at Hamburgh!!

If mines derive their temperature from heat lodged in the centre of the earth, the temperature ought to vary with their distance from the centre, and therefore, since the earth is an oblate spheroid, the mines

of Scandinavia ought at the same depth from the surface to be proportionally warmer than those of tropical countries; a result which has never been, I believe, even suspected.

The existence of *Central Heat* in the sense and to the extent assumed in the Huttonian theory, is contrary to all our experience. If Heat there be in the Centre of the globe, it must have the properties of heat and none other. I ask not how the Heat originally was lodged in that situation, for the origin of all things is obscure; but I ask why, in the countless succession of ages which the Huttonian requires, the Heat has not passed away by conduction, and if it has passed away, by what other heat it has been replaced?

Dr. Chalmers in speaking of Sir Isaac Newton, observes, that it was a "distinguishing and characteristic feature of his great mind, that it "kept a tenacious hold of every position which had proof to substan- "tiate it; but a more leading peculiarity was, that it put a most de- "termined exclusion on every position destitute of such proof. The "strength and soundness of Newton's philosophy was evinced as much "by his decision on those doctrines of science which he rejected, as "by his demonstration of those doctrines of science which he was "the first to propose. He expatiated in a lofty region, where he met "with much to solicit his fancy, and tempt him to devious speculation. "He might easily have found amusement in intellectual pictures, he "might easily have palmed loose and confident plausibilities of his "own on the world. But no, he kept by his demonstrations, his mea- "surements, and his proofs."

Gentlemen, let us, as far as is consistent with the nature of geological investigation, show the strength and soundness of our philosophy in the same manner.

That Heat of considerable intensity prevails occasionally, in certain places, at some depth, is all that we have as yet clearly established. Whether that Heat is permanent, whether it is generally diffused, whether it is central, are questions of mere speculation.

Intimately connected with the hypothesis of *Central Heat* is that of *Refrigeration*.

It has been observed by one of our members, that "the Remains both 'of the animal and vegetable kingdom preserved in strata of different 'ages, indicate that there has been a great Diminution of Tempera- "ture throughout the northern hemisphere, in the latitudes now oc- "cupied by Europe, Asia and America; the change has extended to "the arctic circle as well as to the temperate zone; the heat and "humidity of the air, and the uniformity of climate, appear to have "been most remarkable when the oldest strata hitherto discovered "were formed. The approximation of a climate similar to that now "enjoyed in these latitudes, does not commence till the æra of the "formations termed tertiary; and while the different tertiary rocks "were deposited in succession, the Temperature seems to have been "still further lowered, and to have continued to diminish gradually "even after the appearance of a great portion of existing species upon "the earth." The little knowledge we have of the fossil productions of countries south of the temperate zone, induces me to believe that

these observations are as applicable to the southern hemisphere as to the northern.

This *Refrigeration*, one of the most undoubted facts in geology, is supposed by the Huttonians, and if I mistake not, by M. Elie de Beaumont and others, to arise from a decrease of the *Central Heat*; an opinion, however, which cannot, I think, be supported.

We know of one method only by which Central Heat, if it exists, can pass from the earth, viz. by Radiation. It cannot pass by Conduction. Conduction implies conductors, which in empty space are not to be procured*, but the Radiation of heat, at low temperatures, is so slight that it is scarcely sensible at 100° of Fahrenheit's thermometer, a temperature twice as great as the medium temperature of the surface of the globe at this time. The Temperature of the earth's surface has been shown by Fourier to be as constant as are the dimensions of its orbit, and the period of its annual revolution. Laplace observes, that our planet has undergone no Contraction of Size during the last 2000 years; consequently there has been no sensible *Refrigeration* during that period, and the last Seculum of M. de Beaumont has already extended to more than twice the length of a Millennium.

Another argument, or rather postulate, has been adduced in favour of *Central Heat*,—the Fusion of Unstratified Rocks, and their forcible Injection into the Stratified.

Gentlemen, I have confessed to you again and again, that I am not aware, nor has any one as yet informed me, by what test Stratified and Unstratified rocks can be distinguished; the only test I know is the good will and pleasure of those who make the distinction. The followers of Pluto seize and appropriate to his use as many rocks as they think proper. By virtue of such seizure, these Rocks become necessarily Unstratified: why so? because if Stratified they would be no longer Plutonic. Stratification I know is a question to be determined not by the senses but by the fancy; otherwise, I would say, that the magnificent range of basaltic cliff, which extends from the county of Derry along the coast of Antrim as far as Fairhead, is as distinctly stratified as any mountain-limestone, oolite or chalk in Great Britain.

However, I waive this objection as it leads me away from my subject, and return to the consideration of *Central Heat*. Have those who believe in this agent ever taken into their account the nature of the substances said to have been fused? Many of the trap rocks, not all of them, (for the family is large, and many of its members have been introduced into it, not by nature but by adoption,) I attribute to the agency of the causes which have produced lava, causes which, comparatively speaking, I do not believe to be very deep-seated. These rocks I put out of consideration for the present; the remarks about to be offered apply to granite and its congeners, under which head I would give to every one full liberty to include or reject quartz rock, gneiss, mica slate, eurite, cipollino, hornblende rock, serpentine, &c. Some or all of these, it is the bounden duty of *Central Heat* to fuse and to eject.

* See Comparative View of the Huttonian and Neptunian Systems of Geology.

Such and so limited are the means of Chemistry, that of many substances thus brought within the sphere of our inquiries, the point of fusion is at this day unascertained. The author of the masterly publication before adverted to, brought together many useful observations upon this subject. He observes that "Lavoisier could not melt a "particle of Carbonate of Lime by the intense heat of a burning mir- "ror, and that Quartz, according to Saussure, requires for its fusion "a temperature = 4043° of Wedgwood's pyrometer, Glass requiring "at a medium only 30° of the same scale."

That the Difficulty, which here suggests itself, of providing, in the absence even of imaginary fuel, a Supply of imprisoned Heat sufficient to fuse the substances I have mentioned and others scarcely less refractory, may be mitigated by extending the time employed in the process, or by the aid of compression and other circumstances, I am ready to admit; but, in the most favourable view of the case, the Heat wanted, (when we consider the thickness and extent of these rocks, comprising entire mountains and mountain chains,) must be prodigious; and I cannot but admire the singular taste of those geological speculators, who, enjoying the free range of the globe, have deposited their Caloric exactly in that spot in which it can be of least use to them. The inconvenience of this distribution becomes still more apparent when it is recollected that fusion is not all that is necessary; but that, when fused, these substances must be propelled in a determinate direction and with sufficient force, in many instances, to raise the bed of the sea to the height of an Alpine chain. I will not attempt to point out to you the way in which this is accomplished, but confess at once that I do not understand it.

And yet it appears certain that the surface of our planet has become cooler and cooler, from the period when organic life commenced to the tertiary epoch. If this cannot be explained by the Escape of Heat, there remains only one other mode of explaining it,—a continually diminishing Supply. The latter is the explanation offered by Mr. Lubbock. Sir John Herschel, also, has brought into view causes within the range of physical astronomy which, independently of a Loss of Internal Heat, produce a slow but certain Diminution of Temperature on the surface of our globe *. These auxiliaries, however, are insufficient.

Mr. Lyell has offered another solution of the problem, depending not on celestial but terrestrial causes. The chapter that contains it abounds in valuable information and ingenious reasoning; but when the author tells us that † in every country "*the land has been in some* "*parts raised, in others depressed, by which and other ceaseless changes,* "*the configuration of the earth's surface has been remodelled again and*

* The Baobab-tree of Senegal is supposed by Adanson to have attained the age of 5150 years, and De Candolle attributes to the *Cupressa disticha* of Mexico a still greater longevity. (Lyell, vol. iii. p. 99.)

If these opinions be correct, it seems improbable that any great change either of level or climate can have taken place at these spots within the last 5000 years.

† Principles of Geology, vol. i. p. 113.

" *again since it was the habitation of organic beings, and the bed of the* " *ocean lifted up to the height of the highest mountains,*" I cannot but wish that he had stated this as an opinion, not as a fact.

All these theories have one defect in common ; they do not meet the whole of the case. We have to explain not only the *Cooling gradual* during the long interval that occurred between the formation of the carboniferous beds and the chalk, but also the *Sudden Chill* which followed, and seems to have continued from that time to this. There is yet another element to be taken into account. The coal-beds of Melville Island contain various plants, natives of the country where they are found, and which, if we may trust analogy, require for their healthy growth or for their growth at all, not only tropical heat*, but a tropical apportionment of the periods of exertion and repose. It is a botanical impossibility that such plants could have flourished in a region in which they must have been stimulated by months of continuous Light, and paralysed by months of uninterrupted Darkness. The distribution of Light, therefore, as well as of Heat, must formerly have been different from what it is at present.

To meet this further difficulty, recourse is had to physical astronomy, which gives us the *Precession of the Equinoxes, and a Shifting Axis of Rotation* : but the periodical changes of astronomers are insufficient to explain the phenomena to which I have just drawn your attention. It has therefore been suggested that a greater change may, in the course of ages, have been produced on the axis of the earth's rotation by some foreign cause, say the *Collision of a Comet.*

Such change is undoubtedly possible, but of possibilities there is no end, and we must circumscribe our researches to render them useful. Sir John Herschel gives us no encouragement, therefore, to proceed with this speculation. Mr. Conybeare also dissuades us from it, but by an argument which to me at least appears inconclusive.

His argument, founded upon the lunar theory, is this,—that the internal strata of the earth are ellipses parallel to its external outline, their centres being coincident, and their axes identical with that of the surface. The present axis of the earth must therefore have been its axis from the beginning. It may have been so, yet I should like to be told by what process the form of the internal strata of the earth had been so nicely determined. Possibly, however, I may not understand the expression " internal *Strata.*" All I believe to be ascertained is, that of corresponding sections of the interior the density is nearly the same, and if so, my inference is, not that the earth has never changed its axis of rotation, but that if it has done so, the interior was then sufficiently pliant to accommodate itself to the change.

A much more formidable objection to the employment of such a cause is, that if once called in, we must take it with all its consequences. The effects produced by it will not be what we wish performed, but what its nature obliges it to perform. In explaining the

* Since this passage was written, doubts have been expressed whether the specimens of these plants preserved at the British Museum are sufficiently distinct to warrant the inference.

phenomena of Melville Island, it might render inexplicable those of the rest of the world. If we choose to change the axis upon which the earth revolves, let us at least fix upon the best time for doing it; now what is that time? immediately after the formation of the carboniferous series? The reduction of temperature at that epoch was inconsiderable; tropical plants and animals are found in the lias, in the oolite series, in the chalk. A much more convenient time would be on the first appearance of the tertiary rocks; but however satisfactory it might be to trace to such a cause the violent changes and disturbances which appear to have taken place about that period in all other parts of the world, I am afraid our satisfaction would be greatly diminished on finding that Gosau and Maestricht * escaped unhurt.

Be the cause what it may, the effect is certain. The Temperature of the Crust of the Earth must have been higher when the Coal-measures were deposited than now, and we have reason to think it was still higher at antecedent periods. That a considerable degree of Heat still exists, either partially or generally, at no great distance from the surface, appears from thermal springs and volcanoes.

I am aware that the doctrine of *Internal Cavities* has been regarded as visionary; and in the extent to which it was carried by some of the old Cosmogenists it was so; but that comparatively near to the surface, there are, I do not say Vacuities, but large Spaces unoccupied by solid matter, is not only probable, but almost proved. It seems, indeed, to be a necessary consequence of the structure of the crust of the earth. No miner has ever got to the bottom of a vein, and a vein itself is often a half empty pipe or fissure. The correspondence of the phases of distant volcanoes, the continuous ranges of their eruptive openings, the vast extent of territory shaken simultaneously by their convulsions, are so many proofs of communication below the surface. The bulk of the ejected matter cannot be less than that of the concreted ejections which we see; for at the temperature of fusion it is greater than at a lower temperature, and for every foot of matter ejected, it is necessary to provide a substitute in the place which it occupied.

The continuous streams of lava which issued in Iceland, on one occasion, attained the length of forty or fifty miles. But the bulk of volcanic matter presented to view, does not enable us to form a correct estimate of the quantity of matter ejected; we must take further into account the combustible substances which have vanished, the gases which have escaped, the dust and ashes which, projected into the air, have fallen many miles distant from the place of explosion †. Then only can we entertain a just idea of the Cavities that must have been created in the interior of the earth by the escape of a mass of matter competent to produce an Etna or a Chimboraço. Such Cavities are ill suited to support such Mountains; La Metherie therefore supposes Cavities to be at a distance, and volcanic matter to flow

* See the descriptions of these places in Geol. Trans.

† In 1783, a submarine Volcano off the coast of Iceland ejected so much pumice that the ocean was covered to a distance of 150 miles, and Ships were considerably impeded in their course.

from these through long galleries and fissures of communication. Nor have we in volcanic countries alone decisive evidence of the existence of subterranean Cavities. No rock is exempt from Fissures: in thick beds of limestone Fissures and Caverns are exceedingly abundant; and the extent of these last is sometimes prodigious. Who has not heard of the Grotto of Antiparos? of the Caverns of Carinthia and Carniola, the content of which amounts to some hundred thousand cubic feet? of the Kingston Cave recently explored near Michelstown in Ireland?

To the frequency of Caverns and Openings, by whatever name designated, I ascribe many of the inequalities which vary the surface of the earth; such openings, I conceive, produce phenomena sometimes of Subsidence, sometimes of Elevation. I cannot entertain a doubt, that many of the tilts and contortions of strata usually ascribed to *Soulèvement*, have been occasioned solely by want of adequate support.

The Duchy of Finland exhibits an endless series of lakes filling up the hollows of a granitic surface. Let me be allowed a similar series of subterranean lakes occupying similar basins beneath the level of the Baltic, and receiving, by means of Fissures extending up to the summits of the Scandinavian chain, a continual supply of water which has no outlet; in other words, let me be allowed the use of hydrostatic pressure; and without having recourse to central heat or secular refrigeration, I think I shall be able to account, without difficulty, not by a general and uniform Rising, but by a number of unequal and partial Risings, for the phenomena observed along the shores of the Baltic.

Steam is often referred to as capable of producing the same result, nor will I deny that it might do so under favourable circumstances; but I apprehend Steam rarely does act in nature under such circumstances; for its existence depends on the access of heat, and its force on close confinement, contingencies not very likely to occur in the porous and fissured strata of the earth. Any of the various Gases, if compressed, might also become agents of elevation, but only under the same conditions as steam.

I have reserved for the last the popular theory which accounts for Elevation by the forcible *Inroad of igneous rocks into sedimentary*.

To put this theory to the test, it is natural to inquire, what igneous rocks are. My answer is, whatever geological speculators think proper to call so. The late Professor Dugald Stewart cautioned us strongly, though, alas! in vain, to avoid the language of theory. Appearances, he observes, "should always be described in terms which involve no "opinion as to their causes. These are the objects of separate ex-"amination, and will be best understood if the facts are given fairly, "without any dependence on what should yet be considered as un-"known; this rule is very essential where the facts are in a certain "degree complicated."

In dealing out to rocks the appellation of *igneous*, some geologists are more liberal than others. I have not time to enumerate the various rocks which enjoy this title, still less to investigate their respective claims to retain it. I will therefore content myself with observing, that in the scantiest catalogue they are many in number, and consequently, if ejected in a state of fusion, must have been ejected from different reservoirs and cauldrons, not from a *central* cauldron.

That any rock whatever was originally igneous, is a gratuitous assumption. Lavas themselves may be, and probably are, in very many cases, Rocks not originally igneous, but Rocks which have been exposed at one time or other to the action of fire.

Granite is one of the rocks most usually considered as an *Agent in Elevation*, for what reason I am at a loss to discover. Solid Granite has no inherent principle of motion; if it move, it can only be by virtue of the impulsion it has received from some other body, not in consequence of its igneous origin or its want of stratification. The disturbances of strata that adjoin granite are not more constant, nor more striking nor more extensive than those of strata far remote from it, as for instance, the limestone shales of Derbyshire or the coal-beds of Liege. Granite veins are too small to raise mountains, and the changes or anomalies that take place at the junction of granite with other rocks, whatever else they may prove, appear to me to have no bearing on the question of *Elevation*. On the other hand, the arguments adduced against the doctrine that Granite while fluid has been forcibly injected from beneath into its present position, are to my mind conclusive; especially that which is founded on the frequent transition which takes place from Granite to the rocks that adjoin it. We find a continuous series from Granite through Gneiss and Mica slate to Clay-slates and the Fossiliferous Slates; and it is not possible to stop at any point of this progress, and to say in which direction the tendency is strongest. If the gradation were single, the difficulty would be great, but what shall we say to a repetition of such gradations? In Mr. Weaver's paper on the East of Ireland, two detailed sections are given, in one of which, more than six layers of Granite alternate with as many of Mica slate, and in the other five alternations of the same kind occur, the rocks in each instance forming bands from three to seventy fathoms in thickness.

The reliance which some authors place on Granite and other unstratified rocks, as *Agents of Elevation*, is to me very extraordinary; let one instance suffice. At Castrogiovanni in Sicily, the Pleiocene Beds attain an altitude of three thousand feet; hence it has been inferred, that *since these beds were deposited, there has been formed and introduced into the beds subjacent, a body of Granite, Sienite, Porphyry or other crystalline and unstratified Rocks three thousand feet in thickness.* This supposition is said to be necessary, but since I do not see the necessity, I will venture another supposition, viz. that Etna has not risen to the height of ten thousand feet without occasioning large cavities in its neighbourhood, some of them submarine; that Castrogiovanni is situate over one of these; that the Pleiocene strata have closed the cavity and rendered it water-tight, except on the side of Etna; from whose lofty flanks and cloud-capped crater the caverns beneath are regularly supplied by fissures with rain-water and melted snow. Let the author grant me so much,—I ask no more. The hydrostatic paradox has tripped up the hills of the geological one, and I behold my Pleiocene beds mounted at once on a pedestal three thousand feet high, and capable of still further promotion.

If the explanation here offered meets the case of Castrogiovanni, it will equally account for the height of the tertiary beds in different

parts of the Val di Noto, and for similar phenomena in every country which is or has been formerly the site of volcanic eruptions.

To the appearances on the Gulf of St. Lawrence, described by Captain Bayfield, I have already adverted.

My Predecessor directed your attention last year to the existence in the Morea of four or five distinct Ranges of ancient Sea-cliffs, marked at different levels in the limestone escarpments by lithodomous perforations, lines of littoral and sea-worn caverns, and other striking proofs of former tidal action. Similar Terraces have been observed in Sicily, in Chili, in the Gulf of St. Lawrence and various other places. At Uddevalla in Sweden, are ancient Beaches with shells of living species, two hundred feet above the level of the Baltic, a height strikingly disproportionate to the very moderate Rise ascertained to have taken place in other parts of the Scandinavian coast: many examples of similar phenomena have been found in Great Britain. It would be rash to offer a solution of these phenomena in the gross. Every individual case deserves separate examination. All I undertake at present is to put a new key into the hands of the decipherer.

It was my intention on commencing this address to have discussed at some length the theory of M. Elie de Beaumont, but there is not time now to do it justice. He belongs to that class of authors whose opinions, right or wrong, always instruct me. There is no part of his theory which does not evince thought and diligence, a habit of correct observation and an enlarged mind. In some respects I differ from him, and it will not be difficult to infer from what I have already said, wherein the difference consists. Should these observations engage his notice, I would beg him to consider whether the disturbances in the Alps and elsewhere have not been generalized rather more than they will bear, whether the tilts and upliftings may not have taken place bit by bit at various epochs, and whether, if the *secular Refrigeration of the Globe* cannot be established, and *Central Heat* be an *Ignis fatuus*, his attention may not be usefully directed to more partial but better authenticated sources of disturbance and elevation.

Allow me, in conclusion, to say a few words upon a subject in connexion with which my name has of late been brought forward much more prominently than I could have desired;—I mean *Diluvial Action*.

Some fourteen years ago I advanced an opinion, founded altogether upon physical and geological considerations, that the entire earth had, at an unknown period, (as far as that word implies any determinate portion of time,) been covered by one general but temporary Deluge. The opinion was not hastily formed. My reasoning rested on the facts which had then come before me. My acquaintance with physical and geological nature is now extended; and that more extended acquaintance would be entirely wasted upon me, if the opinions which it will no longer allow me to retain, it did not also induce me to rectify. New data have flowed in, and with the frankness of one of my predecessors, I also now read my recantation.

The varied and accurate researches which have been instituted of late years throughout and far beyond the limits of Europe, all tend to this conclusion, that the geological schools of Paris, Freyberg and London have been accustomed to rate too low the various forces

which are still modifying, and always have modified, the external form of the earth. What the value of those forces may be in each case, or what their relative value, will continue for many years a subject of discussion; but that their aggregate effect greatly surpasses all our early estimates, is I believe incontestably established. To Mr. Lyell is eminently due the merit of having awakened us to a sense of our error in this respect. The vast mass of evidence which he has brought together, in illustration of what may be called *Diurnal Geology*, convinces me that if, five thousand years ago, a Deluge did sweep over the entire globe, its traces can no longer be distinguished from more modern and local disturbances. The first sight of those comparatively recent assemblages of strata, which he designates the *Eocene*, *Meiocene* and *Pleiocene* Formations, (unknown but a few years ago, though diffused as extensively as many which were then honoured with the title of universal,) shows the extreme difficulty of distinguishing their detritus from what we have been accustomed to esteem Diluvium. The Fossil Contents of these formations strongly confirm this argument. M. Deshayes has shown that they belong to a series unbroken by any great intervals, and that, if they be divided from the secondary strata, the chasm can have no relation to any such event as is called The Flood.

Further, the elephants and other animals once supposed to be exclusively *Diluvial*, are now admitted to be referrible to two or three distinct epochs; and it is highly probable that the blocks of the Jura Mountains, of the North of Germany, of the North of Italy, of Cumberland, Westmorland, &c., are not the waifs and strays of one, but of several successive Inundations.

It is, Gentlemen, a well-known rule of such institutions as ours, that the "Authors alone are responsible for the facts and opinions contained in their respective productions." Under that feeling have I spoken on the present occasion, and having freely set before you what has occurred to me on some points of general interest to our science at this time, I think it my duty, in concluding this address, to disclaim and deprecate any attempt to connect what I have here expressed with the general sentiments of the Geological Society. The opinions I have uttered are my own, and I should be sorry that more importance should be attached to them than they intrinsically deserve, from the accident of their having been delivered from this Chair. Had not the whole responsibility fallen on myself, I should have hesitated, or perhaps altogether forborne to bring before you Opinions, several of which I know are little in accordance with those of some of the most distinguished members of our association.