The George Huntington Williams Memorial Lectures

on

THE PRINCIPLES OF GEOLOGY

VOLUME ONE

ARCHIBALD GEIKIE

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THE FOUNDERS OF GEOLOGY

BY

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INTRODUCTORY NOTE

The present publication is the first of a series of volumes, containing lectures by eminent geologists on The Principles of Geology, to be published under the auspices of the Geological Department of the Johns Hopkins University, as a tribute to the memory of Dr. George Huntington Williams, late Professor of Inorganic Geology, who died July 12, 1894. The lectures are provided by the generosity of Mrs. George Huntington Williams who thus desires to offer to the students of the Johns Hopkins University and to the wider circle of workers in this and other countries the latest conceptions of the fundamental principles of geological science. With this end in view, investigators of distinction will be asked to visit Baltimore from time to time to lecture before the students of the University and invited guests from other American institutions.

Two such courses of lectures have already been given, the first in April, 1897, by Sir Archibald Geikie, F. R. S., Director-General of the Geological Survey of Great Britain and Ireland, and the second in April, 1900, by Professor W. C. Brögger of the University of Christiania, Norway.

George Huntington Williams was born in Utica, New York, January 28, 1856. He was the eldest son of Robert Stanton and Abigail Obear (Doolittle) Williams, whose ancestry was of the sturdy Puritan type, the grandparents of both having emigrated from New England toward the close of the eighteenth century. His immediate ancestors were men of intellectual vigor and prominent in the affairs of the community in which they lived. Surrounded in his youth with the refinements which an educated family life can give, Williams spent his school days in Utica, passing through the various grades of the public schools and finally graduating with valedictory honors from the Utica Free Academy. In the autumn of 1874 he entered Amherst College, graduating in the class of 1878. Systematic and conscientious in every detail connected with his school and college life, he then formed habits of mind which characterized his maturer years. In a remarkable degree the youth was father to the man.

After spending the greater portion of the year succeeding graduation as an advanced student at his Alma Mater, he left in July, 1879, to continue his studies in Germany. Going first to Göttingen, at the opening of the winter semester, he came under the influence of Professor Klein which had much to do with the mineralogical trend of his future work. The following year he entered the University of Heidelberg where the great teacher Professor Rosenbusch was drawing to his laboratory those who were anxious to enter the comparatively new domain of microscopical petrography. It was here that the young geologist acquired the exact methods of microscopical investigation which so largely characterized his later work. At the close of the second year under his distinguished teacher, Williams received in December, 1882, the degree of Doctor of Philosophy, his thesis dealing with the eruptive rocks of the region of Tryberg in the Black Forest.

Dr. Williams returned to America at the close of 1882, and in the following March accepted the position of Fellow by Courtesy in the Johns Hopkins University. A year later he became a member of the academic staff with the title of Associate in Geology, which position he held until 1885 when he was made Associate Professor. In 1892 he became Professor of Inorganic Geology. Upon him devolved the organization of the department of geology, and the high efficiency which it attained owed much to his conception of what such a department in a university should be.

From the beginning of his service at the University Dr. Williams directed his attention to a study of Maryland geology, and more especially to the Piedmont area lying to the west of Baltimore. Important problems in rock metamorphism here presented themselves and as a result of this study, numerous contributions were made to scientific journals in this country and in Europe. Later his investigations embraced the South Mountain district with its wonderful exhibition of ancient volcanic rocks of both acid and basic types. A publication upon this district and its wider extension along the eastern border of the continent, made but a short time before his death, was one of Dr. Williams' most important contributions to geological science. Much of the work in the Maryland area was done under the auspices of the U. S. Geological Survey, with which organization Dr. Williams was closely connected. He valued these opportunities not only as a field for personal research but also as nature's laboratory in which young men might be trained in the most exact methods of scientific investigation. From the very first his enthusiasm and his lucid interpretation of geological science drew students to him, while his devotion to their interests made a close bond of sympathy which lasted beyond their student days.

While engaged primarily in the study of the geology of Maryland, Dr. Williams took up other problems during his absence from Baltimore in vacation time, collecting data and materials that formed the basis for more extended examination in the laboratory. Among these important studies may be mentioned his investigations of the greenstone-schists of the Menominee and Marquette regions, an important work on the metamorphism of eruptive rocks, and also his study of the Cortlandt series of the Hudson with their contact phenomena.

Professor Williams was always deeply interested in methods of teaching, and the needs of his class-room work were kept constantly in view. This led to the preparation of a number of important papers along these lines and also to his text-book on the Elements of Crystallography.

Professor Williams was an effective speaker and his public addresses, whether before audiences at his own university or elsewhere, did much to extend the knowledge of that branch of geology to which he had given his life.

The interests of the Johns Hopkins University, which he served during a period of nearly twelve years, were ever before him, and, whenever the opportunity offered, he sought their advancement with a loyalty which was cordially appreciated by all friends of the institution.

As one of his biographers has said, "The output of his academic work as embodied in his students has stamped a value upon it which cannot now be estimated, but its success in the eyes of those who were watching from positions of close association is expressed in the memorial minute adopted by the board of trustees and the academic staff of the University, in which they bear testimony to 'his alert, inquisitive observation; the clear judgment and sound reasoning which he brought to the interpretation of what he saw; his excellent power of statement, whether with voice or pen; his cultivated appreciation of literature; the energy, hopefulness, and enthusiasm which he carried into his work and imparted to his associates; his genuine individual interest in his students; the friendliness and helpfulness of his relations to his colleagues, and his readiness to cooperate in every worthy undertaking.'"

WM. BULLOCK CLARK.

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PREFACE

When the President of the Johns Hopkins University invited me to inaugurate the Lectureship founded in that seminary by Mrs. George Huntington Williams in memory of her husband, the distinguished and widely-regretted Professor of Geology there, I gladly availed myself of the opportunity thus afforded me of renewing personal relationships with the geologists of the United States, and of thus helping to draw closer the bonds of sympathy which unite the students of nature in the Old World and in the New.

In making choice of a subject that would be appropriate for the required course of lectures, I was influenced by the announcement that geologists from all parts of the States and from Canada would be asked to meet me in Baltimore. As my audience might thus include representatives of every department in the wide domain of geological science, it was obviously desirable not to select for treatment any limited field in that domain, but rather to choose some topic of general interest in which students of every part of the science might meet as on common ground. I had often been struck by the limited acquaintance with the historical development of geology possessed even by men who have done good service in enlarging the boundaries of the science. English and American geologists have for the most part contented themselves with the excellent, but necessarily brief summary of the subject given by Lyell in the introductory chapters of his classic *Principles*, no fuller digest of geological history having been published in their language. It seemed to me, therefore, that perhaps no more generally interesting and appropriate theme for my purpose could be selected than the story of the evolution of geology. The necessarily restricted number of lectures to be given would not admit of a discursive survey of the whole history, but a useful end might doubtless be served if a limited period of geological progress were selected and treated in rather greater detail than had hitherto been usual, and with especial reference to the personal achievements of the leaders to whose labours the progress had mainly been due. Accordingly I determined to take this subject for my discourses, and to select the period between the middle of the last and the close of the second decade of the present century—an interval of about seventy years, full of peculiar interest in the development of science, for they witnessed the laying of the foundations of geology.

But even of this limited section of the history it was obviously impossible to attempt an exhaustive discussion. Without for a moment aiming to cover all the ground, I deemed that a useful task might still be undertaken if the story of a few of the great pioneers were briefly narrated, and if from their struggles, their failures and their successes, it could be indicated how geological ideas and theories gradually took shape. Such was the origin and aim of the following lectures.

The personal intercourse to which, after an absence of eighteen years, I looked forward with vivid interest proved to be a source of the keenest enjoyment. Renewing old friendships with some of the veterans of the science, and forming fresh ties of sympathy with many younger workers who have come to the front in more recent years, I could not but be impressed by the extraordinary vitality which geology has now attained in the United States. Every department of the science has its enthusiastic votaries. Surveys, professorships, museums, societies, journals in almost every State are the outward embodiment of the geological zeal that appears to animate the whole community. This remarkably rapid development of the science has not arisen from any influence derived from without, but springs, as it seems to me, from the marvellous geological riches of the American continent itself. In minerals and rocks, in stratigraphical fulness, in palæontological profusion, in physiographical illustrations, the United States have not only no need to borrow from Europe, but in many important respects can produce examples and materials such as cannot be equalled on this side of the Atlantic. Had the study of the earth begun in the New World instead of the Old, geology would unquestionably have made a more rapid advance than it has done. The future progress of the science may be expected to be largely directed and quickened by discoveries made in America, and by deductions from the clear evidence presented on that continent.

Amidst an almost feverish earnestness in the prosecution of investigation there is unavoidably a risk that the present activities of the science, with their engrossing interest, may lead to neglect of its past history, and that in this way unintentional injustice may be done to the labours of the earlier workers, while at the same time the vision of living students may be narrowed by the too exclusive contemplation of what they and their comrades are engaged upon. In inaugurating the Williams Lectureship at Baltimore I hoped that by turning aside to the achievements of half-forgotten pioneers, I might in some small measure help to counteract this tendency by recalling attention to examples of strenuous and successful labour among the Founders of Geology.

Geological Survey Office,
28 Jermyn Street, London,
18*th June* 1897.

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LECTURE I

The Cosmogonists—First beginnings of accurate and detailed observation regarding the Earth's crust and its history—Guettard and his labours.

In science, as in all other departments of human knowledge and inquiry, no thorough grasp of a subject can be gained, unless the history of its development is clearly appreciated. While eagerly pressing forward in the search after the secrets of Nature, we are apt to keep the eye too constantly fixed on the way that has to be travelled, and to lose sight and remembrance of the paths already trodden. Yet it is eminently useful now and then to pause in the race, and to look backward over the ground that has been traversed, to mark the errors as well as the successes of the journey, to note the hindrances and the helps which we and our predecessors have encountered, and to realize what have been the influences that have more especially tended to retard or quicken the progress of research.

Such a review is an eminently human and instructive exercise. Bringing the lives and deeds of our forerunners vividly before us, it imparts even to the most abstruse and technical subjects much of the personal charm which contact with strenuous, patient, and noble natures never fails to reveal. Moreover, it has a double value in its bearing on our own progress in scientific work. A retrospect of this kind leads to a clearer realization of the precise position at which we have arrived, and a wider conception of the extent and limits of the domain of knowledge which has been acquired. On the other hand, by enabling us to comprehend how, foot by foot, the realms of science have been painfully conquered, it furnishes suggestive lessons as to tracks that should be avoided, and fields that may be hopefully entered.

In no department of natural knowledge is the adoption of this historical method more necessary and useful than it is in Geology. The subjects with which that branch of science deals are, for the most part, not susceptible of mathematical treatment. The conclusions formed in regard to them, being often necessarily incapable of rigid demonstration, must rest on a balance of probabilities. There is thus room for some difference of opinion both as to facts and the interpretation of them. Deductions and inferences which are generally accepted in one age may be rejected in the next. This element of uncertainty has tended to encourage speculation. Moreover, the subjects of investigation are themselves often calculated powerfully to excite the imagination. The story of this earth since it became a habitable globe, the evolution of its continents, the birth and degradation of its mountains, the marvellous procession of plants and animals which, since the beginning of time, has passed over its surface,—these and a thousand cognate themes with which geology deals, have attracted numbers of readers and workers to its pale, have kindled much general interest, and awakened not a little enthusiasm. But the records from which the chronicle must be compiled are sadly deficient and fragmentary. The deductions which these records suggest ought frequently to be held in suspense from want of evidence. Yet with a certain class of minds fancy comes in to supply the place of facts that fail. And thus geology has been encumbered with many hypotheses and theories which, plausible as they might seem at the time of their promulgation, have one by one been dissipated before the advance of fuller and more accurate knowledge. Yet before their overthrow, it may often be hard to separate the actual ascertained core of fact within them, from the mass of erroneous interpretation and unfounded inference that forms most of their substance.

From the beginning of its growth, geology has undoubtedly suffered from this tendency to speculation beyond the sober limits of experience. Its cultivators have been often described as mere theorists. And yet in spite of these defects, the science has made gigantic strides during the last hundred years, and has gradually accumulated a body of well-ascertained knowledge regarding the structure and history of the earth. No more interesting record of human endeavour and achievement can be found than that presented by the advance of geology. A hundred years ago the science had no generally acknowledged name and place in the circle of human studies, and now it can boast a voluminous literature, hundreds of associations all over the world dedicated to its cultivation, and a state organization in almost every civilized country for its systematic prosecution. I propose to ask you to trace with me some of the leading steps in this magnificent progress. Even speculations that have been thrown aside, and theories that have been long forgotten, may be found to have been not without their use in promoting the general advance.

If all history is only an amplification of biography, the history of science may be most instructively read in the life and work of the men by whom the realms of Nature have been successively won. I shall therefore dwell much on the individual achievements of a few great leaders in the onward march of geology, and indicate how each of them has influenced the development of the science. At the same time, I shall trace the rise and progress of some of the leading principles of modern geology, which, though now familiar to us all as household words, are seldom studied in regard to their historical development. Thus, partly in the life-work of the men, and partly in the growth of the ideas which they promulgated, we shall be able to realize by what successive steps our science has been elaborated.

The subject which I have chosen, if treated as fully as it might fitly be, would require a long course of lectures. Within the limits permissible to me, I can only attempt to present an outline of it. Instead of trying to summarize the whole progress of modern geology, I think it will be more interesting and profitable to dwell somewhat fully on the labours of a few of the early masters, to touch only lightly on those of their less illustrious contemporaries, and to do little more than allude to the modern magnates whose life and work are generally familiar. I have accordingly selected for more special treatment the period which extends from the middle of last century to the earlier decades of this, or a period of about seventy years. A few later conspicuous names will require some brief notice in order to fill up the general outlines of our picture.

Every geologist is familiar with the account of geological progress sketched by Lyell in the first four chapters of his *Principles.* I need not therefore offer even the briefest summary of what preceded the period which is now to be illustrated in fuller detail. Let me merely recall to your memory the early work of the Italian observers who, with the evidence before their eyes of active and extinct volcanoes, of upraised shell-beds and abundant traces of former terrestrial vicissitudes, took broad views of the history of the earth, and arrived at conclusions which have been sustained and amplified by later generations. The labours of Steno, Vallisneri, Moro, and Generelli furnished a body of fact amply sufficient to disprove the current fantastic theories of the earth and to lay some of the foundation-stones of modern geology. Woodward, Lister, Hooke and Ray may be quoted as notable writers in England by whom, in spite of their imperfect knowledge and frequent mistakes, the rise of geology was heralded. Hooke is especially deserving of an honoured place among the early pioneers of the future science.

The true scientific spirit of observation and experiment had long been abroad and at work in many branches of inquiry before it became dominant in the geological field. The necessity for a close scrutiny of nature as the basis of sound deduction had for generations been recognized by some of the more thoughtful minds before it was developed into a system by Bacon. Even as far back as the latter half of the sixteenth century, the method of practical research, as opposed to mere book-knowledge and theory, had been advocated even for the investigation of the rocky part of the earth. It was proclaimed, in no uncertain voice, by the learned and versatile Dane, Peter Severinus, who counselled his readers thus: "Go, my sons, sell your lands, your houses, your garments and your jewelry; burn up your books. On the other hand, buy yourselves stout shoes, get away to the mountains, search the valleys, the deserts, the shores of the sea, and the deepest recesses of the earth; mark well the distinctions between animals, the differences among plants, the various kinds of minerals, the properties and mode of origin of everything that exists. Be not ashamed to learn by heart the astronomy and terrestrial philosophy of the peasantry. Lastly, purchase coals, build furnaces, watch and experiment without wearying. In this way, and no other, will you arrive at a knowledge of things and of their properties."[1] The modern spirit of investigation in natural science could not be more clearly or cogently enforced than it was by this professor of literature and poetry, of meteorology and of medicine, in the year 1571.

In spite of such teaching, it was long before what is now regarded as the domain of geology was definitely recognized as one that was not to be surrendered to mere fanciful speculation, but which offered an unsurpassed wealth of material for the most sedulous observation and the most scrupulous induction. The chief obstructors of progress in this department of human inquiry were the

[1] Petrus Severinus, *Idea Medecinae Philosophicae*, 1571, p. 73, cap. vii. De principiis corporum (cited by D'Aubuisson).

cosmogonists who, often with the slenderest equipment of knowledge of nature, endeavoured to account for the origin of things. They were not disconcerted by phenomena that contradicted their theories, for they usually never saw such phenomena, and when they did, they easily explained them away.

Many of these writers were divines, yet even when they were laymen they felt themselves bound to suit their speculations to the received interpretation of the books of Moses. Looking back from our present vantage ground, it is difficult to realize that even the little which had been ascertained about the structure of the earth was not sufficient to prevent some, at least, of the monstrous doctrines of these theorists from being promulgated. It was a long time before men came to understand that any true theory of the earth must rest upon evidence furnished by the globe itself, and that no such theory could properly be framed until a large body of evidence had been gathered together.

By a curious coincidence, the writings of the two last and most illustrious of the cosmogonists appeared during the middle of last century in the same year. Leibnitz (born in 1646), fascinated by the problems connected with the history of the earth, had formulated his prescient views on this subject in his *Protogaea*, but this work was not fully published until long after the death of its author. Adopting the idea of Descartes, he believed that at first our planet was a regularly shaped globe of molten liquid which gradually cooled and solidified, and in so doing assumed irregularities of surface which formed mountain chains. The first crust solidified as the most ancient or "primary" rocks, and as the cooling continued, the vapours condensed into a universal ocean of water. The interior of the planet possessed a vaulted or vesicular structure, and the roofs of the vast caverns, from time to time giving way, produced dislocations and inundations, while large bodies of water were drawn off into the interior, and the dry land progressively increased in area and height. By the commotion of the waters large quantities of sediment were produced, which, during the intervals between the disturbances, were deposited over the sea-floor to form the various strata that are to be seen on the land. By a repetition of the same sequence of events, the strata were broken up, and new deposits were laid down upon them, until, as the agencies became quiescent and balanced, a "consistentior rerum status" emerged.

The other cosmogonist, Buffon, born in 1707, had likewise conceived broad and profound ideas regarding the whole realm of Nature. Endowed with a spirit of bold generalization, and gifted with a style of singular clearness and eloquence, he was peculiarly fitted to fascinate his countrymen, and to exercise a powerful influence on the scientific progress of his age. He is the central figure in a striking group of writers and observers who placed France in the very front of the onward march of science, and who laid some of the foundation-stones of modern geology.

The introductory portion of Buffon's voluminous *Natural History* was devoted to a theory of the earth. Though written in 1744, it was not published until 1749. The author had meditated long and deeply on the meaning of the fossil shells found so abundantly among the rocks of the earth's crust, and had recognized that, as they demonstrated the condition of the globe not to have been always what it is now, any true theory of the earth must trace the history of the planet back to a time before the present condition was established. With great insight, he saw that this history must be intimately linked with that of the solar system, of which it formed a part. He thought that the various planets were originally portions of the mass of the sun, from which they were detached by the shock of a comet, whereby the impulse of rotation and of revolution in the same general plane was communicated to them. In composition, therefore, they are similar to their parent sun, only differing from that body in temperature. He inferred that at first they were intensely hot and self-luminous, but gradually became dark as they cooled, the central sun still remaining in a state of incandescence.

Though we now believe the hypothesis of a cometary shock to be untenable, it is impossible to refuse our admiration to the sagacity of the man who first tried to solve the problem of the solar system by the application of the laws of mechanics. His theory, however, was loaded with several crude conceptions. The enormous numbers and wide diffusion of fossil shells, which had so vividly impressed his imagination, proved to him that the land must have lain long under the sea. But he had no idea of any general cause that leads to elevation of the sea-bottom into land. He was thus constrained to resort to his imagination for a solution of the problem. Burnet had supposed the original ocean to be contained within the earth, and that it only escaped at the time of the Flood, when, by the heat of the sun, the crust of the globe had cracked, and thus allowed the pent-up waters to rush out. Buffon's theory was hardly less fanciful. But he reversed the order of events. He inferred from the abundance of fossil shells that there had once been a universal ocean, and that by the giving way of the crust, a portion of the waters was engulfed into caverns in the interior, so as to expose what are now mountains and dry land.

For some thirty years after the publication of his *Theory*, Buffon continued to work industriously in all departments of natural history. At last, in 1778, he returned to the question of the origin of the earth and published his *Epochs of Nature*. In this work he arranged the history of the globe in six epochs—intervals of time of which the limits, though indeterminate, seemed to him none the less real. The first epoch embraced the primeval time when the earth, newly torn from the sun, existed still as a molten mass which, under the influence of rotation, assumed its oblate spheroidal form. The transition from fluidity to solidity, and from luminosity to opacity was brought about entirely by cooling, which commenced at the outer surface. A crust was thus formed, outside of which the substances, such as air and water, which were not solidified by the lowering of temperature, remained as a fluid or æriform envelope, while the interior still continued for a long time liquid.

Buffon's second epoch was characterized by the consolidation of the molten globe, and the appearance of hollows and ridges, gaps and swellings, over its surface. These inequalities in the crust of granite, gneiss and other ancient crystalline rocks, gave rise to the mountains and valleys of the higher portions of the land.

The third epoch included the time when the sea stood from 9000 to 12,000 feet higher than it does now, as was supposed to be indicated by the heights at which marine organisms are found in the rocks. It was then that the calcareous fossiliferous formations were formed, which constitute so much of the present dry land.

The fourth epoch witnessed the emergence of the lower part of the land, owing to the sinking of the waters through cracks into cavities in the interior of the globe. Profoundly as Buffon had meditated on the structure of the earth, he had thus during thirty years made no advance in his views of the origin of the dry land.

The fifth epoch was marked by the advent of huge pachyderms—elephants, rhinoceroses, and hippopotamuses, which, though now confined to warm regions, then wandered far into the north.

The sixth epoch saw the separation of the two continents which, as was inferred from the presence in each of them of what were believed to be the same fossil mammals, were originally united.

Buffon added a seventh epoch, in which he traced the commanding influence of man in modifying the surface of the earth.

Recognizing the powerful agency of rivers and the sea in washing away the materials of the land, he believed that by this action the whole of the existing continents will finally be reduced and covered by the ocean; and he conceived that by the same series of changes new lands will ultimately be formed.

For breadth and grandeur of conception Buffon far surpassed the earlier writers who had promulgated theories of the earth. The rare literary skill with which he presented his views enabled him to exercise a powerful influence on his contemporaries, and to direct their attention to the deeply interesting problems of which he wrote. Yet he could never shake himself free from the theological bias which had so long lain as an incubus upon the progress of scientific investigation. The theological doctors of the Sorbonne compelled him to publish a retractation even of the very guarded statements he had presented in his *Theory of the Earth* — statements which are now accepted among the obvious commonplaces of science. Although the second treatise shows that the long interval of thirty years had given him greater freedom and had still further enlarged his views of nature, he was evidently unaware of much that had been observed and described during that interval by his own countrymen and in other parts of Europe. His eloquent pages are rather a pictorial vision of what his brilliant imagination bodied forth as the origin of things, than a sober attempt to work out a theory on a basis of widely collected, carefully sifted and systematically co-ordinated facts.

Among Buffon's contemporaries there lived a man of a totally different stamp, who, shunning any approach to theory, dedicated himself with the enthusiasm of a true naturalist to the patient observation and accumulation of facts regarding the rocks of the earth's crust, and to whom modern geology owes a deep debt of gratitude, that has never yet been adequately paid. This man, Jean Étienne Guettard (1715-1786), was born in the year 1715 at the little town of Étampes, about thirty miles S.W. from Paris.[1] As

[1] For the biographical facts here given I am indebted to the *Éloge* of

the grandson of an apothecary there, he was destined to succeed to the business of compounding and selling drugs. Before he left home for his professional education, he had already developed a passion for natural history pursuits. When still a mere child, he used to accompany his grandfather in his walks, and his greatest happiness was found in collecting plants, asking their names and learning to recognize them, and to distinguish their different parts. Every nook and corner around Étampes became familiar to him, and in later years he loved to revisit, with the eye of a trained naturalist, the scenes which had fascinated his boyhood. In his writings he loses no opportunity of citing his native place for some botanical or geological illustration. Thus, at the very beginning of a long and suggestive memoir on the degradation of mountains, to which further reference will be made in the sequel, his thoughts revert to the haunts of his infancy, and the first illustration he cites of the processes of decay which are discussed in that paper is taken from a picturesque rock overlooking the valley of the Juine, under the shade of which he used to play with his companions.[1]

Having gained the favourable notice of the famous brothers Jussieu, who gave renown to the botanical department of the Jardin des Plantes, he was allowed by his grandfather to choose a career that would afford scope for his ardour in science. Accordingly he became a doctor in medicine. Eventually he was attached to the suite of the Duke of Orleans, whom he accompanied in his travels,

Guettard by Condorcet (*Oeuvres*, edit. 1847, vol. iii. p. 220) and to the personal references which I have met with in Guettard's writings.

[1] *Mémoires sur différentes parties des Sciences et Arts*, tome iii. p. 210 (1770).

and of whose extensive natural history collections he became custodian. On the Duke's death he enjoyed from his son and successor a modest pension and a small lodging in the Palais Royal at Paris.

It was to botany that his earlier years of unwearied industry were mainly given. In the course of his botanical wanderings over France and other countries he observed how frequently the distribution of plants is dependent upon the occurrence of certain minerals and rocks. He was led to trace this dependence from one district to another, and thus became more and more interested in what was then termed "mineralogy," until this subject engrossed by far the largest share of his thoughts and labours.

But Guettard was far more than a mineralogist. Although the words "geology" and "geologist" did not come into use for half a century later, his writings show him to have been a geologist in the fullest sense of the word. He confined himself, however, to the duty of assiduous observation, and shunned the temptation to speculate. He studied rocks as well as minerals, and traced their distribution over the surface of Europe. He observed the action of the forces by which the surface of the land is modified, and he produced some memoirs of the deepest interest in physiography. His training in natural history enabled him to recognize and describe the organisms which he found in the rocks, and he thus became one of the founders of palæontological geology. He produced about 200 papers on a wide range of subjects in science, and published some half-dozen quarto volumes of his observations, together with many excellent plates.

It is astonishing that this man, who in his day was one of the most distinguished members of the French Academy of Sciences, and who undoubtedly is entitled to rank among the few great pioneers of modern geology, should have fallen into complete oblivion in English geological literature. I shall have occasion to show that the process of ignoring him began even in his lifetime, and that, though free from the petty vanities of authorship, he was compelled in the end to defend his claim to discoveries that he had made. After his death he was the subject of a kindly and appreciative *éloge* by his friend Condorcet, the perpetual Secretary of the Academy.[1] His work was noticed at length in the great *Encyclopédie Méthodique* of Diderot and D'Alembert, published thirteen years after he was laid in the grave.[2] Cuvier in his *éloge* of Desmarest gave to Guettard the credit of one of his discoveries.[3] But his work seems to have been in large measure lost sight of until in 1862,[4] and again in 1866,[5] the Comte d'Archiac dwelt at some length on his services to the progress of geology. More recently Guettard's labours have been the

[1] *Œuvres de Condorcet*, vol. iii. p. 220.

[2] *Géographie Physique* by Desmarest, forming vol. i. of the *Encyclopédie*, and published An III (1794). The article on Guettard (by Desmarest) gives a critical review of his work, especially of those parts of it which bear on physical geography. The large number and value of his observations on fossil organisms is admitted. But his method of constructing mineralogical maps is severely handled, and his claim to the discovery of the extinct volcanoes of Auvergne is contemptuously rejected. The whole tone of the article is somewhat ungenerous. The imperfections of Guettard's work are fully set forth, but little is said of its merits.

[3] Cuvier's *Éloges Historiques*, vol. ii. p. 354 (1819)

[4] A. D'Archiac, *Cours de Paléontologie Stratigraphique*, pp. 284-304, 1862.

[5] A. D'Archiac, *Géologie et Paléontologie*, 1re partie, pp. 112-118 (1866). The account of Guettard in this work is little more than a con-

theme of sympathetic comment from Ch. Sainte-Claire Deville[1] and Aimé de Soland.[2]

In the geological literature of the English-speaking countries, however, we shall search in vain for any adequate recognition of the place of this early master of the science. That famous classic, Conybeare and Phillip's *Outlines of the Geology of England and Wales,* contains a reference to the French observer as the first man who constructed geological maps. Scrope[3] and Daubeny[4] cite him for his observations in Auvergne. But Lyell in his well-known summary of the progress of geology does not even mention his name.

It is difficult to account for this neglect. Possibly it may be partly attributable to the cumbrous and diffuse style in which Guettard wrote,[5] and to the enormous bulk of his writings. When a man contributes scores of voluminous papers to the transactions of a learned academy; when he publishes, besides, an armful of bulky and closely printed quartos, and when these literary labours are put before the world in by no means an attractive form, perhaps a large share of the blame may be laid to his own

densation of the narrative in the author's previous *Cours.* Even after these appreciative references Lecoq in his *Époques Géologiques de l'Auvergne* omits Guettard's name from the list of those he specially cites, and when he has occasion to mention him, does so in a very grudging spirit. See his Introduction, p. xiii. and vol. iii. p. 155.

[1] *Coup d'œil historique sur la Géologie,* pp. 311-314 (1878).

[2] "Étude sur Guettard," *Annales de la Société Linnéenne de Main-et-Loire,* 13me, 14me, et 15me années, pp. 32-88 (1871, 1872, 1873). This appreciative essay contains a list of Guettard's publications.

[3] *Geology and Extinct Volcanoes of Central France,* p. 30, 2nd edition, 1858.

[4] *Description of Active and Extinct Volcanoes,* p. 729, 2nd edition (1848).

[5] Of this defect no one was more sensible than the author himself. See his *Mémoires sur différentes parties des Sciences et Arts,* tome v. p. 421.

door. Guettard may be said to have buried his reputation under the weight of material which he left to support it.

I cannot pretend to have read through the whole of these ponderous volumes. The leisure of a hard-worked official does not suffice for such a task. But I have perused those memoirs which seemed to me to give the best idea of Guettard's labours, and of the value of his solid contributions to science. And I shall now proceed to give the results of my reading. No one can glance over the kindly *éloge* by Condorcet without a feeling of respect and sympathy for the man who, under many discouragements, and with but slender means, succeeded in achieving so much in such a wide circle of acquirement. And there is thus no little satisfaction in resuscitating among American and English geologists the memory of a man whom I trust that they will recognize as one of the founders of their science, deserving a place not inferior to that of some whom they have long held in honour.

And first with regard to Guettard's labours in the domain of geographical geology, or the distribution of rocks and minerals over the surface of the earth. I have referred to the manner in which he was gradually drawn into this subject by his botanical excursions. As the result of his researches, he communicated in 1746 to the Academy of Sciences in Paris a memoir on the distribution of minerals and rocks.[1] Having been much impressed by the almost entire absence of certain mineral substances in some places, though they were abundant enough in others, he was led to suspect that these substances are

[1] *Mém. Acad. Roy. France*, vol. for 1751.

really disposed with much more regularity than had been previously imagined. He surmised that, instead of being dispersed at random, they are grouped in bands which have a characteristic assemblage of minerals and a determinate trend, so that when once the breadth and direction of one of these bands is known, it will be possible, even where the band passes into an unknown country, to tell beforehand which minerals and rocks should be found along its course.

The first sentences of his remarkable *Mémoire et Carte Mineralogique* are well worth quoting. "If nothing," he remarks, "can contribute more towards the formation of a physical and general theory of the earth than the multiplication of observations among the different kinds of rocks and the fossils which they contain, assuredly nothing can make us more sensible of the utility of such a research than to bring together into one view those various observations by the construction of mineralogical maps. I have travelled with the view of gaining instruction on the first of these two points, and following the recommendation of the Academy, which wished to have my work expressed on a map, I have prepared such a map, which contains a summary of all my observations."

The idea of depicting the distribution of the mineral products of a country upon a map was not original with Guettard or the Academy of Sciences. As far back as the later years of the previous century a scheme of this kind was submitted to the Royal Society of London, and appears in the *Philosophical Transactions* with the quaint title of "An ingenious proposal for a new sort of Maps of

Countrys, together with tables of sands and clays, such chiefly as are found in the north parts of England, drawn up about ten years since, and delivered to the Royal Society, March 12, 1683, by the Learned Martin Lister, M.D."[1]

It may be doubted, however, whether this proposal of Lister's, which does not appear ever to have taken any practical shape, was known to Guettard, who, though he obtained a large amount of information about English mineral products, probably derived it all from French translations of English works. He does not appear to have read English. Guettard inferred, from his observations over the centre and north of France, that the several bands of rocks and minerals which he had detected were disposed round Paris as a centre. The area in the middle, irregularly oval in shape, comprised the districts of sand and gravel, whence he named it the Sandy band. It was there that the sandstones, millstones, hard building stones, limestones, and gun-flints were met with. The second or Marly band, exactly surrounding the first, consisted of little else than hardened marls, with occasional shells and other fossil bodies. The third band, called the "Schitose" [Schistose] or metalliferous, encircled the second, and was distinguished by including all the mines of the different minerals, as well as the pits and quarries for bitumen, slate, sulphur, marble, granite, fossil wood, coal, etc.

Having convinced himself that these conclusions could be sustained by an appeal to the distribution of the minerals in the northern half of France, he proceeded to put upon a map the information he had collected. Using

[1] *Phil. Trans.* vol. xiv. p. 739.

chemical and other symbols, he placed a sign at each locality where a particular mineral substance was known to exist. Moreover, employing a variety of engraved shading, he showed in a general way the position and limits of the great Paris basin. The marly band surrounding the central tract of sandy Tertiary strata was represented as sweeping inland from the coast between Boulogne and Dieppe, through Picardy and the east of France to the Bourbonnais, where, turning westward, it reached Poitou, and then struck northward to the coast west of the mouth of the Seine. Though erroneously grouping Secondary sometimes with Palæozoic, sometimes with Tertiary strata, and not accurately coinciding with the modern divisions of the stratigraphical series, the map yet roughly expresses the broad distribution of the formations.

Having put his data on the map of France, he came to see that his three bands were abruptly truncated by the English Channel and Strait of Dover. Carrying out the principles he had established, he conjectured that these bands would be found to pass under the sea and to re-emerge on the shores of England. To test the truth of this hypothesis, he ransacked the French versions of two once famous English books—Joshua Childrey's *Britannia Baconica*,[1] and Gerard Boate's *Ireland's Naturall Historie*.[2]

1 "*Britannia Baconica*, or the natural rarities of England, Scotland and Wales, according as they are to be found in every shire, historically related according to the precepts of the Lord Bacon." London, 1660. A French translation was published in 1662 and 1667.

2 "*Ireland's Naturall Historie*. Being a true and ample description of its situation, greatness, shape and nature; of its hills, woods, heaths, bogs; of its fruitfull parts and profitable grounds, with the severall ways of manuring and improving the same; with its heads or promontories, harbours, roades and bayes; of its springs and fountains, brookes, rivers,

He found much in these volumes to confirm his surmise. Availing himself of the information afforded by them, he affixed to the map of England the same system of symbols which he had used on that of France, and roughly indicated the limits of his bands across the south-eastern English counties. This portion of his work, however, being founded on second-hand knowledge, is more vague and inaccurate than that which was based on his personal observation in France.

As an example of the painstaking earnestness with which Guettard made his geological notes, it may be mentioned that among the symbols he employed on his map there was one for shells or marine fossil bodies, and that this sign is plentifully sprinkled over the map. His reading enabled him also to insert the symbol on many parts of the map of England all the way from the Wash to Sussex. On the map of France, he was able to introduce an additional sign denoting that the shells were not in mere loose deposits, but formed part of solid stone. In a second map, on a smaller scale, accompanying the same memoir, and embracing the whole of Western Europe from the north of Iceland to the Pyrenees and the Mediterranean, Guettard marked by his system of notation

loghs; of its metalls, minerals, freestone, marble, sea-coal, turf and other things that are taken out of the ground. And lastly of the nature and temperature of its air and season, and what diseases it is free from or subject unto; Conducing to the advancement of navigation, husbandry and other profitable arts and professions. Written by Gerard Boate, late Doctor of Physick to the State in Ireland, and now published by Samuel Hartlib, Esq., for the common good of Ireland, and more especially for the benefit of the Adventurers and Planters there." It was published in London in 1652, and was dedicated to Oliver Cromwell. A French version, under the title of *Histoire Naturelle d'Irlande*, was published at Paris in 1666 (*Dict. Nat. Biog.*, *sub voc.* Boate).

the localities where various metals, minerals and rocks were known to exist. In this way he brought into one view a large amount of information regarding the geographical distribution of the substances which he selected for illustration.

This memoir, with its maps, seems to have gratified the Academy of Sciences, for not merely was it inserted in the volume of Transactions for the year, but in the Journal or annual summary of the more important work of the Academy it occupies a conspicuous place. The official record announced that a new application of geography had been inaugurated by the author, who, neglecting the political limits traced on maps, sought to group the different regions of the earth according to the nature of the substances that lie beneath the surface. "The work of M. Guettard," it is further remarked, "opens up a new field for geographers and naturalists, and forms, so to speak, a link between two sciences which have hitherto been regarded as entirely independent of each other."[1]

I have dwelt at some length on this early work of Guettard because of its importance in the history of geological cartography. These maps, so far as I know, were the first ever constructed to express the superficial distribution of minerals and rocks. The gifted Frenchman who produced them is thus the father of all the national Geological Surveys which have been instituted by the various civilized nations of the Old and the New Worlds.[2]

[1] *Mém. Acad. Roy. Sciences*, 1751 ; *Journal*, p. 105.

[2] So far as I have been able to ascertain, the earliest geological map published with colours to express the several areas of the rocks was that issued at Leipzig in 1778 by J. F. W. Charpentier, Professor in the Mining Academy of Freiberg, to accompany his quarto volume on the *Mineral-*

This early effort at mineralogical map-making was merely the beginning of Guettard's labours in this department of investigation. "If you will only let me have a proper map of France," he used to say, "I will undertake to show on it the mineral formations underneath." When Cassini's map appeared, it enabled him to put his design into execution. After incredible exertions, during which he had the illustrious chemist Lavoisier[1] as an assistant, he completed the mineralogical survey of no fewer than sixteen sheets of the map. These labours involved journeys so frequent and prolonged that it was estimated that he had travelled over some 1600 leagues of French soil. At last, finding the work beyond his strength, he left it to his successor Monnet, by whom the sixteen maps and a large folio of explanatory text were eventually published.[2]

It must be acknowledged, however, that Guettard does not seem to have had any clear ideas of the sequence of formations and of geological structure; at least there is

ogische Geographie der Chursächsischen Lande. Eight tints are used to discriminate granite, gneiss, schist, limestone, gypsum, sandstone, river-sand, clay and loam; and there are also symbols to point out the localities for basalt, serpentine, etc. Palassou, in his *Essai sur la Minéralogie des Monts Pyrénées*, Paris, 1781, gave a series of maps with engraved lines and signs, and also a route-map of the part of France between Paris and the Mediterranean, with the general mineralogical characters of each line of route indicated by strips of colour. He thus distinguished by a green line the granite rocks, by a yellow line the "schists," and by a red line the calcareous rocks. He also indicated the presence of these various formations by different symbols, among which was one for extinct volcanoes, that figures in the Clermont region and also to the west of Montpellier. The early map of Fuchsel (1762) will be subsequently referred to.

[1] See on the subject of Lavoisier's co-operation, D'Archiac's *Paléontologie Stratigraphique*, p. 290, and *postea*, p. 210.

[2] *Atlas et Description Minéralogiques de la France, entrepris par ordre du Roi par MM. Guettard et Monnet*, 1780.

no sign of any acquaintance with these in his maps or memoir. His work, therefore, excellent as it was for the time, contained little in common with the admirable detailed geological maps of the present day, which not only depict the geographical distribution of the various rocks, but express also their relations to each other in point of structure and relative age, and their connection with the existing topography of the ground.

In the course of his journeys, Guettard amassed a far larger amount of detailed information than could be put upon his maps. From time to time he embodied it in voluminous essays upon different regions. The longest and most important of these is one in three parts on the mineralogy of the neighbourhood of Paris, in which, besides giving an account of the distribution of the minerals and rocks, he pays special attention to the organic remains of that interesting tract of country, and figures a large number of shells from what are now known as the Secondary and Tertiary formations.

His natural history predilections led him to take a keen interest in the fossils which he himself collected, or which were sent up to Paris from the country for his examination. He devoted many long and elaborate memoirs to their description, and figured some hundreds of them. I may mention, as of particular interest in palæontological investigation, that Guettard was the first to recognize trilobites in the Silurian slates of Angers. In some specimens which had been sent up to the Academy from the quarries of that district, he observed numerous impressions of organic remains, which he referred to sea-weeds and crustacea. The latter he sagaciously compared to modern crabs and prawns. They are well-marked trilobites, and his figures of them are so excellent that the genera, and even in some cases the species, can easily be made out. His representation of the large *Illænus* of these Lower Silurian slates is specially good. His memoir, read before the Academy in 1757, and published in 1762,[1] is thus a landmark in geological literature, for it appeared eighty years before Murchison's *Silurian System* made known the sequence and abundant organic remains of the Silurian rocks of Wales.

Guettard's labours in palæontology ranged over a wide field. We find him at one time immersed in all the details of fossil sponges and corals. At another, he is busy with the mollusca of the Secondary and Tertiary rocks. Fossil fishes, carnivora, pachyderms, cetacea—all interest him, and find in him an enthusiastic and faithful chronicler. His descriptions are not of the minutely systematic and technical order which has prevailed since the time of Linnaeus. Yet some of his generic names have passed into the language of modern palæontology, and one of the genera of Chalk sponges which he described has been named after him, *Guettardia*. He had within him the spirit of the true naturalist, more intent on understanding the nature and affinities of organic forms than on adding new names to the scientific vocabulary. His descriptions and excellent drawings entitle him to rank as the first great leader of the palæontological school of France.

[1] "Sur les Ardoisières d'Angers," *Trans. Acad. Roy. Sciences*, 1762, p. 52. The Dudley trilobite of the Upper Silurian limestone of England had been figured and described by Lhuyd in his *Lithophylacii Britannici Iconographia* (1699), Epist. i. p. 96 and Pl. xxii.; a figure of it was subsequently given in *Phil. Trans.* 1754, Pl. xi. Fig. 2.

As far back as the year 1751, when he was thirty-six years old, he presented to the Academy a memoir on certain little-known fossil bodies, in which he struck, as it were, the keynote of his future life in regard to the organic remains enclosed within the stony records of former ages. Like a man entering a vast charnel-house, he sees on every side proofs of dead organisms. Others had observed these proofs before him, and had recognized their meaning, and he alludes to the labours of his predecessors. He especially singles out Palissy, who, though known chiefly for eminence as a potter, was the first, some two hundred years before, to embrace fossil shells in his view of Nature, to maintain that those shells were the productions of the sea, not of the earth, as had been supposed, and to demonstrate from them that France once lay beneath the sea, which had left behind it such vast quantities of the remains of the creatures that peopled its waters.

In Normandy, whence many of Guettard's early collections came, and where the people of the country looked upon certain fossil bodies as forms of fruit—pears and apples that had fallen from the trees and taken a solid form within the earth—he tells how half-witted he seemed to them when he expressed a doubt regarding what they believed to be an obvious truth. He recognized the animal nature of the organisms, and asserted that the so-called peaches, apples and pears all belonged to the class of corals, though many of them are now known to be sponges.

Of all his numerous and voluminous essays on palæontological subjects, perhaps that which most signally displays Guettard's modern and philosophical habit of mind in dealing with fossil organisms is a long paper in three parts, which appeared in 1765 under the title, "On the Accidents that have befallen Fossil Shells compared with those which are found to happen to Shells now living in the Sea."[1] In spite of his own and earlier writings, many observers continued to believe that the apparent shells found in the rocks of the land never really belonged to living creatures, but were parts of the original structure of the earth. It is incredible how long this belief lasted, and what an amount of energy had to be expended in killing it. I have been told that even within the present century a learned divine of the University of Oxford used to maintain his opinion that the fossils in the rocks had been purposely placed there by the devil to deceive, mislead and perplex mankind. In Guettard's days another opinion of a contrary tendency was promulgated by a Swiss naturalist, Bertrand, who suggested that the fossil plants and animals had been placed there directly by the Creator, with the design of displaying thereby the harmony of His work, and the agreement of the productions of the sea with those of the land.

It is difficult, perhaps, to imagine ourselves in the position of naturalists about the middle of last century, to whom such opinions seemed perfectly logical, natural and probable. Yet unless we make the effort to realize the attitude of men's minds in those days, we cannot rightly appreciate the acumen and sagacity of the arguments with which Guettard assailed these opinions. In much detail, and with many admirable illustrations drawn from his personal observations all over France, he demon-

[1] *Trans. Acad. Roy. Sciences* (1765), pp. 189, 329, 399.

strated that fossil shells often have attached to them other shells, and likewise barnacles and serpulae; that many of them have been bored into by other organisms, and that in innumerable instances they are found in a fragmentary and worn condition. In all these respects the beds of fossil shells on the land are shown to present the closest possible analogy to the floor of the present sea, so that it becomes impossible to doubt that the accidents which have affected the fossil organisms arose from precisely the same causes as those of exactly the same nature that still befall their successors on the existing ocean bottom.

Of course nowadays such reasoning appears to us so obvious as to involve no great credit to the writer who elaborated it. But we must remember the state of natural knowledge more than one hundred and thirty years ago. As an example of the method of explaining and illustrating the former condition of the earth's surface by what can be seen to happen now, Guettard's memoir is unquestionably one of the most illustrious in the literature of geology, opening up, as it did, a new field in the investigation of the history of our globe, and unfolding the method by which this field had to be cultivated.

On what is now known as Physiographical Geology, or the discussion of the existing topography of the land, this same illustrious Frenchman left the impress of his mind. I will cite only one of his contributions to this subject—a memoir "On the Degradation of Mountains effected in our Time by heavy Rains, Rivers and the Sea."[1] This work, which occupies about 200 quarto pages, deals with

[1] See vol. iii. of his *Mémoires sur différentes parties des Sciences et des Arts*, pp. 209-403.

the efficacy of moving water in altering the face of the land. At the very beginning of it, he starts with a reminiscence from the scenes of his infancy, and weaves it into the story he has to tell of the ceaseless degradation of the terrestrial surface. He remembers a picturesque crag of the Fontainebleau sandstone which, perched above the slopes of a little valley, had been worn by the weather into a rudely-formed female figure holding an infant, and had been named by the peasantry the Rock of the Good Virgin. That crag, under which he used to play with his schoolmates, had in the interval of less than half a century gradually crumbled away, and had been washed down to the foot of the declivity. In the same neighbourhood he had noticed at successive visits that prominent rocks had made their appearance which were not previously visible. They seemed, as it were, to start out of the ground, yet he knew that they arose simply from the removal of the material that once covered them. In like manner ravines of some depth were in the course of a few years cut out of ground where there had before been no trace of them. In these striking examples of the general disintegration he sees only the continual operation of "gentle rains and heavy downpours."[1]

From illustrations supplied by his own earliest observation, he passes on to others drawn either from his personal researches or his reading, and exemplifying the potent influence of heavy rains and flooded streams. Not only are the solid rocks mouldering down and strewing the slopes below with their debris, but the sides of the hills are gashed by torrents, and narrow defiles are cut in them,

[1] "Des pluies et des averses," *op. cit.* p. 210.

like the Devil's Gap in Normandy.[1] He combats the notion that landslips, such as had occurred at Issoire in Auvergne in the year 1733, were caused by internal fires or subterranean winds, and agrees with a previous writer in regarding them as the result of the penetration of water from the surface into the interior of the hill. He thus recognized the efficacy of subterranean as well as superficial water, in changing the face of a country.

He believed the sea to be the most potent destroyer of the land, and as an instance of its power he was accustomed to regard the chalk cliffs of the north-west of France as the relics of a great chain of hills, of which the greater part had been swept away by the sea.[2] He shows, further, that while the hills are worn down by the waves, by the rains, and by the inundations to which the rains give rise, the materials removed from them are not destroyed, but are deposited either on the land or along the shores of the sea.[3] He further points out that the detritus of separate river-basins may greatly differ, and that materials may be carried into districts where the rocks are entirely distinct from those in the areas whence the transport has taken place. He refers to the practical value of this observation in questions regarding the source of minerals, ores and useful stones.[4]

He is thus led to give, from his wide knowledge of France, a sketch of the character of the rocks in the different river-basins of the country, and the nature of the materials which the rivers have in each case to transport. He passes in review all the large streams that enter the Atlantic from the Rhine to the shores of Gascony, and

[1] P. 214. [2] Pp. 220, 222. [3] P. 222. [4] P. 223.

considers, likewise, the Rhone with its tributaries on the Mediterranean side of the watershed.[1] He infers that all the debris derived from the waste of the land is not carried to the sea, but that a great deal of it is deposited along the borders of the streams, and that though it may be removed thence, this removal must require many ages to accomplish. He thinks that the levels of the valleys are at present being raised owing to the deposit of detritus in them.[2] The plains watered by the rivers are one vast sheet of gravel, the streams having changed their courses again and again, so as to flow in turn over every part of these alluvial tracts. The thickness of detritus brought down by the rivers gradually increases towards their mouths. Near their sources, on the other hand, any sediment which is deposited is in a manner superficial, and is liable to continued removal and transportation farther down.

The fragmentary material that is accumulated along the margin of the sea is, in Guettard's view, derived either from what is borne down by rivers, or from what is made by the sea itself, the whole being ground into powder by the long-continued beating of the waves. The sea not only acts on its shores, but on submerged rocks, and the detritus thus produced is mingled with the triturated remains of corals, shells, fish-bones and marine plants.[3]

Comparatively little information had been gathered in Guettard's time as to the condition of the sea-bottom. There is thus a peculiar interest in noting the ideas which he expresses on this subject. He thinks that, besides what is laid down upon the shore, another portion of the detritus

[1] Pp. 225-324. [2] P. 326. [3] P. 328.

is borne away seawards, and gradually settles down on the sea-floor. As the nature of the part so transported must depend on that of the material on the shore, he is led to enter upon a minute examination of the mineral constitution of the coast-lines of France, both on the Atlantic and Mediterranean margins of the country.[1]

He recognizes that soluble substances may be carried for great distances from the land, and may remain dissolved in the sea-water for a very long time. He even conjectures that it is possibly these substances that impart its salinity to sea-water.[2]

From all the soundings available in his day, he concludes that the bottom of the sea is, throughout its whole extent, covered mostly with sand, which is probably not derived from the detritus of rivers.[3] He observes, regarding this widely-diffused deposit, that it might be thought to be due to the grinding down of submarine rocks by the sea itself. But he contends that "how violent soever may be the movements of the sea, they can have but little effect, save on those rocks which emerge above the level of the water, the greatest storms being little felt except on the surface, and for a short way below it." In this sagacious and generally accurate inference, however, he was long before anticipated by Boyle.

Considering, further, the problem presented by the general diffusion of sand over the bed of the sea, he thinks that the erosive influence of the ocean cannot be enough to account for this deposit, which is spread over so vast an area. He concludes, therefore, that the sand must date back to the remote ages of the destruction of the mountains.

[1] P. 328. [2] P. 360. [3] P. 401.

The submarine rocks met with in sounding are, he thinks, unquestionably the remains of mountains formerly destroyed, and the detached boulders similarly discovered are no doubt the result of the destruction of these rocks, or in some cases they may have been derived from neighbouring islands where such exist.[1]

No argument against this view of the high antiquity of the sandy sediment on the sea-floor can, he believes, be drawn from the presence of shells, either singly or in numbers, in this sand. These he regards as obviously the relics of molluscs of the present time, those of former ages having been long ago destroyed.[2]

He remarks, in conclusion, that "it follows, from all the observations here recited, that the deposits laid down by the sea along its shores are sandy and loamy; that these deposits do not extend far out to sea; that, consequently, the elevation of new mountains in the sea by the deposition of sediment is a process very difficult to conceive; that the transport of the sediment as far as the equator is not less improbable; and that still more difficult to accept is the suggestion that the sediment from our continent is carried into the seas of the New World. In short, we are still very little advanced towards the theory of the earth as it now exists. All the systems which have been devised in this subject are full of difficulties which appear to me to be insoluble." He proposes, finally, to return, should the occasion present itself, to these questions, which are "all the more interesting the more difficult they are to elucidate."[3]

It cannot be claimed that such enlightened views

[1] Pp. 401, 402. [2] P. 402. [3] Pp. 402, 403.

regarding the subaerial degradation of the land were now for the first time proclaimed to the world. Guettard had been to some extent preceded by the English naturalist Ray, who, some ninety years before, had briefly alluded to the manifest action of "rains continually washing down and carrying away earth from the mountains," and to the destruction of the shores by the continual working of the sea, and who believed that in the end, by the combination of these processes, the whole dry land might possibly be reduced below the sea-level.[1]

Generelli, too, in his defence of Lazzaro Moro, twenty years before the appearance of Guettard's volume, had, with great eloquence, dwelt on the evidence of the constant degradation of the mountains by running water as an argument for the existence of some other natural cause, whereby, from time to time, land was upraised to compensate for the universal waste. It must be admitted, however, that no one had elaborated the subject so fully until it was taken up by the French observer, and that he was the first to discuss the whole phenomena of denudation, apart altogether from theory, as a great domain for accurate and prolonged observation.

I have reserved for mention in the last place the discovery for which chiefly Guettard's name has received such mention as has been accorded to it in English scientific literature. He was the first to ascertain the existence of a group of old volcanoes in the heart of France. This contribution to the geology of the time may seem in itself

[1] *Miscellaneous Discourses concerning the Dissolution and Changes of the World*, by John Ray, Fellow of the Royal Society, London, 1692, pp. 44-56.

of comparatively small moment, but it proved to be another important onward step made by the same indefatigable and clear-sighted naturalist, and laid the foundations of another department of the natural history of the earth. It proved also to be the starting-point of one of the great scientific controversies of the latter half of the last, and the first decades of the present, century. There is thus a peculiar interest in watching how the discovery was made and worked out by the original observer.

The story goes back to the early months of 1752, for on the 10th of May of that year Guettard read to the Academy a "Memoir on Certain Mountains in France which have once been Volcanoes."[1] He tells how he had undertaken further journeys for the purpose of obtaining additional information towards the correction and amplification of his map of France, showing the distribution of his "bands" with their characteristic minerals. He was accompanied by his former school-fellow and then his valued friend, Malesherbes. On reaching Moulins on the Allier, he was struck by the nature of the black stone employed for mile-posts, and felt certain that it must be of volcanic origin. On inquiring whence the material came, and learning that it was brought from Volvic, "Volvic!" he exclaimed, "Volcani Vicus!" and at once determined to make without delay for this probably volcanic centre.[2] His excitement

[1] *Mém. Acad. Roy. Science*, vol. for 1756, p. 27.

[2] Twenty-eight years after this discovery Guettard found himself forced to defend his claim to be the discoverer of the old volcanoes of Central France, and to ask his friend Malesherbes for his testimony to the justice of that claim. Malesherbes accordingly wrote him a letter giving an account of their journey to Auvergne, which Guettard printed in the preface to his treatise, in two volumes, on the mineralogy of Dauphiné. It

in the chase after an unknown volcano seems to have increased with every step of the journey, as more and more of the dark stone appeared in the buildings by the roadside. At Riom he found the town almost entirely built of the material, which he felt sure he had now run nearly to earth. Learning that the quarries were still some two leagues distant, he pushed on to them, and great was his delight to find all his suspicions amply confirmed. He recognized the rock as a solidified current of lava which had flowed down from the high granitic ridge for some five English miles into the plain below, and he found the actual cone and crater from which the molten flood had issued.

We can follow the enthusiastic explorer with warm sympathy as he eagerly and joyously sees at each onward step some fresh evidence of the true volcanic nature of the rocks around him. Though he had never beheld a volcano, he was familiar with their outlines from the available engravings of the time. Ascending a hill beyond the quarries, he recognizes its conical form as that of a typical volcano.[1] As he climbs the rough slopes, he identifies the crumbling debris of black and red pumice, together with the blocks of rugged spongy slags and scoriae, as manifestly the products of a once active volcanic vent. When he reached the truncated summit of the hill, what must have

is curious that, with the statements of the two travellers long ago in print, Scrope should have published a totally inaccurate version of the journey in the first edition of his *Volcanoes of Central France*, and should have repeated it in the second edition.

[1] Desmarest affirms that it was not the Puy de la Nugère, the source of the Volvic lava, which Guettard ascended, but the Puy de la Banniere, and that the former hill was unknown to him, *Encyclopédie Méthodique, Géographie Physique*, vol. i. p. 187.

been his delight when he saw below him the smooth-sloped hollow of the crater, not now belching forth hot vapours and ashes, but silent and carpeted with grass! For centuries the shepherds had pastured their flocks on these slopes, and the quarrymen had been busy cutting and sending off the lava for roads and buildings, but no one had ever suspected that this quiet and lonely spot retained such striking monuments of subterranean commotion.

Descending to the great lava-stream, Guettard scrutinized its structure as laid open in the quarries, and at once noticed how different in character it was from any other rock he had ever seen in France. He observed it to be divided into sheets inclined with the general slope of the ground, but separated from each other by layers of clay, earth or sand, as in the case of sedimentary formations, yet solid, and breaking easily in any direction, so as to lend itself readily to the arts of the stone-mason.

Travelling southward along the base of the picturesque ridge of the Puys, Guettard and Malesherbes reached Clermont, where they procured the services of an intelligent apothecary, who had some knowledge of the topography of the hills. They climbed the steep slopes of the Puy de Dôme—a hill made famous by Pascal. Everywhere they noticed volcanic debris partially concealed under vegetation. If the view from the first volcano above Volvic delighted the travellers, we can imagine their amazement and pleasure when the marvellous panorama around the highest craterless summit spread itself like a map around them. As their eyes ranged over that array of old volcanoes, so perfect in form that it is difficult to believe them to have been silent ever since the beginning of human history, they could mark the cones rising one behind the other in long procession on the granite ridge, each bearing its cup-shaped crater atop.

In descending from the mountain they came upon another crater, probably that of the Petit Puy de Dôme, a singularly perfect example of the type, some 300 feet deep, and the same in diameter of rim, with such regular and smooth slopes that it has been named by the shepherds the Hen's Nest. Everywhere they encountered quantities of pumice, which so entirely convinced Guettard of the true volcanic nature of the district, that he found it unnecessary for his immediate purpose to examine the rest of the puys. Their Clermont guide, though he had previously wandered over the hills, had never suspected their volcanic origin; but he seems to have learnt his lesson promptly, for he soon afterwards, at Guettard's request, sent some details, and wrote about eruptions and explosions as if he had been long familiar with their effects.

Not only did Guettard detect some sixteen or seventeen cones, but he observed that their craters looked in different directions, and he thought that they probably belonged to different periods of eruption. The travellers pushed on to the great volcanic centre of Mont Dore. But Guettard was there less successful. He was unaware of the influence of long-continued denudation in altering the external forms of volcanic hills, and was disposed to regard his ill success as probably due to the mantle of vegetation by which so much of the ground was concealed.

The journey in Auvergne was too brief and hurried to admit of any single point being fully worked out. But

Guettard believed that he had amassed material enough to prove the main question which interested him—that there had formerly been a series of active volcanoes in the heart of France. So he prepared an account of his observations, and read it to the Academy of Sciences on 10th May 1752.

This early memoir on the extinct volcanoes of Europe must not be tried by the standard which has now been attained in the elucidation of volcanic rocks and the phenomena of ancient eruptions. We should be unjust if we judged it by the fuller knowledge obtained of the same region of France by the more detailed examination of other observers even in Guettard's lifetime. Desmarest, to whose splendid achievements I shall refer in my next lecture, was conspicuously guilty of this injustice. He would never allow Guettard credit for his work in Auvergne, finding fault with it because it was imperfect and inaccurate. He wished that, before writing on the subject at all, his predecessor had studied the ground more carefully and in greater detail, and had attended to the different conditions and dates of the eruptions. "Can we regard as a true discovery," he asks, "the simple recognition of the products of volcanic action, when the facts are presented with so little order and so much confusion? Such a discovery implies a reasoned analysis of all the operations of fire, of which the results have been studied, so as to reveal the ancient conditions of all the volcanic regions. Without this it is impossible to dignify the recognition of a few stones with the name of a discovery that will advance the progress of the natural history of the earth."[1] Could any judgment be more unfair?

[1] *Géographie Physique*, Art. "Guettard."

As if no discovery was entitled to the name unless it had been elaborated in the fullest detail and followed to its remotest consequences. When one of Guettard's countrymen and contemporaries could write thus of his claims to recognition, it is not surprising that for the best part of a century his name should have almost entirely passed out of mind.

That Guettard preceded every one else in the recognition of the old volcanoes of Auvergne, and that he thus became the originator of the Vulcanist party in the famous warfare of the end of last century, in no way diminishes the claim of Desmarest to occupy the foremost place among the Vulcanists and to be ranked as the real founder of volcanic geology. I shall have occasion to dwell at some length on Desmarest's work, which for accuracy and breadth has never been surpassed.

Guettard, having never seen a volcano, was guided in his observations and inferences by what he had read of volcanic countries, and what he had learnt about lavas by familiarity with specimens of these rocks brought from Vesuvius and other modern volcanoes. He noted the close resemblance between the rocks of Auvergne and the Italian lavas, not only in appearance, density and other characters, but in their position on the ground, the specimens which he had gathered from the bottom, sides and crests of the puys having each their own distinctive peculiarities, as in existing volcanoes. He compared the curved lines on some of the rocks of Mont Dore and the Puy de Dôme with the ropy crusts of certain Vesuvian lavas.

When this distinguished man stepped from the observation of fact into the region of theory, he at once fell into error, but the error was general in his time, and was shared in by his most illustrious contemporaries. "For the production of volcanoes," he remarks, "it is enough that there should be within these mountains substances that can burn, such as petroleum, coal or bitumen, and that from some cause these materials should take fire. Thereupon the mountain will become a furnace, and the fire, raging furiously within, will be able to melt and vitrify the most intractable substances."[1] He finds evidence in Auvergne of this presumed connection between the combustion of carbonaceous substances and volcanic eruptions, and he cites in illustration the Puy de Crouel and Puy de la Poix, near Clermont, where the black bituminous material can actually be seen at the surface. Summing up his observations he concludes thus: "I do not believe that the reality of our volcanoes will now be called in question, save perhaps from anxiety for the safety of the districts around them. For myself, confident as to the first point, I confess that I share in the anxiety regarding the second. Hot springs have generally been regarded as due to some kind of concealed volcanoes. Those of Mont Dore rise at the very foot of the mountains; those of Clermont are only some two leagues from the chain of the Puys. It may very well be that their high temperature is kept up by the same internal fires which formerly had a communication with these extinct volcanoes, or might now easily establish one should they increase in activity."[2]

His fears for the safety of the Auvernois were by no

[1] *Trans. Roy. Acad. Sciences* for 1756, p. 52. This suggestion is severely criticized by Desmarest, but it was subsequently adopted by Werner, and became a prominent item in the Wernerian creed.

[2] *Op. cit.* p. 53.

means shared by the people themselves, for they refused to believe that the Puys, which they had known from infancy as quiet, well-behaved hills, had ever been anything else, and they looked upon the learned doctor's descriptions of the former eruptions as mere speculation of his own manufacture.

In taking leave of Guettard's scientific labours, I must refer to one further essay of his, on account of its connection with his work among the old volcanoes of Auvergne. Eighteen years after his memoir on these hills had been read to the Academy, he published a paper "On the Basalt of the Ancients and the Moderns."[1] The furious war over the origin of basalt, of which I shall give some account in another lecture, had not yet definitely begun. Various writers had maintained that this rock is of volcanic origin, and we might have supposed that Guettard's experience in Auvergne would have led him to adopt this correct opinion. So far from doing so, however, he entered into an elaborate discussion to show that basalt could not be a volcanic rock. He admitted that it is found among volcanic masses, but he accounted for its presence there by supposing that in some cases it was already in that position before the eruptions, in others that it had been laid down upon the lavas after they had consolidated. "If a columnar basalt can be produced by a volcano," he asks, "why do we not find it among the recent eruptions of Vesuvius and other active volcanoes?" After reviewing all that had then been written on the subject, he concludes that "basalt is a species of vitrifiable rock, formed by crystallization in an

[1] *Mémoires sur différentes parties des Sciences et Arts,* tome ii. p. 226 (1770).

aqueous fluid, and that there is no reason to regard it as due to igneous fusion."[1]

We may gather how little was then known of the characters of modern lavas when Guettard was ignorant of the occurrence of columnar structure among them.[2] He was as hopelessly wrong in regard to the origin of basalt, as he was with respect to the nature of volcanic action. How this error originated will appear in an examination of the controversy to which basalt gave rise. But the most interesting feature in the passage just cited from Guettard is not his mistake about basalt, but his clear enunciation of his belief in its deposition from aqueous solution, for he thus forestalled Werner in one of the most keenly disputed parts of his geognosy.

I know nothing more whimsical in the history of geology than that the same man should be the parent of two diametrically opposite schools. Guettard's observations in Auvergne practically started the Vulcanist camp, and his promulgated tenets regarding basalt became the watchword of the Neptunists.

The notable Frenchman, of whose work I have now attempted to give an outline, must have been a singular figure as he moved about among his contemporaries. Endowed with a healthy constitution, he had strengthened it by travel, and by a hard and sober life. At last he became liable to attacks of a heavy lethargic sleep, during one of which his foot was burnt. The long and painful healing of the wound he bore with stoical patience, though often convinced of the uselessness of the remedies applied. "I see quite

[1] *Op. cit.* p. 268.

[2] We shall find that this ignorance continued for many years after Guettard's time, and was characteristic of the Wernerian school.

well," he would say, "that they want to ward off the stroke; but they will not succeed." The idea of the kind of death that would terminate his life never left his mind, but did not in the least affect his cheerfulness. He continued to come assiduously to the meetings of the Academy of Sciences alone and on foot, taking only the precaution to carry in his pocket his full address, that in case of anything happening to him, he might be taken home. By degrees he declined to dine with his friends, and then went seldom to see them, quietly assigning as his excuse the fear of troubling them with the sight of his death. He passed away at last on the 7th of January 1786 at the age of seventy-one years.

The kindly *éloge* of Condorcet enables us to form some idea of the character and peculiarities of the man. From his childhood onwards he was eminently religious. His nature was thoroughly frank and honest, simple and unambitious. Scrupulously exact in his own dealings with fact, he hated everything savouring in the least of insincerity and subterfuge. His transparent sincerity gained him friends everywhere; yet he was readily irritated, and had a certain brusqueness of manner, which perhaps detracted from the charm of his character and led to his being sometimes much misunderstood. One of his acquaintances once thanked him for having given a vote in his favour. "You owe me nothing for that," was Guettard's abrupt reply. "If I had not believed that it was right to give it to you, you should not have had it; for I don't like you." Condorcet tells how, when they met at the Academy on the occasion of the delivery of the customary *éloges* of deceased members, Guettard, who looked on all these things as unveracious statements, would say to the perpetual Secretary, "You are going to tell a lot of lies. When it comes to my turn I want only the truth told about me." Condorcet, in sketching the defects as well as the excellences of his friend's character, remarks that in fulfilling his wishes in the strictest sense, he is rendering to Guettard the homage that he himself would most have desired. So little did he try to seem better than he was that his defects might be most prominent to those who only casually met him, while his sterling qualities were known only to his friends. "Those who knew Guettard merely by some brusque answer or other indication of bad temper," his biographer remarks, "would be surprised to learn that this man, so severe in appearance, so hard to please, forced by the circumstances of his position to live alone, had actually adopted the large family of a woman who had been his servant, brought up the children and watched over the smallest details of their education; that he could never see any one in distress without not only coming to his help, but even weeping with him. He bore the same sensibility towards animals also, and expressly forbade that any living creature should be killed for him or at his house. He was a man who, losing control of his words when in bad humour, had quarrelled more than once with each of his friends, yet had always ended by loving them and being loved more than ever by them; who had hurt most of his associates in his disputes with them, but yet had preserved the friendship of several of them, and had never diminished in any one of them the esteem which it was impossible to refuse to his character and his virtues."[1]

Guettard's position in the history of science is that of

[1] Condorcet's *Éloge*, pp. 238, 240.

an indefatigable and accurate observer who, gifted with a keen eye, well-trained powers of investigation, and much originality of mind, opened up new paths in a number of fields which have since been fruitfully cultivated, but who rigidly abstained from theory or speculation. In geology, he deserves to be specially remembered as the first to construct, however imperfectly, geological maps, the first to make known the existence of extinct volcanoes in Central France, and one of the first to see the value of organic remains as geological monuments, and to prepare detailed descriptions and figures of them. To him also are due some of the earliest luminous suggestions on the denudation of the land by the atmospheric and marine agents. "By his minute and laborious researches he did more to advance the true theory of the earth (on which, however, he never allowed himself to hazard a single conjecture) than the philosophers who have racked their brains to devise those brilliant hypotheses, the phantoms of a moment, which the light of truth soon remands into eternal oblivion."[1]

[1] Condorcet's *Éloge*.

LECTURE II

The rise of volcanic geology—Desmarest—Rise of geological travel—Pallas, De Saussure.

THE leading position taken by France in the investigation of the history of the earth was well maintained in the later decades of last century. Geology as a distinct science did not yet exist. The study of rocks and their contents was known as mineralogy, which as a pursuit, often of economic value, had been in vogue for centuries. The idea that beyond the mere variety of its mineral contents, the crust of the earth contained a record of the earth's evolution, for many ages before the advent of man, only very slowly took definite shape. Buffon partly realized it; Guettard had a fuller perception of its nature, though he failed to observe proofs of a long succession of changes earlier than the present condition of the surface.

One of the most valuable parts of Guettard's work was his recognition of the existence of volcanic rocks in regions far removed from any active volcano. We have seen that he was led to this important deduction by a train of observation and inference, and that although he never worked out the subject in detail, the credit of the first discovery, denied to him in his lifetime and after it, must in common fairness be assigned to him.

Central France was the region that furnished Guettard with his proofs of extinct volcanoes. It was the same region that afterwards supplied fuel to the controversy over the origin of basalt which raged with fury for so many years. The story of this old battle is full of interest and instruction. We learn from it how the advance of truth may be impeded by personal authority; how, under guise of the most rigorous induction from fact, the most perverse theories may be supported; how, under the influence of theoretical preconceptions, the obvious meaning and relations of phenomena may be lost sight of, and how, even in the realm of science, dry questions of interpretation may become the source of cruel misrepresentation and personal animosity.

To understand the history of this controversy, we must trace the career of another illustrious Frenchman who, with less opportunity for scientific work than Guettard, less ample qualifications in all departments of natural science, and less promptitude in putting the results of his observations into tangible form, has nevertheless gained for himself an honoured place among the founders of modern geology.

Nicholas Desmarest (1725-1815) was born in humble circumstances at Soulaines, a little town in France between Bar-sur-Aube and Brienne, on 16th September 1725.[1] He was thus exactly ten years younger than Guettard. So pinched were the conditions of his youth that he could hardly read even when fifteen years old. From that time, on the death of his father, better prospects dawned upon him.

[1] The biographical details of this sketch are taken from the well-known eloquent *Éloge* of Desmarest by Cuvier, *Recueil des Éloges Historiques*, edit. 1819, vol. ii. p. 339.

The parish priest urged his guardian to have him educated as far as the slender means left for his sustenance would allow. He was accordingly sent to the college of the Oratorians of Troyes; but the pittance available for his benefit was exhausted by the first few terms of his stay there. He had, however, made such marked progress that his teachers, interested in his career, were glad to continue gratuitously the instruction for which he could no longer pay. At the end of his time with them, they passed him on to their brethren in Paris.

Having made some advance, especially in geometry and physics, he was able to support himself by private teaching and other labours which, however, barely provided the necessaries of life. After some ten years of this drudgery, the studies which had been his occupation and solace, came at last to be the means of opening up a new and noble career to him.

The appearance of Buffon's *Theory of the Earth*, in 1749, had had a powerful influence in France in directing attention to the revolutions through which our globe has passed. Among the results of this influence, a society which had been founded at Amiens by the Duc de Chaulnes, proposed in 1753 a prize for an essay on the question whether England and France had ever been joined together. The subject caught Desmarest's fancy, he made some investigations, sent in an essay and carried off the prize.

Cuvier, in his *Éloge*, remarks on the strong contrast between the way in which Desmarest approached his task and that in which Buffon, who had aroused public attention to these subjects, was accustomed to deal with them. The young aspirant to fame, then twenty-eight years of age, allowed himself no hypothesis or theory. He would not travel beyond the positive facts and the inferences that might be legitimately deduced from them. Dealing with the correspondence between the material forming the opposite cliffs of the two countries (which had already been pointed out by Guettard), and with the form of the bottom of the shallow strait, he passed on to consider the former prevalence in England of many noxious wild animals, which could not have swum across the sea, and which man would certainly have taken care not to introduce. From a review of all the considerations which the subject presented, he drew the inference that a neck of land must once have connected England and France, and that this isthmus was eventually cut through by the strong currents of the North Sea.

This essay, so different in tone from the imaginative discourses of Buffon, attracted the attention of D'Alembert, and led him to seek the acquaintance of its author. The friendship of this great man was itself a fortune, for it meant an introduction into the most learned, intelligent, and influential society of the day. Desmarest was soon actively employed in tasks for which his knowledge and capacity were found to fit him, and thenceforth his struggle with poverty came to an end. Among those who befriended him, the young Duc de la Rochefoucault was especially helpful, taking him on his travels and enabling him to see much of France and Italy.

Shortly after the middle of last century, the Governments of Europe, wearied with ruinous and profitless wars, began to turn their attention towards the improvement of the industries of their peoples. The French Government especially distinguished itself for the enlightened views which it took in this new line of national activity. It sought to spread throughout the kingdom a knowledge of the best processes of manufacture, and to introduce whatever was found to be superior in the methods of foreign countries. Desmarest was employed on this mission from 1757 onwards. At one time he would be sent to investigate the cloth-making processes of the country: at another to study the various methods adopted in different districts in the manufacture of cheese. Besides being deputed to examine into the condition of the industries of different provinces of France, he undertook two journeys to Holland to study the paper-making system of that country. He prepared elaborate reports of the results of his investigations, which were published in the *Mémoires* of the Academie des Sciences, or in the *Encyclopédie Méthodique*. At last in 1788 he was named by the King Inspector-General and director of the manufactures of France.

He continued to hold this office until the time of the Revolution, when his political friends—Trudaine, Malesherbes, La Rochefoucault, and others—perished on the scaffold or by the knife of the assassin. He himself was thrown into prison, and only by a miracle escaped the slaughter of the 2nd September. After the troubles were over, he was once more called to assist the Government of the day with his experience and judgment in all matters connected with the industrial development of the country. It may be said of Desmarest that "for three quarters of a century it was under his eyes, and very often under his influence, that French industry attained so great a development."

Such was his main business in life, and the manner in which he performed it would of itself entitle him to the grateful recollection of his fellow-countrymen. But these occupations did not wholly engross his time or his thoughts. Having early imbibed a taste for scientific investigation, he continued to interest himself in questions that afforded him occupation and solace, even when his fortunes were at the lowest ebb.

"Resuming the rustic habits of his boyhood," says his biographer, "he made his journeys on foot, with a little cheese as all his sustenance. No path seemed impracticable to him, no rock inaccessible. He never sought the country mansions, he did not even halt at the inns. To pass the night on the hard ground in some herdsman's hut, was to him only an amusement. He would talk with quarrymen and miners, with blacksmiths and masons, more readily than with men of science. It was thus that he gained that detailed personal acquaintance with the surface of France with which he enriched his writings."

During these journeyings, he was led into Auvergne in the year 1763, where, eleven years after Guettard's description had been presented to the Academy, he found himself in the same tract of Central France, wandering over the same lava-fields, from Volvic to the heights of Mont Dore. Among the many puzzles reported by the mineralogists of his day, none seems to have excited his interest more than that presented by the black columnar stone which was found in various parts of Europe, and for which Agricola, writing in the middle of the sixteenth century, had revived Pliny's old name of "basalt." The wonderful symmetry, combined with the infinite variety of the pillars, the vast size to which they reached, the colossal cliffs along which they were ranged in admirable regularity, had vividly aroused the curiosity of those who concerned themselves with the nature and origin of minerals and rocks. Desmarest had read all that he could find about this mysterious stone. He cast longing eyes towards the foreign countries where it was developed. In particular, he pictured to himself the marvels of the Giant's Causeway of the north of Ireland, as one of the most remarkable natural monuments of the world, where Nature had traced her operations with a bold hand, but had left the explanation of them still concealed from mortal ken. How fain would he have directed his steps to that distant shore. Little did he dream that the solution of the problems presented by basalt was not to be sought in Ireland, but in the heart of his own country, and that it was reserved for him to find.

Before referring to the steps in Desmarest's progress towards the discovery of the origin of basalt, let me briefly sketch what was known on the subject at the time when he began his researches. Agricola, who, as I have just said, revived the Latin name Basaltes for this dark prismatic rock, mentioned that it was to be seen in different parts of Germany, and in particular that it formed the eminence on which the old castle of Stolpen in Saxony had been built.[1] It was afterwards found to be abundantly distributed, not only in Saxony, but in Silesia, in Cassel, and in the valley of the Rhine above Cologne.[2] In these places it usually

[1] *De Natura Fossilium*, lib. vii. p. 315. Folio, Basel, 1546.

[2] Various authors who had noticed the occurrence of basalt before the publication of his memoir are cited by Desmarest. *Mém. Acad. Roy. Sciences*, vol. for 1774, p. 726 *et seq.*

formed detached eminences, frequently capping hills, and presenting its columns in vertical rows along its edges. There was nothing about it likely in those days to suggest a volcanic origin. The exposures of it usually belonged to a far older geological period than the comparatively recent lava-streams of Auvergne, and in the course of time the cones and craters and scoriae, that no doubt originally marked these sites, had gradually disappeared.

The Giant's Causeway, too, though it displayed on a far more colossal scale the characteristic structure and scenery of basalt, was equally silent in regard to its origin. The marvels of this part of the coast of Ireland had frequently been brought to the notice of the learned, from the latter part of the seventeenth century onward.[1] But here as elsewhere, it was rather the symmetrical structure than the mode of formation that engaged the attention of the older observers. Even as far back as the year 1756, one of these writers pointed out the remarkable resemblance of certain rocks in Nassau and in the district of Trèves and Cologne to the Giant's Causeway, which by that time had become famous.[2]

The western islands of Scotland, which far surpass the Irish coast in the extent and magnificence of their basalt cliffs, were still unknown to the scientific world. The first report about their wonders seems to have reached London in the spring of 1761, when the Bishop of Ossory

[1] See Sir R. B., *Phil. Trans.*, xvii. (1693) p. 708; S. Foley, xviii. (1694) p. 170, with a map and bird's-eye view. T. Molyneux, *Ibid.* p. 181 and xix. (1698) p. 209, with drawings of the columns. R. Pocock, xlv. (1748) p. 124, and xlviii. part i. (1754), with further figures illustrating the jointing of the columns.

[2] A. Trembly, *Phil. Trans.* xlix. (1756) p. 581.

sent to the Royal Society a letter he had received from E. Mendez da Costa telling him that "in Cana Island to the southward of Skye and near the island of Rum the rocks rise into polygon pillars . . . jointed exactly like those of the Giant's Causeway."[1] But it was reserved for Sir Joseph Banks to give the first detailed account of the cliffs of Staffa and Fingal's Cave, which from that time shared with the Giant's Causeway in the renown that drew a yearly increasing number of travellers to these distant shores.[2]

Much had thus been learnt as to the diffusion of basalt in Europe, and many excellent drawings had been published of the remarkable prismatic structure of this rock. But no serious attempt seems to have been made to grapple with the problem of its origin. Some absurd notions had indeed been entertained on this subject. The long regular pillars of basalt, it was gravely suggested, were jointed bamboos of a former period, which had somehow been converted into stone. The similarity of the prisms to those of certain minerals led some mineralogists to regard basalt as a kind of schorl, which had taken its geometrical forms in the process of crystallization. Romé de Lisle is even said to have maintained that each basalt prism ought to have a pyramidal termination, like the schorls and other small crystals of the same nature.[3]

Guettard, as we have seen, drew a distinction between

[1] *Phil. Trans.* lii. (1761) p. 163.

[2] See Pennant's *Tour in Scotland*, 1772, where Banks's narrative is inserted with a number of excellent engravings of the more remarkable features in Staffa.

[3] In the second edition of his *Crystallographie* (1783) he clearly distinguishes between crystallization and basaltic structure. The latter he regards as due to desiccation or cooling, tome i. p. 439.

basalt and lava, and this opinion was general in his time. The basalts of Central and Western Europe were usually found on hill tops, and displayed no cones or craters, or other familiar sign of volcanic action. On the contrary, they were not infrequently found to lie upon, and even to alternate with, undoubted sedimentary strata. They were, therefore, not unnaturally grouped with these strata, and the whole association of rocks was looked upon as having had one common aqueous origin. It was also a prevalent idea that a rock which had been molten must retain obvious traces of that condition in a glassy structure. There was no such conspicuous vitreous element in basalt, so that this rock, it was assumed, could never have been volcanic.[1] As Desmarest afterwards contended, those who made such objections could have but little knowledge of volcanic products.

We may now proceed to trace how the patient and sagacious inspector of French industries made his memorable contribution to geological theory. It was while traversing a part of Auvergne in the year 1763 that he detected for the first time columnar rocks in association with the remains of former volcanoes. On the way from Clermont to the Puy de Dôme, climbing the steep slope that leads up to the plateau of Prudelle, with its isolated outlier of a lava-stream that flowed long before the valley below it had been excavated, he came upon some loose columns of a dark compact stone which had fallen from the edge of the overlying sheet of lava. He found similar columns standing vertically all along the mural front of the lava,

[1] See for instance Wallerius' *Mineralogia* (1773) i. p. 336, replied to by Desmarest, *Mém. Acad. Roy. Sciences* (1774), p. 753.

and observed that they were planted on a bed of scoriæ and burnt soil, beneath which lay the old granite that forms the foundation rock of the region. He noticed still more perfect prisms a little further on, belonging to the same thin cake of dark stone that covered the plain which leads up to the foot of the great central puy.

Every year geological pilgrims now make their way to Auvergne, and wander over its marvellous display of cones, craters and lava-rivers. Each one of them climbs to the plateau of Prudelle, and from its level surface gazes in admiration across the vast fertile plain of the Limagne on the one side, and up to the chain of the puys on the other. Yet how few of them connect that scene with one of the great triumphs of their science, or know that it was there that Desmarest began the observations that directly led to the fierce contest over the origin of basalt.

That cautious observer tells us that amidst the infinite variety of objects around him, he drew no inference from this first occurrence of columns, but that his attention was aroused. He was kept no long time in suspense on the subject. "On the way back from the Puy de Dôme," he tells us, "I followed the thin sheet of black stone and recognised in it the characters of a compact lava. Considering further the thinness of this crust of rock, with its underlying bed of scoriæ, and the way in which it extended from the base of hills that were obviously once volcanoes, and spread out over the granite, I saw in it a true lava-stream which had issued from one of the neighbouring volcanoes. With this idea in my mind, I traced out the limits of the lava, and found again everywhere in its thickness the faces and angles of the columns, and on the top their cross-section, quite distinct from each other. I was thus led to believe that prismatic basalt belonged to the class of volcanic products, and that its constant and regular form was the result of its ancient state of fusion. I only thought then of multiplying my observations, with the view of establishing the true nature of the phenomenon and its conformity with what is to be found in Antrim—a conformity which would involve other points of resemblance."

He narrates the course of his discoveries as he journeyed into the Mont Dore, detecting in many places fresh confirmation of the conclusion he had formed. But not only did he convince himself that the prismatic basalts of Auvergne were old lava-streams, he carried his induction much further and felt assured that the Irish basalts must also have had a volcanic origin. "I could not doubt," he says, "after these varied and repeated observations that the groups of prismatic columns in Auvergne belonged to the same conformation as those of Antrim, and that the constant and regular form of the columns must have resulted from the same cause in both regions. What convinced me of the truth of this opinion was the examination of the material constituting the Auvergne columns with that from the Giant's Causeway, which I found to agree in texture, colour and hardness, and further, the sight of two engravings of the Irish locality which at once recalled the scenery of parts of Mont Dore. I draw from this recognized resemblance and the facts that establish it a deduction which appears to be justified by the strength of the analogy—namely, that in the Giant's Causeway, and in all the prismatic masses which present themselves along the cliffs of the Irish coast, in short even among the truncated summits of the interior, we see the operations of one or more volcanoes which are extinct, like those of Auvergne. Further, I am fully persuaded that in general these groups of polygonal columns are an infallible proof of an old volcano, wherever the stone composing them has a compact texture, spangled with brilliant points, and a black or grey tint."

Here, then, was a bold advance in theoretical as well as observational geology. Not only was the discovery of Guettard confirmed, that there had once been active volcanoes in the heart of France, but materials were obtained for explaining the origin of certain enigmatical rocks which, though they had been found over a large part of Europe, had hitherto remained a puzzle to mineralogists. This explanation, if it were confirmed, would show how widely volcanic action prevailed over countries wherein no sign of an eruption has been witnessed since the earliest ages of human history.

Desmarest was in no hurry to publish his discovery. Unlike some modern geologists, who rush in hot haste into print, and overload the literature of the science with narratives of rapid and imperfect observations, he kept his material beside him, revolving the subject in his mind, and seeking all the information that he could bring to bear upon it. He tells us that in the year following his journey in Auvergne, he spent the winter in Paris, and while there, laid before the Intendant of Auvergne the desirability of having the volcanic region mapped. His proposition was accepted, and Pasumot, one of the state surveyors, was entrusted with the task of making a topographical map of the region from Volvic to beyond Mont Dore. The whole of the summer of 1764 was taken up with this work. Desmarest accompanied the geographer, who himself had a large acquaintance with the mineralogy of his day. The result was the production of a map which far surpassed anything of the kind that had before been attempted, in the accuracy, variety, and clearness of its delineations of volcanic phenomena.

At last, in the summer of 1765, after two years of reflection, he communicated to the Academy of Sciences at Paris the results at which he had arrived. But even then he showed his earnest desire for the utmost accuracy and fulness attainable. He kept back his paper from publication. Next year he returned to Auvergne, after a prolonged journey through the volcanic regions of Italy, from the Vicentin and Padua southwards to Naples and Vesuvius. In 1769 he once more revisited the volcanoes of Central France, extending his excursions into the Cantal. In the early part of the summer of 1771 he again brought before the Academy the results of his researches on the origin and nature of basalt, embodying in his Memoir the mass of material which his extended travel and mature reflection had enabled him to bring together. But it was not until three years later, viz., in 1774, that his long-delayed essay at last appeared in the annual volume of the *Memoirs of the Academy.* Life was more placid in those days than it has since become. The feverish haste to be famous, and the frantic struggle for priority, which are now unhappily so rampant, were but little known in Desmarest's days. He kept his work eleven years beside him, enriching it continually with fresh observations drawn from extended journeys, and thus making his conclusions rest on an ever-widening basis of accurately determined fact.

The Memoir, as finally published, was divided into three parts, two of which appeared together, the third not until three years later. In the first part, the author narrated his observations in Auvergne and other districts, bearing on the nature of basalt. Time would fail us were we to try to follow him in his survey of the regions where he found the evidence which he brought forward. Let me refer merely to the concluding pages, in which he states his opinion as to the origin of the columnar rock which he had tracked with such diligence from district to district. His account, he remarks, would be incomplete if he did not indicate at the same time the materials which have been melted by the fire in order to produce basalt. He had collected a series of specimens of granite which he believed to represent these materials. They had undergone different degrees of alteration, some showing still their fusible spar, quartz or other minerals, while others had partly undergone complete fusion. He had convinced himself that various other volcanic rocks besides basalt had resulted from the fusion of granite, the base of which may have been completely melted, while the quartz of the original rock remained unchanged. He was not aware that the difference of chemical composition demonstrates that the melting of granite could never have produced basalt.

These ideas, which we now know to be erroneous, might readily occur to the early observers. It is undoubtedly true that pieces of more or less completely melted granite are to be found among the ejections of old volcanoes, and the inference would naturally suggest itself that the fires kindled by the combustion of carbonaceous substances underneath a volcano might fuse the surrounding and overlying rocks, and expel streams of molten material. We shall find that Werner adopted this view, and that through him it became predominant over Europe even after more enlightened conceptions of the subject had been announced.

Desmarest does not seem to have had at this time, if ever, any very definite conception of the origin of the high temperature within volcanic reservoirs. Nor had chemistry yet afforded much assistance in ascertaining the resemblances and differences among rocks and minerals. His mistakes were thus a faithful reflex of the limited knowledge of the period in which he wrote.

In the second part of his Memoir, Desmarest gives a historical narrative of all that had been written before his time on the subject of basalt. The most interesting and important passages in this retrospect are the comments of the author on the writings he summarises, and the additions which he is thereby enabled to make to the observations already given by him. He confesses that, had he begun his investigations among such isolated patches of basalt as those capping the hills in Cassel and Saxony, he would never have been able to affirm that basalt is only a lava. But he had encountered such perfect demonstration of the volcanic nature of the rock, tracing it with its fresh scoriæ up to the very craters whence it flowed, that he could not allow this clear evidence to be invalidated, or even weakened, by cases where the volcanic origin had been more or less obscured.

It is at this point in his investigation that the genius of Desmarest shines with a brilliance far above that of any of his contemporaries who concerned themselves with geological problems. Guettard had clearly indicated the volcanic origin of the puys of Auvergne, and no great acumen was needed to follow up the clue which he had thus given. But to trace a pathway through the maze of lavas of many different ages, to unite and connect them all in one method of interpretation, and thus to remove the endless difficulties and harmonise the many apparent contradictions which beset the investigation, was a task which called forth the highest powers of observation and induction. Among the many claims of France to the respect and gratitude of all students of geology, there is assuredly none that ought to be more frankly recognized than that, in her wide and fair domain, she possessed a region where the phenomena were displayed in unrivalled perfection, and that in Desmarest she could boast a son gifted with the skill, patience, imagination, and originality that qualified him so admirably for the laborious task which he undertook. His achievements form one of the most notable landmarks in the early history of geology.

Desmarest, wandering over the volcanic districts of Central France, had been profoundly impressed, as every traveller must be, by the extraordinary varieties in the condition of the various lava-currents. Some of these sheets of rock retain still the dark, verdureless, rugged surfaces which they assumed ages ago when their molten floods stiffened into stone. Others have lost their covering of scoriæ, and are seen clinging to the sides of valleys, in positions which seem impossible for any lava-current to have taken. Others are perched in solitary outlying sheets on the tops of plateaux, with no cone near them, nor any obvious source from which they could have flowed.

Pondering on these apparently contradictory phenomena, Desmarest, with the inspiration of true genius, seized on the fruitful principle that would alone explain them. He saw that the varying conditions of the several lavas were due to the ceaseless influence of atmospheric denudation. He convinced himself that the detached outliers of basalt, capping the ridges and plateaux, are really remnants of once continuous sheets of lava, and that their isolation, together with the removal of their original covering of scoriæ and slags, is to be ascribed to the operations of rain and melted snow. The depth of the valleys cut through these lava-platforms was found by him to be commensurate with the antiquity of the lavas, and with the size of the streams that flowed between the severed escarpments.

He ascertained that, in proportion to their antiquity, the lava-streams had lost, one after another, the usual outward features of the younger sheets. The superficial scoriæ had disappeared, and the craters were worn away, until only scattered outliers of compact dark rock remained. Yet between this extreme and that of the most recent eruptions, where the lavas, in unbroken, rugged, cavernous sheets, extend from their craters down into the present valleys, where they have driven aside the running streams, every intermediate stage could be found.

Thus the doctrine of the origin of valleys by the erosive action of the streams which flow in them, though it has been credited to various writers,[1] was first clearly taught from actual concrete examples by Desmarest. The first attempt to trace back the history of a landscape, to show its successive phases, and to connect them all with the continuous operation of the same causes which are still producing like effects, was made by this illustrious native of France.

So satisfied was Desmarest with the proofs furnished by Auvergne regarding the volcanic origin of basalt, that he coined the term "basalt-lava," with an apology to the mineralogists, and remarked that when once the characters of this rock have been appreciated, it may be recognized everywhere, in spite of the most stupendous degradation. Casting his eye over the map of Europe, and noting the localities from which the occurrence of basalt had been reported, he saw two great regions of ancient volcanic activity in the heart of the continent. One of these lay to the east, along the confines of Saxony and Bohemia into Silesia, from Freiberg to Lignitz; the other stretched from the Rhine above Cologne, through Nassau, Hesse-Darmstadt, and Cassel.

The map which has been already referred to as accompanying this remarkable memoir, depicts with great clearness the grouping of the volcanoes over a large part of Auvergne. It represents them by distinct kinds of engraving, so as to show four classes differing from each

[1] Thus by Lyell and Murchison it was ascribed to Saussure, Playfair, and Montlosier, *Edin. New Phil. Journ.* vol. vii. (1829), p. 15. In England it has been more commonly assigned to Hutton and Playfair, and to Scrope.

other in age and other characters. The first of these classes includes the younger lava-streams, not yet cut through by running water, and still connected with their parent cones. The second embraces those lavas which bear decomposed earthy materials on their surface, and from which their original craters have disappeared. In the third class are ranged those lavas which have been reduced to detached outliers separated by valleys; while in the fourth, some isolated masses are placed which Desmarest thought had been "melted in place," or erupted where they now appear.

The third part of the memoir, though read with the second part in 1771, was not published until 1777. In this essay the author discussed the basalt of the ancients, and the natural history of the various kinds of stones to which at different times the term basalt had been applied.

It is interesting to follow the slow elaboration of his views through his successive memoirs. We must remember that, during these busy years, his time and thoughts were chiefly taken up with the inquiries into industrial development which the Government of the day had entrusted to him, and which necessitated frequent and prolonged journeys, not only in France, but in other countries of Europe. He felt that the great questions in physical geography which specially occupied his attention could best be studied in Auvergne. He returned to that region at every available opportunity, revisiting again and again localities already familiar to him, and testing his deductions by fresh appeals to nature. Four years after his great monograph on the origin of basalt had been read to the Academy of Sciences, he presented another essay, developing still further the ideas of denudation and successive eruptive periods which had been briefly sketched in his first communication. The scope of this new effort may be judged of from its full title : "On the Determination of Three Epochs of Nature from the Products of Volcanoes, and on the Use that may be made of these Epochs in the Study of Volcanoes." This essay was laid before the Academy in the year 1775. An extract from it appeared after the lapse of four years,[1] but the full paper was not published until the year 1806[2]—no less than thirty-one years after its original preparation. During this long interval the controversy about the origin of basalt had extended over most of the countries of Europe, and had involved the very subjects of which Desmarest treated. He himself, keenly as the matters in dispute interested him, took no part in the warfare. In his memoir he ignores the combatants and their strife, but quietly repeats and strengthens statements which he had published a generation before, and which, had they been properly considered and verified, would have prevented any controversy from ever arising. I shall have more to say about this dispute in my next lecture. In the meantime let us consider the character of Desmarest's long-delayed contribution to the literature and theory of geology.

The progress of his investigations had led him to perceive the necessity of correlating the various phenomena connected with ancient volcanoes, and especially with reference to the questions of their relative age and of the alterations they have undergone from exposure to

[1] *Journal de Physique*, tome xiii. (1779), p. 115.

[2] *Mém. de l'Instit. des Sciences Math. et Phys.* tome vi. (1806), p. 219. It was read again on 1st Prairial, An XII (20th May 1804).

the elements. The facts known to him suggested an arrangement of them into three groups or epochs, which were not meant to imply definite periods of time or precise dates, but would express the idea of a recognizable succession of events. His researches had assured him that the volcanic history of Auvergne "formed a whole, which, though incomplete, showed that Nature had followed the same order of procedure in the most remote ages as in the most recent times."

In co-ordinating the appearances presented by the different volcanic masses, he began with the consideration of what were obviously the youngest, on the principle that the last operations of Nature are simpler, and have undergone less modification from the influences which are continually changing the face of the land. He perceived that volcanoes are only temporary accidents in the midst of the ordinary and normal operations of nature, that the materials erupted by volcanoes, at various intervals from a remote antiquity, must have suffered from the universal degradation, and that the extent of their waste would be proportionate to the length of time during which the loss had been continued. The latest lavas must unquestionably present most nearly the primitive forms of volcanic masses, and should thus serve as a standard for comparison, to be kept before the eyes of every observer who would judge correctly of the extent and progress of the alteration that is to be seen in other regions.

The first of his three periods includes the products of still active and recently extinct volcanoes. These are distinguished by the association of crater-bearing cones of cinders and scoriæ, with streams of rugged lava, which can be followed from the cones into the surrounding country over which they have flowed. The most modern lava-streams are not cut through by valleys, but form continuous sheets. Yet within the limits of this first epoch proofs of alteration manifest themselves. The loose scoriæ and cinders are washed down to lower levels, the cones are attacked and the lavas begin to be trenched. As these changes advance, the flow of running water gradually cuts through the sheets of lava and forms valleys across them. The epoch embraced all the ages required for this erosion, and during its continuance repeated outflows of lava took place. Each of these currents of melted rock would seek the lowest levels, and would thus mark the valley-bottom of its time, in the long process of excavation.

In the records of the second epoch, the scoriæ and ashes have been swept away, the cones have entirely disappeared, and the streams of lava have been cut into separate patches by the erosion of the valleys, above which they are now left perched as high plains or plateaux. Notwithstanding the stupendous results thus achieved, Desmarest seeks no vast terrestrial disturbance to account for them. He finds their explanation in the working of the very same meteoric agents which are still carrying on the same process of degradation. The cellular parts of the lavas, under the influence of the weather, crumble down into mere loose earth, which is easily washed away by rain and melted snow, leaving only the harder and more resisting core of more solid rock. In like manner, the loose materials of the cones are removed, until perhaps only masses of lava remain behind that may have solidified at their bottoms. By this series of operations an entire transformation is wrought on the face of the country. Lavas which originally covered the floors of valleys, as the ground around them is lowered, are at last turned into high tablelands, and are still further cut through and separated into detached portions, according to the multiplication and deepening of the ravines and valleys by which they are traversed. To realize the ancient continuity of these venerable lava-sheets, we must in imagination fill up the valleys, and thus restore the plain over which the molten rock originally flowed.

As all the scoriæ and craters are gone, the only way of detecting an eruptive centre in the volcanic products of this epoch is to find the point of common origin for several streams, such points being often marked by large isolated patches of lava (culots).

Desmarest arrives at the important conclusion that the lavas of his second epoch were erupted before the excavation of the present valleys out of the original plain over which the streams of basalt were poured. The volcanic events of which they are the memorials must thus go back to a remote antiquity, for the erosion of valleys is obviously an exceedingly slow process. But these lavas are evidently much younger than the horizontal sedimentary strata and the granite which they overlie, both of which are also trenched by the valleys.

The third and most ancient epoch is denoted by a series of lavas, which, instead of overlying the sedimentary strata, underlie them or are interstratified with them. These sediments are now recognized as the deposits of one of the old Tertiary lakes of Europe. Their layers are full of land-plants, land and fresh-water shells, and remains of terrestrial mammals. But to Desmarest they were proofs of the former presence of the sea over the heart of France. He inferred that the pebbles of various lavas which he found among these strata denoted former volcanic eruptions, before the accumulation of the marine deposits. But he noticed also indications of the discharge of lava during the sojourn of the sea over this region. He believed that his third epoch must have lasted some considerable time, so as to permit the deposition of 600 or 900 feet of horizontal sediments above the lowest lavas.[1]

He remarks that from ignorance of this method of following the sequence of eruptions and the effects of continuous waste, naturalists had failed to detect the existence of lavas of the second and third epochs in districts where eruptions of the first epoch were no longer to be recognized. These observers, he contended, had misread the evidence of nature, referring what were undoubtedly volcanic rocks to deposition from water, to schists, and to *pierre de corne*, and on the other hand mistaking for volcanic craters what were only hollows dug out by running water in the lavas of the second, or even of the first epoch.

The sagacity of these generalizations has been amply sustained by the researches of later times. Alike in volcanic geology and in the doctrines of denudation, the labours of Desmarest marked the rise of a new era in the

[1] In the article "Auvergne" in his *Géographie Physique*, p. 882 (published in 1803), he briefly summarises his three epochs thus—"I have distinguished three kinds of volcanoes in Auvergne, first, ancient volcanoes; second, modern volcanoes; and third, submarine volcanoes." Probably most of the lavas of his third epoch are rather of the nature of intrusive sills.

investigation of the past history of the earth. They showed how patient detailed research could solve some of the most transcendently interesting problems in geology, and how the minute and philosophical investigation of one small area of the globe could furnish principles of universal application.

In one respect, perhaps, this far-seeing observer seems to have been almost afraid to push his views of denudation to their logical conclusion. There occur in Central France many flat, isolated areas of basalt capping detached hills and fragments of plateaux, not apparently connected with any visible lava-current or centre of eruption. These patches were called by him culots, and he explained their origin by supposing them to mark the positions of volcanic vents up which the melted material had risen without flowing out, and where it had solidified within the crater, being retained by the encircling wall of scoriæ and cinders. The removal of the surrounding loose material would, he thought, leave the lava as a cake with steep scarped sides crowning the slopes below. Possibly some of his culots originated in the way supposed, but there can be little doubt that most of them are remnants of lava-streams reduced to almost the last stage by the progress of denudation.

From the long intervals which he allowed to elapse between the presentation of his papers to the Academy and their final publication, it might be supposed that Desmarest was probably of a procrastinating, possibly even of an indolent, temperament. Yet, when we consider the amount of work, official and scientific, which he accomplished, we must acquit him of such an imputation.

His voluminous reports on the various industries of France show how actively and zealously he laboured in his official harness. But perhaps the best proof of his indefatigable industry was his colossal *Géographie Physique*, which he undertook as part of the famous *Encyclopédie Méthodique* founded by Diderot and D'Alembert. The exhaustive treatment of his subject may be inferred from the fact that after devoting to it four massive quarto volumes of from 700 to 900 pages each, he had only got to the letter N when death closed his labours.

The first volume of this great work is in many respects the most interesting. The author in his preface tells how he means to exclude from his task all discussion of theories of the earth, for, as he frankly confesses, he had long looked upon these theories as utterly opposed to the principles of Physical Geography. But on second thoughts, as unfortunately such theories really existed, having much the same relation to Physical Geography that fable bears to history, he had resolved to give a summary of the subject, thus conforming to the practice of some writers who begin their histories with a brief mention of the heroic times.[1] Accordingly he devotes the first volume to notices of the more important authors who had treated of his subject, excluding those who were still alive. He made, however, exceptions to this exclusion in favour of Pallas and Hutton. Though he undertook to present merely an impartial summary of the opinions of other writers, it is instructive to have these summaries from the hand of a man like Desmarest, who was contemporary with many of those of whom he discourses. The inter-

[1] *Géographie Physique*, vol. i. (1794), preface.

spersed comment and criticism in his notices are specially valuable.

The other three volumes were devoted to descriptions of places, districts, and countries, and to articles or subjects in Physical Geography—a branch of knowledge which Desmarest regarded as embracing two equally important and closely related subjects—the interior structure of the globe and its external form. Geology was not yet admitted to a formal place among the sciences, but geological questions occupy a prominent place in the massive quartos of the *Encyclopédie Méthodique*.[1]

The delays that attended the publication of Desmarest's important and original observations and deductions respecting the volcanic geology of Auvergne reached their climax in the case of his detailed map of that region. We have seen that at his instigation a topographical survey of Auvergne on a large scale was begun as far back as 1764, and that reductions of this map accompanied his Memoirs presented to the Academy of Sciences. The map itself, however, with all its elaborate detail, bearing on the history of the volcanoes of Central France, still remained in his hands. Year after year he sought to bring it nearer to his ideal of perfection. Every part of the region had been scrupulously examined by him, every puy was set down, every crater was carefully drawn, every current of lava was traced out from its source to its termination,

[1] Vol. i. of the *Géographie Physique* appeared in An III (1794); vol. ii. in 1803; vol. iii. in 1809, and vol. iv. in 1811. Among the geological articles of interest in these volumes reference may be made to those on Antrim, Auvergne, Basalte, Chaussée des Géans, and Courans. Vol. v., left unfinished by Desmarest, was continued by Bory de St. Vincent, Doin, Ferry, and Huot, and was not published until 1828.

every detached area of basalt was faithfully represented. By a system of hachures and signs the modern and ancient lavas were discriminated. But he still kept the work back, and when he died it remained unpublished.

Of all his contributions to the progress of geology, this map must be considered the most memorable. It was the compendium of all his toil in Auvergne, and showed, as in a model, the structure of the country which he had so patiently and successfully elucidated. The reduced map published in his first Memoir and the portions of the map issued with his second Memoir, were all that he allowed to appear in his lifetime, but they failed to impress the minds of his contemporaries, as the entire map would have done, with its complete and clear delineation of the whole district. Labouring after a perfection which he could not attain, he not only lost the credit which the map would have brought him in his lifetime, but he retarded the progress of the sound views which he himself held and wished to see prevail. Had this truly admirable map been published by him, together with a general description of the volcanoes depicted on it, his name would have been placed at once and by universal assent at the head of the geologists of his day, and the miserable controversy about the nature of basalt would either never have arisen, or could have been speedily set at rest. Cuvier tells us that Desmarest himself was fully conscious of the desirability of publishing the map, but his life slipped away as he still aimed at further improvement of it. Yet he could not bear that other observers should enter his volcanic region and describe its features. It used to be said that he seemed to look on Auvergne as his own property, and certainly he was the legitimate owner of most of the observations made there after him.

Cuvier, who knew him well and who had watched with interest his declining years, gives us a vivid picture of Desmarest. The illustrious geologist was little fitted to push his way in a society where the most successful art was that of self-advertisement. He took no more pains about his private interest than he did about his rights in regard to scientific discovery, importuning neither the dispensers of fortune nor those of fame. With his crust and his cheese, he said, he needed no Government help to visit the manufactories or the mountains. In short, in studying all the processes of art, all the forces of nature, he had entirely neglected those arts that sway the world, because nothing which agitates the world could move him. Even works of wit and imagination remained unknown to him, because they did not lie within the range of his studies. His friends used jocularly to affirm that he would have broken the most beautiful statue in order to ascertain the nature of an antique stone, and this character was so widely given to him that at Rome the keepers of the museums felt some alarm in admitting him. In society, too, things, whatever they might be, affected him on one side only. For instance, when an Englishman was recounting at the house of the Duchesse d'Anville the then recent thrilling incident in Cook's first voyage, when his vessel, pierced by a point of rock, was only saved from sinking by the stone breaking off and remaining fixed in the hole, every one present expressed in his own way the interest he felt in the story. Desmarest, however, quietly inquired whether the rock was basaltic or calcareous.

A character so little affected by external things was naturally immovable in regard to relations and habits. From the earliest days when he began to be known, he had been engaged to pass his Sundays at Auteuil with a friend. Ever afterwards he would appear there on the usual day, even when his friend was dead, and when age no longer allowed him to enjoy the country; and as he had from the first gone on foot, he always went there on foot until he was eighty-five years old. All that his family could then prevail upon him to do was to take a carriage.

Nor was he less constant in more trivial affairs. Never did he dine or go to bed later one day than another. Nobody remembered ever to have seen him change the cut of his clothes, and down to his last days his wig and his coat recalled the fashions in vogue under the Cardinal de Fleury.

After recalling his kindliness and helpfulness to poor inventors, for whom he ever evinced the heartiest sympathy, his biographer concludes in eloquent words, with which I may fitly close this sketch of Desmarest's career. "The Academy of Sciences saw in him, as it were, the monument of a bygone age, one of those old philosophers, now too few, who occupied only with science, did not waste themselves in the ambitions of the world, nor in rambling through too wide a range of study, men more envied than imitated, who have supplied us with that succession of octogenarians and nonagenarians, of which our history is full. Living like these worthies, Desmarest fulfilled a similar career, and reached without infirmities or any grave malady the age of ninety years. He died on the 20th September 1815.

"During his protracted lifetime he saw the Academy twice renewed. Among so large a number of colleagues he doubtless recognised that there were many who equalled or even surpassed him in enlightenment or in mental power, but he had the happiness to be assured that his name would last as long as that of any one among them."

For the sake of continuity in the narrative, I have traced the labours of Desmarest from their beginning to their close without adverting to those of his contemporaries. His views regarding the volcanic origin of basalt were adopted by a number of good observers, among whom reference may be made to Raspe, Fortis, Dolomieu, Faujas de St. Fond, Montlosier, and Breislak. But a still more numerous and more blatant band, urged on its way by Werner, opposed these doctrines. Although the controversy raged through Desmarest's life, he took, as I have said, no share in it. He made an occasional allusion to the disorder and confusion that had been introduced into a question which in itself was simple enough to those who knew how to look at the actual facts. He asked reproachfully what would become of natural history and mineralogy, if every question were treated as that concerning basalt had been? And he wrote somewhat scornfully of the authors who, without having ever undertaken any researches of the kind themselves, ventured in discussing those of others to indulge in unfounded hypotheses.[1] When any belated straggler from the enemy's camp came to consult Desmarest on the subject in dispute, the old man would content himself with the answer, "Go and see."

Leaving this controversy for consideration in the next

[1] See the article "Basalte" in vol. iii. of the *Géographie Physique*, published 1809.

lecture, I will pass from the subject for the present, for the purpose of calling attention to one of the most interesting features of the scientific life of the closing decades of last century—the rise of the spirit of scientific travel.

Of all the physical events that happened in the latter half of the eighteenth century, there was probably none so fruitful in fostering, among the civilized countries of the world, an emulation in discovery and research, as the transit of Venus, which occurred in the summer of 1769. To that event we owe the voyages of Cook, and all the rich harvest of results which they added to our knowledge of the geography of the globe. What England did on the ocean, it was reserved for Russia to rival on the land. The Empress Catherine II. had been irritated by the sarcastic remarks made by a French astronomer who had travelled to Russia to observe the previous transit of Venus in 1763, and she is even said to have been at the trouble of refuting them herself. At all events, she resolved to do without foreign assistance for the second transit. Determined that the work should be done thoroughly, and in such a way as to redound to the glory of her reign, she commissioned the Academy of Sciences of St. Petersburg to organize the expedition. This undertaking was conceived in a truly imperial spirit. Not only were astronomers sent out for the more immediate objects of the research, but advantage was taken of the occasion to despatch a competent band of observers for the purpose of penetrating into every region of the vast empire, and making known its condition and resources.

The instructions drawn up for the guidance of the explorers were of the most exhaustive kind. Accurate observations were to be made in the geography and meteorology of each region visited, the positions of the principal places were to be astronomically determined, the nature of the soils, the character of the waters, and the best means of reclaiming the waste places were to be accurately observed. The travellers were to inquire into the rocks and minerals, and to attend to the outer forms and internal composition of the mountains. They were further to carry on careful researches among the plants and animals of each territory, and, in short, to obtain as much accurate information as possible in every department of natural history. Nor were the social problems of life forgotten. The expedition was further instructed to pay special attention to the various races of mankind met with in the journeys, and to report on their manners, customs, religions, forms of worship, languages, traditions, monuments and antiquities. They were likewise enjoined to take note of the condition of agriculture, of the maladies that affected man and beast, and the best remedies for them, of the cultivation of bees and silk-worms, the breeding of cattle and sheep, and generally of the occupations, arts, and industries of each province.

A survey of this complete nature, carried over so vast a region as the Russian Empire, demanded much skill, labour and time. It was fortunately entrusted to a man in every way qualified for the task—Pierre Simon Pallas (1741-1811). The whole expedition comprised seven astronomers and geometers, five naturalists and several assistants. Starting from St. Petersburg in June 1768, they traversed the vast empire to its remotest bounds, making many journeys in every direction. After six years of unwearied labour, and almost incredible suffering and privation, during which Pallas had from time to time sent home accounts of his more important observations, he returned in July 1774.

Never before had so large a store of observations in all departments of natural history, extending over so wide a region of the earth's surface, been gathered in so brief a time. Pallas wrote his results in German (his native language, for he was born at Berlin in 1741), and sent them home as they were ready. They were published at St. Petersburg between 1772 and 1776, in three quarto volumes. They were afterwards translated into French, and appeared at Paris during the years from 1788 to 1793,[1] in five handsome quartos, with a folio atlas of plates.

Pallas was an accomplished naturalist, and made some original and valuable contributions to zoology. But it is only with his geological work that we are here concerned. One of the geological questions which especially interested him was the occurrence of the remains of huge pachyderms in the superficial deposits of the north of Siberia. These remains, as far back as the latter years of the seventeenth century, had been known to exist, for a trade in the ivory tusks of fossil elephants from the Siberian coasts and rivers had before that time been carried on. The actual bones of these animals were subsequently disinterred by observers capable of describing their mode of occurrence, so that Pallas had his curiosity much excited by the accounts which had already been published. There was still much to be found out regarding these strange

[1] Another edition of this translation appeared in 8 volumes 8vo, and was reprinted at Bâle in 1806.

relics of the frozen north, and Pallas determined to investigate the subject in the fullest detail. He kept his eye open for every trace of fossils of any kind, and one of the most valuable parts of his labours is to be seen in the precision with which he chronicles every fossiliferous locality. But the most astonishing feature of his journeys in this respect was the proofs he obtained of the almost incredible number of bones and tusks of the huge pachyderms. The whole vast basin of Siberia lying to the east of the Ural mountains and north of the Altai chain to the shores of the Arctic Ocean was found by him to be, as it were, strewn with these remains. He noticed that the bones belonged to species of elephant, rhinoceros and buffalo, and in one case he saw parts of the carcase of a rhinoceros still retaining its leather-like skin and its short hairs. From the abundance of hair on some parts of the skin of these animals, he inferred that the rhinoceros of Siberia could live in a more temperate climate than its descendants now enjoy.

But undoubtedly the most important contribution made by Pallas to geological investigation is to be found in his memoir on the formation of mountains and the changes that have taken place on the globe, particularly with regard to the Empire of Russia.[1] The highest mountains, he remarked, are composed of granite, with various schists, serpentine, grits, and other bedded masses in vertical or highly inclined positions. These formed his Primitive band, and in his opinion were older than the creation of organized beings, for no trace of organic remains was to be found in any part of them.

[1] *Act. Acad. Sci. Imp. Petropolit.* 1777, pp. 21-64.

The primitive schistose band of the great chains is immediately succeeded by the calcareous band, which consists first of solid masses of limestone, either containing no marine productions or only slight traces of them. The thick beds of limestone are placed at high angles and parallel to the direction of the chain, which is also generally that of the schistose band. As they recede from the line of the mountains, the limestones rapidly sink down into a horizontal position, and soon appear full of shells, corals and other marine organisms. These upheaved limestones form his Secondary mountains. A third series of rocks, which seemed to him to be the record of some of the latest revolutions of the globe, consists of sandstones, marls, and various other strata, forming a chain of lower hills in front of the limestone range. To this series of deposits he gave the name of Tertiary mountains.

These geological terms, thus proposed by Pallas, were not of course used by him in their more precise modern definition. We know, for example, that his Tertiary mountains consisted mainly of the younger Palæozoic sediments which are now called Permian, and that with these ancient formations he included the sands and clays that inclose the remains of mammoth, rhinoceros and other extinct mammals.

The main value of his observations lies in his clear recognition of a geological sequence in passing from the centre to the outside of a mountain-chain. He saw that the oldest portions were to be found along the axis of the chain, and the youngest on the lower grounds on either side. He recognized also that the sea had left abundant proofs of its former presence on the land, he thought that its level had never been more than 100 fathoms higher than at present, and he supposed that the elevation of the mountains had been caused by commotions of the globe.[1]

We now pass from the Ural chain which served Pallas as his type of mountain-structure to another and more famous group of mountains, where, during the same period, another not less zealous explorer was at work. The labours of De Saussure among the Alps mark an epoch, not only in the investigation of the history of the globe, but in the relations of civilized mankind to the mountains which diversify the surface of the land.

Up till towards the end of last century mountain-scenery was usually associated in men's minds with ideas of horror, danger, and repulsion. Every reader of English literature will remember passages, alike among poets and prose-writers, wherein the strongest abhorrence is expressed for the high, rugged and desolate regions of the earth. These tracts, which had in themselves no attractions, were generally looked upon as best seen from a distance, and not to be entered or traversed save on the direst compulsion.

This prejudice, which we all now laugh at, was first broken down by the scientific researches of Horace-Benedict de Saussure (1740-1799), from which we may date the rise of the modern spirit of mountaineering. He it was who first taught the infinite charm and variety of mountain-scenery, the endless multiplicity of natural phenomena there to be seen, and the enthusiasm which the mountain-world will

[1] See the summary of Pallas's views given by D'Archiac in his *Cours de Paléontologie Stratigraphique*, p. 159, 1862. For a fuller exposition consult *Journal de Physique*, xiii. (1779), pp. 329-350.

awaken in the heart of every responsive climber. How few among the thousands who every year repair to the Alps, the Pyrenees, the Caucasus, or who find their way to the peaks of the Rocky Mountains and the Sierra Nevada, are aware of the debt they owe to the great geologist of Geneva!

De Saussure was born in that city in the year 1740. His career at college was so distinguished that at twenty years of age he became a candidate for a professorship of mathematics, and at two-and-twenty obtained one of philosophy. Trained in physical science, he acquired habits of exactitude in observation and reasoning, which stood him in good stead in the scientific life to which he eventually devoted himself. Botany was his first love, and after a long and fruitful devotion to other parts of the domain of science, it was to plants that he turned again at last in the closing years of his life. Amidst his laborious campaigns in the Alps, the plants of the mountains never lost their charm for him. Among the highest crests, surrounded by all that is most impressive in nature, and occupied with the profoundest problems in the history of the globe, he would carefully gather the smallest flower and mark it with pleasure in his notebook.[1]

De Saussure's attitude towards his native mountains may be inferred from a few of the sentences with which he prefaces his immortal work. "It is the study of mountains which above all else can quicken the progress of the theory of the earth or geology. The plains are uniform, and allow the rocks to be seen only where these have been excavated by running water

[1] Cuvier, "Éloge de Saussure," *Éloges*, vol. i. p. 411.

or by man. The high mountains, on the other hand, infinitely varied in their composition as in their forms, present gigantic natural sections wherein the order, the position, the direction, the thickness and the nature of the different formations of which they are composed, as well as the fissures which traverse them, can be seen with the greatest clearness and at one view. Nevertheless, to no purpose are these facilities of observation offered, if those who propose to study the question do not know how to consider these grand objects as a whole and in their widest relations. The sole object of most travellers who call themselves naturalists is to collect curiosities; they walk, or rather they crawl, with their eyes fixed on the ground, picking up little bits here and there, without aiming at any general observations. They are like an antiquary who at Rome, with the Pantheon and the Colosseum in front of him, should scrape the ground to seek for pieces of coloured glass without ever casting his eyes on the architecture of these superb edifices. It is not that I advise the neglect of detailed observations. On the contrary, I look upon them as the only basis of solid knowledge. But while we gather these details, I desire that we should never lose sight of the great masses, and that we should always make a knowledge of the great objects and their relations, our aim in studying their small parts.

"But to observe these mighty masses we must not content ourselves with following the high-roads, which nearly always wind through the valleys, and which never cross the mountains, save by the lowest passes. We must quit the beaten tracks, and climb to the lofty summits, whence the eye can take in at one sweep a multiplicity of objects. Such excursions are toilsome, I admit; we must relinquish carriages, and even horses, endure great fatigue, and expose ourselves sometimes to considerable danger. Many a time the naturalist, when almost within reach of a summit on which he eagerly longs to stand, may doubt whether he has still strength enough left to reach it, or whether he can surmount the precipices which guard its approaches. But the keen fresh air which he breathes makes a balm to flow in his veins that restores him, and the expectation of the great panorama which he will enjoy, and of the new truths which it will display to him, renews his strength and his courage. He gains the top. His eyes, dazzled and drawn equally in every direction, at first know not where to fix themselves. By degrees he grows accustomed to the great light, makes choice of the objects that should chiefly occupy his attention, and determines the order to be followed in observing them. But what words can describe the sensations or the ideas with which the sublime spectacle fills the soul of the philosopher. Standing as it were above the globe, he seems to discover the forces that move it, at least he recognizes the principal agents that effect its revolutions."

De Saussure spent his life among the scenes he so enthusiastically described, studying the meteorology no less than the geology of the Alps. As regards the geological structure of the mountains and the origin of their component rocks, he seems hardly to have advanced beyond the ideas of Pallas. He believed, with Werner, that the central granite had resulted from deposition and crystallization in the waters of a primeval ocean. The vertical or highly inclined limestones, and other strata flanking the granite, were for a long time regarded by him as still in the position in which they were originally deposited. It was only when he found among these strata layers of sand and rounded pebbles that he was driven to admit that there had been some disturbance of the earth's surface.

Like Pallas and his contemporaries generally, De Saussure never attempted to set down his observations of the distribution of the rock formations upon a map, nor, though he had before him the excellent sections constructed by Lehmann, to which reference will be made in the following lecture, did he give definite expression to his ideas of the mutual relations of the rocks by constructing a horizontal section even of the most general and diagrammatic kind. It is thus a somewhat laborious task to gather from his *Voyages dans les Alpes* what precisely were the opinions he held in regard to tectonic questions. To him, however, so far as I have been able to discover, we owe the first adoption of the terms geology and geologist. Our science had formed a part of mineralogy, and subsequently of physical geography. The earliest writer who dignified it with the name it now bears was the first great explorer of the Alps.[1]

[1] In the year 1778 there appeared at the Hague the first imperfect edition of De Luc's *Lettres Physiques et Morales sur les Montagnes*, in the introduction to which the author states that for the science that treats of the knowledge of the earth he employs the designation of Cosmology. The proper word, he admits, should have been Geology, but he "could not venture to adopt it because it was not a word in use" (Preface, p. viii.) In the completed edition of his work, published the next year, he repeats his statement as to the use of the term Cosmology, yet he uses Geology in his

De Saussure's theoretical views underwent some modification during the prolonged period occupied by the publication of his work, though they seem never to have advanced much, notwithstanding his constantly increasing experience and the enormous amount of observations amassed by him regarding the rocks of the mountains.

His first quarto volume appeared in 1779, the second in 1786, the third and fourth in 1796. There was thus an interval of fifteen years during which, with unwearied industry, he continued to traverse the Alps from end to end, and to multiply his notes regarding them. Yet he does not seem ever to have reached any broad conceptions of stratigraphical succession, or of orographical structure. When he came upon strata crumpled and doubled over upon themselves, he thought of crystallization in place as the cause of such irregularities. The idea of subterranean disturbance would sometimes occur to him, but for many years he dismissed it with an expression of his incredulity, remarking that "if the underground fires had been able to upraise and overturn such enormous masses, they would have left some trace of their operation, but that after the most diligent search he had been unable to discover any mineral or stone which might even be suspected to have undergone the action of these fires."[1] He had thus no conception of any operation of nature, other than that of volcanoes, which could produce great disturbances of the

text notwithstanding (vol. i. pp. 4, 5). In the same year (1779), De Saussure employs the term Geology in his first volume without any explanation or apology, and alludes to the geologist as if he were a well-known species of natural philosopher. (See his *Discours Préliminaire*, pp. vii., ix., xiv., xvi.)

[1] *Voyages dans les Alpes*, vol. iii. (1796) p. 107.

terrestrial crust. Not only had he met with no trace of any igneous rock in the Alps, but the granite veins which he found traversing a schist, and which he at once regarded as throwing light on the origin of that rock, were believed by him to be almost demonstrably due to infiltration, as the granite itself had been formed from crystallization in the waters of the ancient ocean.[1]

Even when he found the vertical conglomerate of Valorsine, and recognized that it must have been originally deposited horizontally, he refrained from hazarding a conjecture as to the reason of its position. "We are still ignorant," he says, "by what cause these rocks have been tilted. But it is already an important step, among the prodigious quantity of vertical strata in the Alps, to have found certain examples which we can be perfectly certain were formed in a horizontal position."[2]

It is interesting, however, to notice that, among the agenda which he inserted at the close of his last volume, as the fruit of his long experience, he gives a chapter of suggestions as to what should be looked for in regard to organic remains among the rocks. Some of these suggestions are full of sagacity, and show that, though he had not followed them in his own researches, he recognized the importance of the advice he was giving. One of his admonitions was "to ascertain whether certain shells occur in the older rocks but not in the later, and whether it is possible by their means to fix the relative ages and eras of appearance of the different species." Another recommendation is "to compare exactly the fossil bones, shells,

[1] Vol. i. pp. 533 *et seq.*
[2] Vol. ii. § 690.

and plants with their living analogues and to determine whether they differ from these."[1]

One of the most interesting features of De Saussure's work is exhibited in the care with which he equipped himself for the study of the rocks of the region that he undertook to examine and describe. Petrography was at that time in a very embryonic condition. Linnaeus and Wallerius had made a beginning in the definition of rocks, but Werner's labours had hardly begun. The Swiss naturalist set himself with his usual ardour to the study, into which he introduced his accustomed order and precision. Among other aids in his researches, he devised a series of experiments in fusion, in order to determine for himself the probable origin of different rocks, and especially to enable him to decide whether certain varieties could be produced by the melting of others. It will be remembered that Desmarest had propounded the doctrine that the basalts of Auvergne had been formed by the fusion of the underlying granite by volcanic fire. De Saussure, when he began to study these questions, was astonished to discover how little had been done in the way of experimental research into the nature of rocks. He selected various Swiss granites, and found that in no instance could he reduce them by fusion into basalt. In case there might be any deficiency in the granites of his own country, he tried the effects of a high temperature on pieces of granite which he had himself collected in Auvergne, but equally without success. He then experimented on a granite containing abundant schorl, and obtained a black vesicular glass sprinkled with the white grains of infusible quartz. He next took specimens of

[1] Vol. iv. p. 505.

different porphyries, and though he got a compact black enamel, nothing appeared in the least resembling basalt, whence he concluded that it could not be from the natural fusion of such rocks as these that basalt was derived.[1]

These experiments are especially interesting, as they mark the earliest beginnings of experimental geology. The results obtained by them were negative, and De Saussure did not advance further along the path he had thus opened into a domain which was destined in future to become so fruitful. But his name must ever be had in honour for the share he took in establishing the use of direct experiment in the elucidation of geological problems. He did not live to put in practice the directions which he left for the further exploration of the Alps by those who should come after him. A disease, which perhaps took its rise from the fatigues and privations of his life among the mountains, began to increase upon him after his fiftieth year. It was aggravated by anxiety on account of the effect of the French Revolution on his private resources. After three successive strokes of paralysis he died in 1799 at the age of fifty-nine years.

De Saussure was the first and most illustrious of that distinguished band of geologists which Switzerland has furnished to the ranks of science. To his inspiration and example we owe the labours of Merian, Escher von der Linth, Studer, Favre, and the later and living observers who have so diligently and successfully unravelled the complicated structure of the Alps. His descriptions of a great mountain-chain form admirable models of careful observation and luminous narrative. Though he did not add much to the

[1] Vol. i. p. 122-127.

advancement of geological theory, he contributed largely to the stock of ascertained fact, which was so needful as a basis for theoretical speculation. The data which he collected became thus of the utmost service to those who had to work out the principles of geology. To Hutton, for example, they supplied many admirable illustrations of the geological processes on which he based his *Theory of the Earth.* It was under the guidance of the great Swiss observer that the Scottish philospher stood in imagination on the summit of the Alps, and watched from that high tower of observation the ceaseless decay of the mountains, the never-ending erosion of the valleys, and that majestic evolution of topography which he so clearly portrayed. Among the illustrious men who contributed to plant the foundations of geology, an honoured place must always be assigned to De Saussure.

LECTURE III

History of the doctrine of Geological Succession—Lehmann, Fuchsel, Werner—The spread and decline of Wernerianism—D'Aubuisson, Von Buch.

The most casual observation suffices to convince us that the surface of the earth has not always been as it is to-day. At one place we recognize, in sheets of sand and gravel, proofs of the former presence of running water, where none is now to be seen. Elsewhere shells and other marine organisms underneath the soil prove that the dry land was formerly the bed of the sea. Masses of sandstone, conglomerate and limestone, once evidently laid down in horizontal layers on the sea-bottom, but now hardened into stone, disrupted, placed on end, and piled up into huge hills and mountain-ranges, show beyond all question that stupendous disturbances attended the conversion of the sea-floor into land.

These proofs of former revolutions on the surface of the globe are so abundant, so easily comprehended and so convincing, that some of them early attracted notice. The frequent references to them, in the literature of ancient as well as modern times, prove how familiar they have been from the dawn of civilization. They have suggested many cosmologies and theories of the earth, for there has ever been a craving to explain the origin of things. But these older interpretations of nature were rather exercises of the imagination than results of observation and deduction.

In the gradual growth of knowledge regarding the history of our globe, it is surprising how late men were in realising that this knowledge must be based not on mere speculation, but on patient investigation of what evidence can be gathered from the structure of the planet itself. Slowly and laboriously the truth was reached that the rocks which form the terrestrial crust bear witness to the passage, not of one or two, but of a whole series of revolutions, that these changes occupied vast intervals of time, and that while they varied indefinitely in their local effects from one region to another, they were but incidents in one vast onward march of development which embraced the whole globe within its influence. What we now know as the doctrine of geological succession, in other words, the history of the evolution of the earth, during a prolonged series of ages up to the present time, took shape with extreme slowness, each generation adding a little to the basis of fact and to the superstructure of inference.

There were in especial two lines of investigation along which progress could be made. On one of these, the various masses of rock that are visible over the surface of the globe had to be studied with a view to the determination of their origin and sequence. On the other line, the details of these rock-masses, and more particularly of the sedimentary series, had to be worked out, and their organic contents to be noted, in order to ascertain how far the living creatures of older times differed from those of the present.

The former of these two branches of research naturally came to be pursued first. It is by far the more obvious of the two, and considerable progress had to be made in it before the very possibility of the second line of enquiry could be recognized and pursued.

We have seen that with all his sagacity and insight, Guettard gave no indication that he had any ideas as to the chronological relations of the various groups of strata which he included in his "bands." Neither he nor his contemporaries ventured to draw geological sections. We have found that even De Saussure and Pallas, though they saw that the rocks of the central parts of mountain-chains are older than those of their flanks, did not definitely express their ideas on this subject in graphic form. Desmarest had clearly perceived the evidence for a long sequence of volcanic eruptions in Central France, but he never applied this evidence towards an elucidation of the general history of the globe. Yet as far back as the time of Guettard, the first seeds had been sown which, before the end of the century, were to germinate in so wide an expansion of geological theory.

I propose in the present lecture to trace the history of the idea of geological succession during the latter half of last century. In that time it was advanced more particularly by three observers, Lehmann, Fuchsel, and Werner.

The earliest definite statements as to a recognisable order among the rocks of the earth's crust are to be found in a treatise by Johann Gottlob Lehmann (died 1767), published at Berlin in 1756. It is a little duodecimo volume, roughly printed on poor paper, extending to 240 pages, and bearing the title *Versuch einer Geschichte von Flötz-Gebürgen*, etc. It gives the results of the author's own observations among the rocks of the Harz and the Erzgebirge. He recognized three orders of mountains. 1st, Those which appeared coeval with the making of the world; 2nd, those which arose from a general alteration of the ground; and 3rd, those which have been formed from time to time by local accidents. The first order is distinguished not only by the greater height of its members, but by their internal structure. The rocks are less various, their strata are not horizontal but vertical or inclined, and their layers are neither so weak nor so multifarious as those of the other groups. Nor are they mere superficial deposits, but they plunge down into unknown depths into the earth's interior. The second order, or Flötz-gebirge, are of much younger date, and have arisen from the successive deposit of sediments from water that once covered their sites, these sediments being now seen in flat sheets or strata piled above each other to no great height. Lehmann showed that these sedimentary deposits contain abundant petrifactions, such as remains of wild animals, shells, plants and trees. He gave a number of sections to show the order in which the strata succeed each other, remarking that the coarser sediments were generally lowest, while limestone came at the top. His profiles of the succession of strata showed a remarkable grasp of some of the essential features of tectonic geology. It is singular that these suggestive examples should not have had more imitators during the latter half of last century. Nothing could be more precise and distinct than Lehmann's demonstration of the stratified nature and aqueous origin of the younger formations of the earth's crust, or his proofs that the strata succeed each other in a definite order in the region with which he was acquainted.

Contemporary with Lehmann, and though less frequently quoted, worthy of a still higher place in the bede-roll of geological worthies, was George Christian Fuchsel (1722-1773).[1] This remarkable man was the son of a baker in Ilmenau, at the northern foot of the Thuringian Forest. He studied at the Universities of Jena and Leipzig, and having from an early date addicted himself to minerals and rocks, he was lucky enough to find a seam of coal at Mühlberg, near Erfurt, and still more fortunate to receive from the proprietor of the ground a reward of 200 crowns for the discovery. At Erfurt he took his degree of Doctor of Medicine, and eventually became physician to the Prince of Rudolstadt. He lived to the age of only fifty-one, and died in the year 1773.

His position at Rudolstadt was favourable for the cultivation of his taste for geological pursuits. To the south rose the ancient rocks of the Thuringer Wald, flanked by the great series of Permian and Triassic formations, regularly superposed upon each other, and cut out into valleys by the rivers that drain the mountain range. In the year 1762, when he was forty years of age, he published one of the most remarkable treatises which up to that time had been devoted to the description of the actual structure and history of the earth. It was in Latin, and, under the title of "A History of the Earth and the

[1] For the data here given I am indebted to a brief notice by C. Keferstein in the *Journal de Géologie*, vol. ii. (1830), p. 191, and to his account of Fuchsel in his *Geschichte und Litteratur der Geognosie* (1840), p. 55 *seq.*

Sea, based on a History of the Mountains of Thuringia," appeared in the *Transactions of the Electoral Society of Mayence*, established at Erfurt.[1] It was illustrated with a geological map and sections of the country. Eleven years later he published in German a *Sketch of the most Ancient History of the Earth and Man*, which contained a further development of his geological views.[2]

These views were founded on the author's own observations in the region where he had been born and passed his life. He recognized as clearly as Lehmann, and with more accuracy of detail, the sequence of stratified rocks resting in gently-inclined strata against the older upturned masses of the mountains. He noted the position of the Coal with its exotic plants, followed by the copper-bearing shales, Zechstein, mottled sandstone, marls, gypsum, and finally the Muschelkalk.

Taking no limited or parochial view of the phenomena that presented themselves before his eyes, he connected the history of his little principality with that of the whole globe. In the order of succession of the rocks around him, he saw the records of a series of changes which the earth had once undergone. These changes were conceived by him to have been of no abnormal kind, but to have resembled those which might quite possibly occur now, for, in his opinion, our planet had always presented phenomena similar to those of the present time. He saw

1 "Historia terrae et maris, ex historia Thuringiae per montium descriptionem erecta" (*Trans. Elect. Soc. Mayence*, vol. ii. pp. 44-209). The map was the first detailed geognostical and petrographical map of a large district in Germany, and the sections were excellent for their time.

2 *Entwurf zu der ältesten Erd- und Menschengeschichte*, 275 pages, 8vo, 1773.

that the existing dry land was in large measure formed of strata that had once been laid down on the floor of the sea, like the sandstones, marls and limestones with which he was familiar. Rising from underneath these strata, the older and inclined rocks of the mountains appeared to him as the relics of a more ancient continent, which had in like manner been built up of marine sediments. He believed that the tilted, highly-inclined positions of these rocks were due to their having tumbled down into the hollow interior of the earth.

Fuchsel, with singular sagacity, not only interpreted the origin of individual strata, but divined that a continuous series of strata of the same composition constitutes a formation, or the record of a certain epoch in the history of the globe, thus anticipating a doctrine which afterwards took a prominent place in the system of Werner. All these sediments were originally deposited horizontally. Where they have been placed in inclined positions, the alteration was, in his opinion, to be attributed to some subsequent disturbance, such as the effects of earthquakes or oscillations of the ground. To earthquakes also he assigned the production of the rents which, being filled from above, now form veins in the rocks. It was his opinion that the earthy passage-beds between formations marked intervening periods of disturbance.

The Muschelkalk in Fuchsel's district forms the highest of the Secondary formations, and is succeeded by the various alluvial deposits. These youngest accumulations, containing only terrestrial remains, were looked upon by him as having arisen from the action of a great deluge.

This singularly shrewd observer deserves further to be remembered for the place which he assigned to organic remains in his theoretical views of the past history of the earth. He clearly recognized these objects as relics of once living things. He saw that the Coal was distinguished by its land-plants, the Zechstein by its gryphites, the Muschelkalk by its ammonites; further, that some formations contained only marine remains, others only terrestrial, and thus that the latter point to the neighbourhood of ancient land, while the former indicate the presence of the sea.

The clear and detailed evidence brought forward by Lehmann and Fuchsel, that the materials of the terrestrial crust had not been thrown down at random, but succeeded each other in a certain definite order, and contained a record of former processes and changes, like those in progress now, ought to have given a great forward impetus to the study of the history of the earth. Lehmann's work, however, was not in itself attractive, and Fuchsel's first essay, though by far the most detailed and philosophical treatise on the subject that had yet appeared, was written in Latin and buried in the publications of an obscure Society. Fuchsel himself lived quietly in a little town, with no disciples to spread his doctrines, so that his very name remained hardly known even in Germany, while other and much inferior writers achieved a wide reputation. His writings seem to have dropped out of sight, until they were unearthed and brought to notice fifty-seven years after his death by Keferstein. The seed sown by Lehmann and Fuchsel was thus long in springing into abundant growth. During the remainder of last century the idea of geological succession was proclaimed, indeed, from the housetops, but it was so mingled with fanciful hypothesis, that its truth and real value were almost lost sight of.

We come now to the time of the advent of a man who bulks far more largely in the history of geology than any of those with whom up to the present we have been concerned—a man who wielded an enormous authority over the mineralogy and geology of his day. Through the loyal devotion of his pupils, he was elevated even in his lifetime into the position of a kind of scientific pope, whose decisions were final on any subject regarding which he chose to pronounce them. During the last quarter of the eighteenth century, by far the most notable figure in the ranks of those who cultivated the study of minerals and rocks was unquestionably Abraham Gottlob Werner (1749-1817).

The vast influence which this man wielded arose mainly from his personal gifts and character, and especially from the overmastering power he had of impressing his opinions upon the convictions of his hearers. It was an influence of a curiously mingled kind. From one point of view, Werner appears to us as the enthusiastic teacher, drawing men from all countries under his spell, and kindling in them much of his own zeal for the study of minerals and rocks. In another aspect, he stands out as the dogmatic theorist, intolerant of opinions different from his own, training his pupils in an artificial and erroneous system, and sending them out into the world not patiently to investigate nature, but to apply everywhere the uncouth terminology and hypothetical principles which he had taught them.

Though he himself mixed but little publicly in the dispute, he was directly the cause of the keen controversy over the origin of basalt, the echoes of which had hardly ceased when some of the older geologists of our day were born. I have myself known a number of men who remembered well the acrimony of the warfare, and some of whom even played the part of combatants in the struggle. Werner had a large following. He was undoubtedly the most popular teacher of the science of minerals and rocks in his time. His services to mineralogy were great, and have always been freely admitted. By the partiality of his pupils and friends he was also raised to the highest eminence as a teacher of geology, and was even looked up to as the founder of that science. The noise of conflict, and the plaudits of enthusiastic disciples have now long been silent. We can calmly consider what Werner did, in what state he found the science of the rocks, and in what condition he left it. As the result of my own investigation in this subject I have been compelled to arrive at the conclusion that, although he did great service by the precision of his lithological characters and by his insistence on the doctrine of geological succession, yet that as regards geological theory, whether directly by his own teaching, or indirectly by the labours of his pupils and followers, much of his influence was disastrous to the higher interests of geology. The career of such a man, so full of contradictions, so preponderant in the studies to which it was devoted, and so momentous in its effects upon the progress of science in his own generation, merits the careful consideration of all who would realize how geology has gained its present place.

Werner was born on 25th September 1749 at Wehrau on the Queiss in Upper Lusatia.[1] His ancestors had been engaged in the iron industry of that region of Germany for some 300 years. His father was inspector of Count Solms' foundry, and at one time it seemed as though the future mineralogist were to carry on, in the same profession, the traditions of the family. From infancy he was familiar with stones. When still hardly able to speak, it was one of his favourite amusements to break down pieces of sandstone and marl. After he had begun to learn his alphabet, his father, as a reward for proficiency in his lessons, would allow him to look over a small collection of minerals which he kept in a box, and would talk to him about them, their origin and their uses. Late in life Werner could vividly recall the very minerals that were the playthings of his childhood—various ores and spars, as well as some varieties of which his father did not know the names. When he could read, his favourite books were lexicons of mining and manufactures, wherein he specially selected the articles on mineralogy. His tendencies, thus early shown, were further fostered by his father, who in hours of leisure would entertain him with stories of the mines.

In his tenth year the boy went to school at the old fortified town of Bunzlau in Silesia, and after a few years returned in 1764 to assist his father and become controller of the smelting-houses at Wehrau. But the aspirations he

[1] For the biographical details given in this sketch I am indebted partly to the "Kurzer Nekrolog Abraham Gottlob Werners," by K. A. Blöde, in the *Memoirs of the Mineralogical Society of Dresden*, vol. ii. (1819), p. 249, and partly to the *Éloge* on Werner by Cuvier. Blöde, who had access to family documents, gives 1749 as the year of Werner's birth; Cuvier and other authorities make it 1750.

had formed to devote himself to minerals seem at last to have grown too strong to be resisted, so that after doing his duty at the foundries for five years he resolved to betake himself in 1769 to the Mining Academy of Freiberg, which had been founded two years before, and of the attractions of which he had no doubt heard much. Amid what was now thoroughly congenial to him, he threw himself with enthusiasm into the work of the school, not only availing himself of all the formal instruction in the art of mining to be had from the teachers, but visiting all the chief Saxon mines, especially those of most importance in the Freiberg district, descending the shafts, joining in the manual labour of the miners, and thus making himself master of the whole art of mining, below ground as well as above. His zeal and capacity were soon recognized by the officials at Freiberg, and before he had been long there he was offered a place in the Saxon Corps of Mines. He was not unwilling to accept the appointment, but determined first of all to prosecute a wider range of study for a few years at the University of Leipzig.

Accordingly, after some two years spent in mining pursuits Werner went to Leipzig in the spring of the year 1771, and for the next two years devoted himself almost entirely to the study of law. In his third and last year at the University, he seems to have taken up a miscellaneous series of subjects, especially modern languages, but he settled down at last to the prosecution of his first love—mineralogy, and with such industry and enthusiasm did he pursue this study, that while in his twenty-fifth year, and still a "student of the science and law of mining," he published his first essay—a little duodecimo of 300 pages, on the external characters of minerals.[1] We can imagine the astonishment and delight of the lovers of mineralogy when they first got hold of this treatise, and found there, instead of the miscellaneous, isolated, and heterogeneous observations to which they were accustomed, an admirably orderly method and a clear marshalling and co-ordination of facts, such as had never before been seen in mineralogical literature.

On leaving the University of Leipzig, Werner went back to his home by the Queiss. It seemed as though the authorities at Freiberg, who at one time were so anxious to secure his services, had now forgotten his existence. He had heard nothing more of the proposal to engage him, and began to arrange his plans for the future. But the officials, though slow in their movements, had not lost sight of him. They had made note of his progress at Leipzig, and especially of his admirable little book, and at last in February 1775, to his own astonishment, Werner received a call from them to become Inspector and Teacher of Mining and Mineralogy in the Freiberg Mining Academy at a yearly stipend of 300 thalers. He thus attained before he was twenty-six years of age the position in which he spent the rest of his life and achieved his great fame. For some forty years he continued in the same appointment. By his genius he raised the Mining School from a mere local seminary, founded for the training of a few Saxon miners, to the importance of a great academy or university, to which, as in mediaeval times, his renown as a

[1] "Von den aüsserlichen Kennzeichen der Fossilien, abgefasst von Abraham Gottlob Werner, Der Bergwerks-Wissenschaften und Rechte Beflissenen," Leipzig, 1774.

teacher drew pupils from all corners of the civilized world. Men advanced in years, as well as youths, sometimes even men of science already distinguished, betook themselves to the acquisition of German that they might attend the lectures of the great oracle of geology.

The life of such a man, seldom tempted to stir from home, immersed in the daily discharge of the duties of his office, and only varying from year to year the subject of his prelections, offers little incident to the biographer. Moreover, though he precociously began so young as an author, he wrote merely a few short treatises and papers in journals, thus leaving hardly any personal memorial behind him. It is from the writings of his pupils that we chiefly learn what manner of man he was, and what were the special characteristics of his teaching.

From the portrait of him prefixed to one of his works,[1] we gather that his large keen eyes looked out beneath a broad and high forehead, over which his hair was dressed in the formal wig-fashion of the day, and turned up in large curls on either side. The round, smooth-shaven face had as its most conspicuous feature a mouth in which, while the firm lips denoted decision of character, the upward curve on either side, combined with a slight dimpling of the cheeks, gave the impression of great sweetness of disposition, with a touch of humour, and a certain degree of timidity. There is moreover a notable trimness of person, indicative of the exceeding orderliness of his whole nature.

His personal charm must have been altogether remarkable. Cuvier tells us with what paternal fondness Werner

[1] *New Theory of the Formation of Veins.* Translated by Charles Anderson, M.D. Edinburgh, 1809.

was accustomed to treat his pupils. There was no sacrifice of time or energy which he would not make for their sake, even his slender purse was at their service if they ever stood in need of pecuniary help. When the students crowded round him so that only a portion of them could conveniently see and hear his demonstrations, he would divide them and repeat his lecture.[1]

His manner of discourse also was so attractive and stimulating that he riveted the attention of his pupils, incited them to pursue the studies that he loved, and fired them with a desire to apply his methods. Ostensibly he had to teach mineralogy—a science which in ordinary hands can hardly be said to evoke enthusiasm. But Werner's mineralogy embraced the whole of nature, the whole of human history, the whole interests and pursuits and tendencies of mankind. From a few pieces of stone, placed almost at random on the table before him, he would launch out into an exposition of the influence of minerals and rocks upon the geography and topography of the earth's surface. He would contrast the mountainous scenery of the granites and schists with the tamer landscapes of the sandstones and limestones. Tracing the limits of these contrasts of surface over the area of Europe, he would dwell on their influence upon the grouping and characteristics of the nations. He would connect, in this way, his specimens with the migration of races, the spread of languages, the progress of civilization. He would show how the development of the arts and industries of life had

[1] There is an enthusiastic account of Werner as a teacher by one of his pupils, C. A. Böttiger: "Über Werner's Umgang mit seinen Schülern,"—*Auswahl. Gesellsch. Mineralog. Dresden*, Band ii. p. 305 (1819).

been guided by the distribution of minerals, how campaigns, battles, and military strategy as a whole, had been dependent on the same cause. The artist, the politician, the historian, the physician, the warrior were all taught that a knowledge of mineralogy would help them to success in their several pursuits. It seemed as if the most efficient training for the affairs of life were obtainable only at the Mining School of Freiberg.

By such continual excursions into domains that might have been thought remote enough from the dry study of minerals, and by the clear and confident method, playful vivacity and persuasive eloquence with which they were conducted, Werner roused his hearers to a high pitch of enthusiasm. No teacher of geological science either before or since has approached Werner in the extent of his personal influence or in the breadth of his contemporary fame.

Let us now inquire what were the leading characteristics of his doctrines, and what permanent influence they exerted upon the progress of the science of his time. His brilliance and discursiveness might attract and retain large audiences, but his lectures must have possessed more solid and enduring qualities, which inspired his disciples to devote their lives to the studies into which he introduced them, and filled them with the ardour of devoted proselytes.

The first feature to which we may direct our attention, distinguishable in every part of his life and work, was his overmastering sense of orderliness and method. This habit of mind became in him a true passion. He is said to have bought books rather to arrange them systematically than to read them. He observed the details of social etiquette as punctiliously as the characters of minerals, but with one remarkable exception, to which I shall afterwards allude; and he would deliberate over the arrangement of a dinner with as much gravity as over that of his library or his cabinet.

We cannot take up any of Werner's writings without at once noting this prominent peculiarity of his mind. Every fact, every proposition is definitely classified and ticketed, and even if he has little or nothing to say under any particular subdivision, the subdivision is nevertheless placed in its due niche all the same.

This methodical habit proved of the greatest service to the cause of mineralogy. When Werner entered upon his mineralogical studies, the science of minerals was an extraordinary chaos of detached observations and unconnected pieces of knowledge. But his very first essay began to put it into order, and by degrees he introduced into it a definite methodical treatment, doing for it very much what Linnaeus had done some years before for botany. Like that great naturalist, he had to invent a language to express with precision the characters which he wished to denote, so that mineralogists everywhere could recognize them. For this purpose he employed his mother tongue, and devised a terminology which, though artificial and cumbrous, was undoubtedly of great service for a time. Uncouth in German, it became almost barbarous when translated into other languages. What would the modern English-speaking student think of a teacher who taught him, as definite characters, that a mineral could be distinguished as "hard, or semi-hard," "soft or very soft," as "very cold, cold, pretty cold, or rather cold," as "fortification-wise bent," as "indeterminate curved lamellar," as "common angulo-granular," or as "not particularly difficultly frangible"?[1]

Werner arranged the external characters of minerals in so methodical a way, that they could readily be applied in the practical determination of species. Yet strangely enough he neglected the most important of them all—that of crystalline form. From the individual minerals, he proceeded to the consideration of their distribution, and the character and origin of the different rocks in which they occur. To this branch of inquiry he gave the name of geognosy, or knowledge of the earth, and he defined it as the science which reveals to us in methodical order the terrestrial globe as a whole, and more particularly the layers of mineral matter whereof it consists, informing us of the position and relations of these layers to each other, and enabling us to form some idea of their origin. The term geology had not yet come into use, nor would either Werner or any of his followers have adopted it as a synonym for the "geognosy" of the Freiberg school. They prided themselves on their close adherence to fact as opposed to theory. One of them, with pointed reference to the writings of Hutton and Playfair, which had appeared shortly before, wrote: "We should form a very false conception of the Wernerian geognosy were we to believe it to have any resemblance to those *monstrosities* known under the name of *Theories of the Earth*. . . . Armed with all the *facts* and inferences contained in these visionary fabrics, what account would we be able to give of the mineralogy of a country, if required of us, or of the

[1] These terms are all taken from the Wernerian system as expounded in English by Werner's pupil, Jameson.

general relations of the great masses of which the globe is composed?"[1] The geognosts boasted of the minuteness and precision of their master's system, and contrasted the positive results to which it led with what they regarded as the vague conclusions and unsupported or idle speculations of other writers. Werner arranged the crust of the earth into a series of formations, which he labelled and described with the same precision that he applied to the minerals in his cabinet. He taught that these formations were to be recognized all over the world, in the same order and with the same characters. The students whom he sent forth naturally believed that they carried with them, in this sequence, the key that would unlock the geological structure of every country.

But never in the history of science did a stranger hallucination arise than that of Werner and his school, when they supposed themselves to discard theory and build on a foundation of accurately-ascertained fact. Never was a system devised in which theory was more rampant; theory, too, unsupported by observation, and, as we now know, utterly erroneous. From beginning to end of Werner's method and its applications, assumptions were made for which there was no ground, and these assumptions were treated as demonstrable facts. The very point to be proved was taken for granted, and the geognosts, who boasted of their avoidance of speculation, were in reality among the most hopelessly speculative of all the generations that had tried to solve the problem of the theory of the earth.

[1] Jameson, "Elements of Geognosy," forming vol. iii. of his *System of Mineralogy*, p. 42. The italics in this quotation are in the original.

Werner's first sketch of his plan of the structure of the earth's crust and the succession of the rocks that compose it appeared as a thin quarto of only 28 pages, published at Dresden in the year 1787.[1] It was descriptive rather than theoretical, and was marked by all its author's precision and orderliness of statement. It contained the essence of his system in its simplest form. In later years, as we shall see, further experience compelled him to enlarge and modify the system, but without changing the fundamental conceptions on which it was founded. The modifications, however, were not embodied by Werner in any later edition of his work. They were given by him from time to time in his lectures, and gradually became known from the writings of his students. One of the most devoted and distinguished of these followers was Robert Jameson, who afterwards became Professor of Natural History in the University of Edinburgh. He was mainly instrumental in introducing the Wernerian doctrines into Britain, and continued for many years to be their most ardent supporter. In many respects the fullest accounts of Werner's views are to be found in the various works of the Edinburgh Professor, and I shall cite some passages from them in the present lecture.

One of the fundamental postulates of the Wernerian doctrines was the existence of what were termed universal formations. When he elaborated his system, Werner had never been out of Saxony and the immediately adjacent regions. His practical knowledge of the earth was, there-

[1] *Kurze Klassification und Beschreibung der verschiedener Gebirgsarten*, von A. G. Werner, Bergakademie Inspector, und Lehrer der Bergbaukunst und Mineralogie zu Freiberg. Dresden, 1787.

fore, confined to what he could see there, and so little was then known of the geological structure of the globe as a whole, that he could not add much to his acquaintance with the subject by reading what had been observed by others. With this slender stock of acquirement, he adopted the old idea that the whole globe had once been surrounded with an ocean of water, at least as deep as the mountains are high, and he believed that from this ocean there were deposited by chemical precipitation the solid rocks which now form most of the dry land. He taught that these original formations were universal, extending round the whole globe, though not without interruption, and that they followed each other in a certain order. He affirmed that the first-formed rocks were entirely of chemical origin, and he called them Primitive, including in them granite, which was the oldest, gneiss, mica-slate, clay-slate, serpentine, basalt, porphyry, and concluding with syenite as the youngest. Succeeding these came what he afterwards separated as the Transition Rocks, consisting chiefly of chemical productions (greywacke, greywacke-slate and limestone), but comprising the earliest mechanical depositions, and indicating the gradual lowering of the level of the universal ocean. Still newer, and occupying, on the whole, lower positions, marking the continued retirement of the waters, were the Floetz Rocks, composed partly of chemical, but chiefly of mechanical sediments, and including sandstone, limestone, gypsum, rock-salt, coal, basalt, obsidian, porphyry, and other rocks. Latest of all came the Alluvial series, consisting of recent loams, clays, sands, gravels, sinters, and peat.

This system was not put forward tentatively as a suggestion towards a better comprehension of the history of the earth. It was announced dogmatically as a body of ascertained truth, about which there could be no further doubt or dispute. Let me quote by way of illustration a few sentences from Werner's *Theory of Veins,* where he definitely expresses his opinions on these matters. "In recapitulating the state of our present knowledge," he observes, "it is obvious that we know with certainty that the floetz and primitive mountains have been produced by a series of precipitations and depositions formed in succession from water which covered the globe. We are also certain that the fossils which constitute the beds and strata of mountains were dissolved in this universal water and were precipitated from it; consequently the metals and minerals found in primitive rocks, and in the beds of floetz mountains, were also contained in this universal solvent, and were formed from it by precipitation. We are still further certain that at different periods, different fossils have been formed from it, at one time earthy, at another metallic minerals, at a third time some other fossils. We know, too, from the position of these fossils, one above another, to determine with the utmost precision which are the oldest, and which the newest precipitates. We are also convinced that the solid mass of our globe has been produced by a series of precipitations formed in succession (in the humid way); that the pressure of the materials, thus accumulated, was not the same throughout the whole; and that this difference of pressure and several other concurring causes have produced rents in the substance of the earth, chiefly in the most elevated parts of its surface. We are also persuaded that the precipitates taking place from the universal water must have entered into the open fissures which the water covered. We know, moreover, for certain, that veins bear all the marks of fissures formed at different times; and, by the causes which have been assigned for their formation, that the mass of veins is absolutely of the same nature as the beds and strata of mountains, and that the nature of the masses differs only according to the locality of the cavity where they occur. In fact, the solution contained in its great reservoir (that excavation which held the universal water) was necessarily subjected to a variety of motion, whilst that part of it which was confined to the fissures was undisturbed, and deposited in a state of tranquillity its precipitate."[1]

It would be difficult to cite from any other modern scientific treatise a series of consecutive sentences containing a larger number of dogmatic assertions, of which almost every one is contradicted by the most elementary facts of observation. The habit of confident affirmation seems to have blinded Werner to the palpable absurdity of some of his statements. When, for example, he speaks of the great reservoir or excavation which held the universal water, what idea could have been present to his mind? If the primeval ocean, as he asserted, surrounded the whole globe, and was as deep as the mountains are high, where was the excavation? As an acute writer in the *Edinburgh Review* pointed out, the excavation spoken of by Werner "can mean nothing else than the convexity of the solid nucleus round which the universal water was diffused. To

[1] *Neue Theorie von der Enstehung der Gängen*, chap. vii. § 68 (1791). English translation by Anderson, p. 110 (1809).

call this convexity an excavation, is to use such a freedom with language as can only be accounted for by the perplexity in which every man, of whatever talents, must find himself involved when he attempts to describe a whole, of which the parts are inconsistent with one another."[1]

The theory of a primeval universal ocean that overtopped the mountains, which formed the basis of Werner's teaching, led in every direction to such manifest contradictions and absurdities, that we need a little patience and some imagination to picture to ourselves how it could have been received and fervently believed in by men of intelligence, to whom the facts of the earth's structure were not wholly unknown. It was claimed for Werner that the doctrine of a universal and gradually subsiding ocean, though it had been taught long before his time, was first demonstrated by him to be true, (1) because he found the older strata occupying the highest eminences, and the younger coming in at successively lower levels, down to the modern alluvia of the plains and the sea-shore, and (2) because the primitive and loftiest rocks are entirely formed of chemical precipitations, those of mechanical origin not appearing until a much later period, and becoming increasingly abundant down to the present time, when they constitute almost all the deposits that are now taking place.[2]

One of the most obvious questions that would arise, we might suppose, in the mind of any student of ordinary

[1] *Edin. Review*, xviii. p. 90 (1811).

[2] Jameson's *Geognosy*, p. 78. Werner's followers, from the prominence they gave to the sea in their geognosy, were styled Neptunists, while those of Hutton, who dwelt on the potency of the earth's internal fire, were dubbed Vulcanists.

capacity to whom the theory was propounded, would be how did the deep primitive ocean disappear. Steno, Leibnitz, and other older writers had conjectured that the waters found their way into vast caverns in the earth's interior. Such a conjecture, however, was not suited to the taste of the true Wernerian, who would allow no speculation, but took his stand on a basis of ascertained fact. Well, we may be curious to know how he disposed of the difficulty. Yet we shall search in vain through Wernerian literature for any serious grappling with this obvious, and one would have thought formidable, objection to the doctrine. Werner himself appears to have inclined to the belief that the waters vanished into space. He thought it possible that "one of the celestial bodies which sometimes approach near to the earth may have been able to withdraw a portion of our atmosphere and of our ocean."[1] But if once the waters were abstracted, how were they to be brought back again so as to cover all the hills on which his highest Floetz formations were deposited?

The most famous of the English followers of Werner, Jameson, honestly asked the question, "What has become of the immense volume of water that once covered and stood so high over the whole earth?" His answer may be cited as thoroughly characteristic of the mental attitude of a staunch Wernerian. "Although," he says, "we cannot give any very satisfactory answer to this question, it is evident that the theory of the diminution of the water remains equally probable. We may be fully convinced of its truth, and are so, although we may not be able to explain it. To know from observation that a great pheno-

[1] See D'Aubuisson's *Géognosie*, i. p. 414 (1819).

menon took place, is a very different thing from ascertaining how it happened."[1] I do not suppose that in the whole literature of science a better illustration could be found of the advice—"When you meet with an insuperable difficulty, look it steadfastly in the face—and pass on."

One might have thought that having disposed of the universal ocean, even in this rather peremptory fashion, the Wernerians would have been in no hurry to call it back again, and set the same stupendous and inexplicable machinery once more going. But the exigencies of their theory left them no choice. Having determined, as an incontrovertible fact, that certain rocks had been deposited as chemical precipitates in a definite order from a universal ocean, when these philosophers, as their knowledge of Nature increased, found that some of these so-called precipitates occurred out of their due sequence and at much higher altitudes than had been supposed, they were compelled to bring back the universal ocean, and make it rise high over hills from which it had already receded. Not only had they to call up the vasty deep, but they had to endow it with rapid and even tumultuous movement, as it swept upwards over forest-clothed lands. Having raised it as high as their so-called Floetz formations extended, and having allowed its waters to settle and deposit precipitates of basalt and greenstone, they had to hurry it away again to the unknown regions where it still remains. This, forsooth, was the system that discarded hypothesis and rested proudly on an irrefragable foundation of demonstrable fact.

Among the features of the Wernerian school, one of the

[1] Jameson, *op. cit.* p. 82.

most singular was the position it took up with regard to the evidence for disturbances of the earth's crust, and for the universality and potency of what is now termed igneous action. A hundred years before Werner's time Steno had pointed to the inclined and broken strata of Northern Italy as evidence of dislocation of the crust. The Italian observers, and especially Moro, familiar with the phenomena of earthquakes and volcanoes, had been impressed by the manifest proofs of the potency of the internal energy of the earth upon its outer form. But these early adumbrations of the truth were all brushed aside by the oracle of Freiberg. I have tried to imagine the current of thought by which Werner was led to this crowning absurdity of his system, and I think we may trace it in the history of his relation to the basalt hills of Saxony. The question is of some interest, not only as a curious piece of human psychology, but because it was on this very point of the origin of basalt that the Wernerian ship finally struck and foundered.

The year after his appointment as teacher of mineralogy, Werner visited the famous Stolpen, one of the most picturesque castle-crowned basalt hills of Saxony, to which I have already referred in connection with Agricola's revival of the old word "basalt." He had probably by this time begun to form in his mind a more or less definite picture of chemical precipitation from aqueous solution, as applied to the history of rock-masses. But be this as it may, he was aware that basalt, by not a few observers before his time, had been claimed as a rock of volcanic origin. How far he had then made up his mind as to the formation of that rock must remain in doubt. But he tells himself that at the Stolpen he "found not a trace of volcanic action, nor the smallest proof of volcanic origin. So I ventured publicly to assert and prove that all basalts could certainly not be of volcanic origin, and that to these non-volcanic rocks the Stolpen mass undoubtedly belongs. Though at first I met with much opposition, yet soon several geognosts came over to my views. These views gained special importance from the observations which I made in 1777 on the old subterranean fire in the coal-field that lies around the hills of basalt and porphyry-slate in the middle of Bohemia, and the consequent pseudo-volcanic hills that have arisen there. After further more matured research and consideration, I hold that no basalt is volcanic, but that all these rocks, as well as the other Primitive and Floetz rocks, are of aqueous origin."[1] Thus ten years of reflection had only served to make him more positive in maintaining an opinion which the most ordinary observation in his own Saxony ought to have enabled him to disprove and reject. He had not only asserted that basalt is a chemical precipitate, but had placed it among his primitive rocks.

When we remember the long and patient labours of Desmarest before he announced his conclusions regarding the volcanic origin of basalt, we cannot but wonder at the

[1] *Kurze Klassification und Beschreibung der Verschiedenen Gebirgsarten*, 1787, p. 25. Later in the same year (1787) he visited a little eminence near Scheibenberg in the Erzgebirge, and found there a cake of basalt lying on clay and sand, and thought he could trace these materials passing into each other. Whereupon he announced as a "new discovery" that all basalt is of aqueous origin, and constitutes, with clay, sand and wacke, one single formation which originally extended far and wide over the primitive and floetz rocks, but has in course of time been worn away, leaving only cappings on the hills.—Keferstein, *Geschichte der Geognosie*, p. 69.

audacity of Werner in discarding these conclusions without comment, and announcing an entirely opposite opinion, rapidly formed on the slender evidence of one or two isolated patches of basalt. It was not as if he claimed to apply his explanation merely to those few cases which he had himself examined; he swept all the basalts of the earth's surface into his net. His view had not even the merit of originality, for, as we have seen, Guettard, among others, had held the opinion that basalt is of aqueous origin. But, announced as a new discovery, with all the authority of the great Freiberg professor, it commanded attention and met with wide acceptance. Even from the time of its promulgation, however, it awakened some opposition, and it became the subject of bitter controversy for fully a generation. Only a month after Werner proclaimed his discovery he was answered by J. K. W. Voigt of Weimar, who maintained the volcanic nature of the very examples cited by the professor.[1] Werner replied, and was again answered, but soon retired from the combat and devoted his energies to strengthen his theory. As an instance of the wide interest taken in the question, I may mention that even at Berne, where there are no basalts, nor any other traces of volcanic action, the Society of Naturalists of that town offered a prize of twenty-five thalers for the best essay in answer to the question, "What is Basalt: Is it volcanic or is it not?" The successful competitor, after elaborately reviewing all the arguments brought forward by the vulcanists, pronounced in favour of Werner's views.[2]

[1] *Bergmann. Journ.* 1788, 1789, 1791, pp. 185, 347, etc. See also Hoffmann's *Geschichte der Geognosie* (1838), p. 117.

[2] J. F. W. Widenmann, Höpfner's *Magazin für die Erdkunde*, iv. (1789), p. 135.

Werner himself made two contributions to the discussion, one giving his theory of volcanoes,[1] and the other his matured views upon basalt.[2]

Volcanoes and volcanic action, if they were regarded as betokening any potent kind of reaction between the interior and the exterior of our planet, were utterly antagonistic to Werner's conception of the structure and history of the earth. In a world which had entirely resulted from the precipitations and depositions of an ocean of water, there was obviously no place for internal fire. In the system which Werner had so laboriously devised, it was imperatively necessary to treat volcanoes as modern and accidental phenomena, which never entered into the process of the formation of the crust of the earth. Accordingly, in his earliest sketch of his classification of rocks, he placed volcanic rocks among the latest of the whole series. And this view he maintained to the last. That volcanic action had been in progress from the very beginning of geological time, and that it had played an important part in building up the framework of the land in many countries all over the globe, were ideas that seem never to have occurred to him.

We have seen how old was the notion that volcanoes, or "burning mountains," arose from the combustion of subterranean beds of coal. Werner adopted this opinion, which suited his system, and was quite in congenial surroundings there. In 1789, two years after the appearance of his little *Kurze Klassification,* he definitely announced, in one of the papers above referred to, what

[1] Höpfner, *Magazin für die Erdkunde*, iv. (1789), p. 239.
[2] *Bergmännishes Journal,* 1789, i. p. 252. See also p. 272.

he called the "highly probable conjecture that most, if not all, volcanoes arise from the combustion of underground seams of coal."[1] The coal might be set on fire by spontaneous combustion, and the most vigorous volcanoes would be those starting on the thickest masses of coal. In order to support this belief, it was necessary to furnish evidence of the existence of deposits of coal around volcanoes. And much research and ingenuity were displayed in collecting all the known examples. Not only coal, but every kind of natural inflammable substance was pressed into service, and made to do duty as fuel for the subterranean fires.

It was also obviously needful to maintain that volcanoes must be comparatively modern phenomena. We are told that "it was only after the deposition of the immense repositories of inflammable matter in the Floetz-trap that volcanoes could take place; they are therefore to be considered as new occurrences in the history of nature. The volcanic state appears to be foreign to the earth."[2]

The similarity of basalt to many undoubtedly volcanic rocks had long been noticed, and could not escape the observant eyes of Werner. But he did not therefore infer basalt to be of volcanic origin. He had already established, as one of the indisputable canons of geognosy, that basalt was precipitated from chemical solution in a universal ocean. The way in which he accounted for the resemblance between basalt and lava must be regarded as a signal proof of his ingenuity. He announced that volcanoes not only occur where there are seams of coal, but

[1] See the paper just cited in Höpfner's *Magaz.* iv. (1789), p. 240.

[2] Jameson's *Geognosy*, p. 96.

where these are covered by sheets of basalt and wacke, and that eruptions of lava take place when these overlying rocks are melted by the combustion of the coal. He thus provided himself with a triumphant answer to any objector who felt inclined to question his dictum as to the origin of basalt. If the rock occurred on isolated hill tops, it was a member of the Floetz-trap formation produced by universal chemical precipitation. If it was found in the condition of lava, the original precipitate had been fused by the burning of underlying seams of coal.

With so flexible a theory to defend and apply, it can be understood how the pupils of the Freiberg school scouted the idea that volcanoes were of any real geognostical importance, and how they had a ready answer to any opponent, or a prompt explanation of any apparent difficulty in the acceptance of their master's teaching. If any one claimed that basalt was of volcanic origin, he was at once confidently assured that this was an entire mistake, for the great law-giver of Freiberg had pronounced it to be a chemical precipitate from water. If he ventured to quote the columnar structure as in favour of his view, he was told that he ought to know that lava never assumed this structure,[1] and that "rocks which have been formed or altered by the action of heat are most distinctly different from those that constitute the great mass of the crust of the globe."[2] If he brought to the unabashed Wernerian a piece of obsidian, and asked whether such a rock should not be admitted to be a volcanic glass, "Nothing of the kind," would have been, in effect, the immediate reply. "It is true that the rock does resemble 'completely melted

[1] Jameson, *op. cit.* p. 58. [2] *Op. cit.* p. 74.

stony substances, and occurs in volcanic countries,' but the notion that it is itself of volcanic origin is quite unfounded, because obsidian has never been observed accompanying lava, because it is connected with basalt, and because it contains a considerable portion of water of composition, which is never the case with true volcanic rocks."[1] If the questioner, still unconvinced, ventured to present a piece of pumice, and point to its froth-like structure and its presence in volcanic countries as evidence of its former fusion, the answer would have been an equally prompt and decided negative. Let me quote the actual words of a Wernerian in reply. "It was formerly the general opinion that pumice was a volcanic product, because it frequently occurs in countries conjectured to be of igneous formation. It is now ascertained to be an aquatic product, from the following facts: 1, It alternates with Neptunian rocks, as basalt and porphyry; 2, it is most distinctly stratified; 3, it passes into obsidian and pearlstone, and is thus connected with basalt, pitchstone, etc.; 4, it contains water of composition, which is never the case with true volcanic rocks; 5, it has never been observed to flow in streams from the crater or sides of a volcano, and no one ever saw it forming a stream in countries containing extinct volcanoes."[2]

Well might the inquirer retire in despair from such an encounter. In vain would he have sought an explanation of the origin of the vesicular structure of the rock, or have asked how this structure could ever have originated from an aqueous solution. He would probably have been plied with a few more "facts" of equal veracity, and a few

[1] See Jameson, *op. cit.* p. 196. [2] *Ibid.*

more examples of reasoning in a circle. But he would never succeed in extracting an expression of doubt, or an admission that the *ipse dixit* of the Freiberg professor could for a moment be called in question.

The same attitude which Werner assumed towards volcanoes was consistently maintained by him in his treatment of the proofs of disturbances in the terrestrial crust. He seems never to have realized that any reservoir of energy is stored up in the interior of our globe. It was part of his teaching that the spheroidal form of the planet furnished one of the proofs of a primeval universal ocean. He admitted that the crust had been abundantly cracked, but in these cracks he saw no evidence of any subterranean action. His own statement of his views on this subject is sufficiently explicit, and I quote his words: "When the mass of materials of which the rocks were formed by precipitation in the humid way, and which was at first soft and movable, began to sink and dry, fissures must of necessity have been formed, chiefly in those places where the greatest quantity of matter has been heaped up, or where the accumulation of it has formed those elevations which are called mountains."[1] He gave no explanation of the reason why the precipitates of his universal ocean should have gathered more thickly on one part of the bottom than on another. It was enough for himself and his disciples that he was convinced of the fact.

As all rents in the earth's crust were thus mere superficial phenomena resulting from desiccation and the slipping down of material from the sides of mountains, so it was conceived by Werner that, when they were filled up,

[1] *Theory of Veins*, § 39.

the mineral matter that was introduced into them could only come from above. He drew no distinction in this respect between what are now called "mineral veins" and "intrusive veins." Veins of granite, of basalt, of porphyry, of quartz, of galena, or of pyrites were all equally chemical precipitates from an overlying sea. He does not appear to have seen any difficulty in understanding how the desiccation and rupture of the rocks were to take place, if the sea still covered them, or how, if they were exposed to the air and evaporation, he was to raise the level of the ocean again so as to cover them, and fill up their rents with new precipitates.

Werner's original scheme of classification of the rocks of the earth's crust had at least the merit of clearness and simplicity. Though he borrowed his order of sequence partly from Lehmann and Fuchsel, he worked it into a scheme of his own regarding the origin of the rocks and their successive production from a universal ocean. Tracing in the arrangement of the rocks of the earth's crust the history of an original oceanic envelope, finding in the masses of granite, gneiss, and mica-schist the earliest precipitations from that ocean, and recognizing the successive alterations in the constitution of the water as witnessed by the series of geological formations, Werner launched upon the world a bold conception which might well fascinate many a listener to whom the laws of chemistry and physics, even as then understood, were but little known. Unfortunately the conception was based entirely on the imagination, and had no real foundation in observation or experiment.

Werner adopted the leading ideas of his system in an early part of his career when his personal experience was extremely limited. And having once adopted them, he maintained them to the last. His methodical mind demanded some hypothesis that would allow him to group, in definite and genetic connection, all the facts then known regarding the structure of the earth's crust. His first sketch of a classification of rocks shows by its meagreness how slender at that time was his practical acquaintance with rocks in the field. The whole of the Primitive formations enumerated by him are only twelve in number, and some of these were confessedly rare. As years went on, he intercalated new varieties, introduced the division of Transition rocks, and was compelled to reduplicate some of his primitive formations by having to find places also for them among the Floetz series.

Yet with all these shiftings to and fro the apparent symmetry and conspicuous method of the system were retained to the end. No Saxon mine could have had its successive levels more regularly planned and driven, than the crust of the earth was parcelled out among the various Wernerian universal formations. Each of these had its definite chronological place. When you stood on granite, you knew you were at the base and root of all things mundane. When you looked on a hill of Floetz-trap you saw before you a relic of one of the last acts of precipitation of the ancient universal ocean.

But Nature has not arranged her materials with the artificial and doctrinaire precision of a mineralogical cabinet. Werner's system might temporarily suffice for the little part of the little kingdom of Saxony which, when he promulgated his views, he had imperfectly explored. But as his experience widened and new facts accumulated, the modifications to which I have referred were so serious that they might well make the author of the system pause, and raise in his mind some doubts whether the fundamental conception on which the system was based could possibly be true.[1] It was eventually found, for instance, that some granite overlies instead of underlying the slates of the Primitive series; that some greenstones, instead of occurring among the Primitive rocks, lie in the Floetz division; that there are ever so many horizons for porphyry, which was at first believed to be entirely Primitive. These contradictions were surmounted by affixing such adjectives as "oldest" or "newest" to the several appearances of the same rock, or by numbering them according to their various horizons. Thus there were oldest and newest granites, oldest and newer serpentine, and first, second, and third porphyry formations.

This patching up of the system may have saved it in appearance, but a moment's reflection will show us that it was fatal to Werner's fundamental doctrine of a series of successive chemical precipitates from a universal ocean, which by the deposition of these precipitates was gradually altering its constitution. The modifications rendered necessary by fresh discovery proved that the supposed definite sequence did not exist. In fact, as was well said by a critic at the time, they were mere "subterfuges by

[1] D'Aubuisson, a loyal and favoured pupil of Werner, remarks that "Werner has continued from year to year to modify, and even to recast, some parts of his doctrine, while his disciples, following his teaching, in proportion as their observations have multiplied, have added, and are continually adding new improvements to his system."—*Traité de Géognosie* (1819), preface, p. xvi.

which the force of facts was evaded."[1] They were devised for the purpose of bolstering up a system which was entirely artificial, and to the erroneousness of which new facts were continually bearing witness.

It was claimed for Werner that he first established the doctrine of geological succession in the earth's crust. We have seen that the idea was already supplied to him by Lehmann and Fuchsel, and it is now evident that, by working into it his notion of universal aqueous precipitates, he introduced an element of hypothesis which threw back for some years the progress of sound geology. What was true in the doctrine was borrowed from his predecessors, what was his own consisted largely of unwarranted assumption. He undoubtedly did enormous service by his precise definitions and descriptions of rocks, and by dwelling on the fact that there was an observable order of succession among them, even though he mistook this order in some important particulars, and entirely misinterpreted its meaning and history. The full significance of geological succession was not understood until it was worked out independently in England and France by a rigid collection of facts, as I shall describe in a later lecture.

It was the exigencies of Saxon mining industry that started the Mining School of Freiberg. The teaching there had necessarily constant reference to the underground operations of the district. Much of Werner's practical acquaintance with the relations and structure of rock-masses was derived from what he learnt at the mines. It was only natural, therefore, that he should have inculcated upon his pupils the vast importance of subterranean

[1] *Edinburgh Review*, vol. xviii. (1811), p. 95.

exploration in unravelling the structure of the earth. The devout Wernerian put mines before mountains as a field for geological investigation.[1] Indeed the whole system of the Freiberg school, with its limited knowledge, its partial view of things, its dogmatism and its bondage to preconceived theory, is suggestive rather of the dim lamp-light and confined outlook of a mine than of constant and unfettered contact with the fresh and open face of nature.

These characteristics of Werner's teaching were keenly felt by some of the more clear-sighted of his contemporaries, who, though they recognized his genius and the vast services he had rendered to mineralogy by solid achievement, as well as by the enthusiasm he had excited in many hundreds of pupils, yet felt that in regard to geological progress his influence had become retrogressive and obstructive. This judgment was forcibly expressed in the article which appeared in the *Edinburgh Review* in the year 1811 from which I have already quoted. I have reason to believe that this article was from the pen of Dr. W. H. Fitton, who afterwards became one of the leaders of English geology. A few sentences from it may find a fitting place on the present occasion.

"The Wernerian school obstructs the progress of discovery. The manner in which it does so is plain. By supposing the order already fixed and determined when it is really not, further inquiry is prevented, and propositions are taken for granted on the strength of a theoretical principle, that require to be ascertained by actual observation. It has happened to the Wernerian

[1] See, for example, Jameson, *op. cit.* p. 43.

system, as it has to many other improvements; they were at first inventions of great utility; but being carried beyond the point to which truth and matter of fact could bear them out, they have become obstructions to all further advancement, and have ended with retarding the progress which they began with accelerating. This is so much the case in the instance before us, that when a Wernerian geognost, at present, enters on the examination of a country, he is chiefly employed in placing the phenomena he observes in the situations which his master has assigned to them in his plan of the mineral kingdom. It is not so much to describe the strata as they are, and to compare them with rocks of the same character in other countries, as to decide whether they belong to this or that series of depositions, supposed once to have taken place over the whole earth; whether, for example, they be of the Independent Coal or the Newest Floetz-trap formation, or such like. Thus it is to ascertain their place in an ideal world, or in that list of successive formations which have nothing but the most hypothetical existence:—it is to this object, unfortunately for true science, that the business of mineralogical observation has of late been reduced."[1]

Werner's writings are so few and slight that his disciples and admirers continually expressed their sorrow that he would leave so little behind him save his world-wide fame. His natural dislike of the pen increased with his years. He would discourse eloquently on many subjects, but could never bring himself to write fully on any one. Usually when he went to lecture he would retire for a

[1] *Edin. Review*, vol. xviii. (1811), art. 3, pp. 96, 97.

quarter of an hour to arrange his ideas, and when he appeared before his audience he brought with him only some scraps of paper, with a few words scribbled on them. He never wrote a single lecture. By degrees he ceased to write letters, even when the dearest friend begged for a reply, and to save himself from the reproach of this neglect, he came at last never to open the letters which he received. Cuvier tells how once an author, desiring to consult some of the learned men of the day concerning a work which he proposed to publish, circulated his voluminous manuscript among them. The precious parcel disappeared in the circuit. After endless seeking, it was disinterred in Werner's room from underneath some hundreds of others. He never answered the Academy of Sciences of Paris when it conferred on him the very high distinction of electing him one of its eight foreign associates, and he might never have heard of the affair had he not come across the mention of it in some almanack. "But," says Cuvier, "we forgave him when we heard that about the same time a messenger sent express by his sister from Dresden had been kept waiting, at the professor's expense, for two months for a mere signature to some pressing family document."

Werner's life passed placidly in the midst of the work which he loved and the pupils and friends who looked up to him with veneration and affection. His health was never robust, and the effort of lecturing proved sometimes a great strain upon his energy. After a discourse in which he would pour forth his ideas with the full flow of his exuberance, the bodily and mental effort would be so great that he would have to change his clothes even to his inner raiment. He tried to preserve both body and mind in an equable frame. Among his little foibles was the care he took never to expose himself to a draught. He kept himself out of controversy, and eventually refrained even from reading the journals, and from knowing what was said in the outer world about himself and his opinions. In this tranquil life he might perhaps have prolonged his days, had not his feelings been deeply stirred by the misfortunes which, during the Napoleonic wars, had befallen Saxony, his adopted home. He took these trials so much to heart that they led to a series of internal complications, from which he died at Dresden, in the arms of his sister, on 30th June 1817, in the sixty-eighth year of his age.

With all his efforts after the placid life of a philosopher, there was one subject that not unnaturally stirred Werner's wrath—the unwarranted publication, or at least circulation of his lectures and theories. As he did not publish them himself, and as there was a widespread desire to become acquainted with them, MS. copies of notes of his lectures were widely circulated, as a kind of mercantile speculation. This was bad enough, but he heard of an intention to print and publish them. So he took an opportunity of cautioning the world that, while willing to shut his eyes on the past, he could not tolerate any such conduct in future, that he was himself engaged in revising his works on the several branches of science he professed, and that they would "forthwith appear one after another, enriched by his latest observations and discoveries."[1] But the revision was never made, and the publications never appeared.

[1] *New Theory of the Formation of Veins*, 1791, preface.

Whether the regrets loudly expressed by his contemporaries that he published so little were justified, may perhaps be open to doubt. If Werner's fame had rested on his written works, or even on his teaching as expounded by his pupils, it could never have grown so great, nor, judging from what we know of his views in maturer life, can we suppose that any account of them by himself would really have added to his reputation, or have contributed materially to the advancement of science. It was not his writings, nor even his opinions and theories, that gave him his unquestioned authority among the geologists of his time. His influence and fame sprang from the personality of the man. His unwearied enthusiasm and eager zeal in the furtherance of his favourite studies, his kindness and helpfulness, his wide range of knowledge, and the vivacity, perspicuity, and eloquence with which he communicated it, his absolute confidence in the solidity of his theoretical doctrines—these were the sources of his power rather than the originality and importance of his own contributions to geology. His followers, indeed, captivated by the precision of his system and its apparent applicability in any and every country, claimed for him the highest place in the ranks of those who had studied the history of the earth. But the exaggeration of their claim was amply shown by the rapidity with which the Wernerian doctrines began to fall into disrepute even before the death of their author.

Among the permanent effects of Werner's teaching we must recognize one of the most important in the bias towards the mineralogical and petrographical side of geology which has ever since distinguished the German school. His theoretical views, however, retarded there the acceptance of the fuller development of the doctrine of geological succession which made such rapid progress even during his lifetime in England and in France.

But unquestionably the greatest service which Werner did to the cause of geological science was the enthusiasm he inspired for that branch of knowledge in so many capable men. "It was to his irresistible influence," as Cuvier has well remarked, "that the world owes those authors who have treated so fully of minerals, and those indefatigable observers who have so fully explored the globe. The Karstens and the Wiedemanns in the cabinet, the Humboldts, the Von Buchs, the D'Aubuissons, the Hermanns, the Freieslebens, at the summit of the Cordilleras, in the midst of the flames of Vesuvius and of Etna, in the deserts of Siberia, in the depths of the mines of Saxony, of Hungary, of Mexico, of Potosi, have been borne onward by the spirit of their master; they have brought back to him the honour gained by their labours; and we may say of him, what was never truthfully said before, save of Linnaeus, that Nature everywhere found herself interrogated in his name."

It was one of the most singular episodes in the history of geological science that the first serious check to the triumphal march of Wernerianism through Europe came from two of Werner's most distinguished pupils, D'Aubuisson and Von Buch, and that their first opposition to their master's teaching was inspired by that very volcanic tract in Central France to which Desmarest had so long before appealed in vain. Let us see how, in this instance, the whirligig of time brought in his revenges.

Jean François D'Aubuisson de Voisins (1769-1819) was born in the south of France on 16th April 1769. After receiving his early education in his own country, he spent some years as a diligent student at the Mining School of Freiberg. For four consecutive years, he tells us, he was in the most favourable circumstances for mastering the Wernerian doctrines, inasmuch as the illustrious teacher honoured him with particular attention, and in the course of many conversations unfolded to him the principles of his science, and traced for him the path that would lead him to the establishment of a true geognosy.[1] While still pursuing his studies in Saxony, D'Aubuisson took up the question of the basalts of that kingdom, travelled over all their scattered hills, and at last wrote a treatise upon them, which appeared in Paris in 1803. In this little volume of 170 pages the Wernerian doctrine as to the origin of basalt is not only accepted but treated as if it were incontestable. In one passage, indeed, the author guards himself by saying that his conclusions have reference only to the basalts which he himself has seen, and that if some day he can visit Auvergne and the Vivarais, he perhaps may be better able to discuss the question more generally, and to appreciate what has been written on the other side.[2] His essay was presented to the Institute of Sciences, and the two referees, Haüy and Ramond, to whom it was submitted, appended to their favourable report on it a most judicious piece of advice to the young author. "A subject," they say, "where the analogies already hazarded have led to more than one mistake, demands the utmost

[1] *Traité de Géognosie* (1819), vol. i. preface, p. xv.

[2] *Mémoire sur les Basaltes de la Saxe*, Paris 1803, pp. 97, 100, 101.

caution in their use, and in a field which the two parties dispute foot by foot, every step should be justified by an observation and marked by a fact. Citizen D'Aubuisson has never seen either active or extinct volcanoes. Living till now in the midst of aqueous formations, we should like him to visit places where fire has manifested its empire. We would especially desire that he should see the basalts of Auvergne, which another disciple of Werner [Leopold von Buch] has just visited. That the citizen D'Aubuisson knows how to observe, is shown by his published works, even if the memoir we have now been considering were not ample enough proof, and the interest of his observations cannot be recognized in a manner more useful to science than by encouraging him to continue them."

D'Aubuisson lost no time in following the advice thus given to him. He went to Auvergne and found the basaltic rocks there lying on granite, which in some valleys could be seen to be more than 1200 feet thick. If these rocks were lavas, they must, according to the Wernerian doctrine, have resulted from the combustion of beds of coal. But how could coal be supposed to exist under granite, which was the first chemical precipitate of a primeval ocean? Such an infra-position was inconceivable, and thus an apparent confirmation of the Freiberg view of the aqueous origin of basalt was at first obtained. But a very short time sufficed to stagger the young geologist. He saw the perfect craters with their rugged lava-streams, which he followed along their branches into the valleys. It was impossible to resist this evidence. "The facts which I saw," he says, "spoke too plainly to be mistaken;

the truth revealed itself too clearly before my eyes, so that I must either have absolutely refused the testimony of my senses in not seeing the truth, or that of my conscience in not straightway making it known. There can be no question that basalts of volcanic origin occur in Auvergne and the Vivarais. There are found in Saxony, and in basaltic districts generally, masses of rock with an exactly similar groundmass, which enclose exactly and exclusively the same crystals, and which have exactly the same structure in the field. There is not merely an analogy, but a complete similarity; and we cannot escape from the conclusion that there has also been an entire identity in formation and origin."[1]

The frank and courageous Wernerian read his recantation before the Institute of France the year after his work on the Saxon basalts appeared.[2] Still retaining his profound admiration for Werner, he nevertheless relinquished one after another the peculiar tenets of the Freiberg school, and became so impartial a chronicler of geological progress, that in his remarkably able treatise on geology, though inclining, on the whole, to his master's system, he did not entirely adopt it, but presented his facts and inferences in such a manner that, as he himself claimed, even a follower

[1] *Géognosie*, vol. ii. pp. 603, 605.

[2] "Sur les volcans et les basaltes de l'Auvergne," read to the Institute of Sciences in 1804; *Journ. de Physique*, tom. lviii. p. 427, lix. p. 367, lxxxviii. (1819), p. 432; *Soc. Philom. Bull. Paris*, 1804, p. 182. It is an indication of the slowness of the transmission of scientific news in those days that in the English translation of D'Aubuisson's *Basalts of Saxony*, which appeared at Edinburgh in 1814—that is, eleven years after the original—the translator states that he had heard of the author's having modified his views regarding the basalts of Auvergne, but that he was not aware that he had expressed any change of opinion in respect of those of Saxony.

of Hutton would hardly find a few paragraphs which he would wish to modify. D'Aubuisson lived into his seventy-third year, and died in 1819.

We turn now to the story of Leopold von Buch (1774-1853), the most illustrious geologist that Germany has produced. He came of a good family, which as far back as the twelfth century held an important position in the district of Altmark. His father, an ambassador in the Prussian service, had a family of six sons and seven daughters. Leopold, the sixth son, born on 25th April 1774, passed through a short course of mineralogical and chemical teaching at Berlin, and then went to Freiberg at the age of sixteen, to place himself under the guidance of Werner. He lived mostly under that great teacher's roof for three years, having for part of the time as his companion Alexander von Humboldt, with whom he then began a lifelong friendship. From Freiberg, where he drew in the pure Wernerian inspiration, he proceeded to the University of Halle, and later to that of Göttingen. For a brief period he held an appointment in the mining department of Silesia, but he soon abandoned the trammels of official employment, and having a sufficient competence for life, dedicated himself heart and soul to independent geological research. He was by far the most eminent of all the band of active propagandists who, issuing from Freiberg, spread themselves over Europe to illumine the benighted natives with the true light of Wernerianism.

Von Buch's earlier writings were conceived after the strictest rules of his master's system. In his first separate work, a mineralogical description of Landeck, he proclaimed, among other orthodox tenets of the Freiberg school, his adhesion to the aqueous origin of basalt, collected all the instances he could find of organic remains in that rock, and boldly affirmed that "it cannot be denied that Neptunism opens up to the spirit of observation a far wider field than does the volcanic theory."[1]

In the year 1797 Von Buch had his first view of the Alps, and in the following year began his more distant journeys, passing into Austria, and thence into Italy, where he spent a considerable time among the volcanic districts. In 1802 he published the first of two volumes descriptive of these early travels. It was appropriately dedicated to Werner, and expressed his continued adhesion to the Wernerian faith. "Every country and every district," he remarks, "where basalt is found furnishes evidence directly opposed to all idea that this remarkable rock has been erupted in a molten condition, or still more that each basalt hill marks the site of a volcano."[2] Before the second volume appeared, the writer of that sentence had an opportunity of visiting Auvergne. His conversion there appears to have been as rapid as that of D'Aubuisson, but his announcement of it was much more sensational. It was in the spring of 1802 that he went to Central France, but owing to various accidents the second volume of his travels did not appear until the year 1809.[3] He had made

[1] *Gesammelte Schriften*, vol. i. p. 63.

[2] *Geognostische Beobachtungen auf Reisen durch Deutschland und Italien*, Berlin, i. (1802), p. 126. It is a curious fact that A. von Humboldt also began his geological career among the basalts of Germany, and published in 1790 a little tract of 126 pages, entitled *Mineralogische Beobachtungen über einige Basalte am Rhein.*

[3] The descriptions of Auvergne are contained in an Appendix to vol. ii., consisting of *Mineralogische Briefe aus Auvergne an Herrn Geh. Ober-Bergrath Karsten*, p. 227 (1809).

no secret, however, of his change of opinion, for in the winter following his French tour, a letter from him was published, recommending a geologist who wanted to see volcanoes to choose Auvergne rather than Vesuvius or Etna.[1] His views were thus well known to Haüy and Ramond when they recommended D'Aubuisson to betake himself to the same volcanic region.

When his fuller account of his rambles in Auvergne appeared, its very first sentence betrayed a curious ignorance or forgetfulness of the literature of the subject. "Here we are," he says, "in a region about which the naturalists of France have talked so much, to which they have persistently referred us, but which they have never yet described to us." It is difficult to believe that Von Buch had never seen Desmarest's papers and accompanying maps. Yet throughout the whole account which he gives of his excursions he does not once refer to them, but writes as if he were almost the first geologist who had ever made any detailed and exact observations in the country.[2]

Nothing could be more explicit than Von Buch's testimony to the volcanic origin of the basalts of Auvergne. The marvellous cone and crater of the Puy de Pariou excited, as they well might, his astonishment and admiration.

[1] *Journal des Mines*, vol. xiii. 1802-1803, p. 249. Boué, in an obituary notice of Von Buch, says picturesquely that "in the year 1798 the learned geognost left Germany a Neptunist and came home in 1800 a Vulcanist." His conversion, though as complete, was not quite so rapid, for even after his visit to Italy and Central France, though he gave up some parts of the Wernerian system, he still clung tenaciously to others which he afterwards abandoned.

[2] He refers indeed several times to Montlosier's *Essai sur les volcans de l'Auvergne*, which he calls an excellent work. In one passage he actually credits this author with some of the most important generalizations made by Desmarest. See pp. 279, 280.

"Here," he says," we find a veritable model of the form and degradation of a volcano, such as cannot be found so clearly either at Etna or Vesuvius. Here at a glance we see how the lava has opened a way for itself at the foot of the volcano, how with its rough surface it has rushed down to the lower grounds, how the cone has been built above it out of loose slags which the volcano has ejected from its large central crater. We *infer* all this also at Vesuvius, but we do not always *see* it there as we do at the Puy de Pariou."[1]

Perhaps the most interesting passages in Von Buch's brightly-written letters are to be found at the end. The obviously volcanic origin of the rocks in Auvergne, and their position immediately above a mass of granite through which the craters had been opened, had evidently powerfully impressed his mind. With all these recent vivid experiences he reflects upon his earlier wanderings among the basalt hills of Germany, and, as if taking his readers into his inner confidence, he declares that "it is impossible to believe in a particular or local formation of basalt, or in its flowing out as lava, when we know what the relations of this rock are in Germany, and when we remember how many different kinds of rocks are there associated with basalt as essential accompaniments, how these rocks form with basalt a connected whole which is absolutely inconsistent with any notion of volcanic action—a peculiar coal-formation, entirely distinct from any other, only found with basalt and entirely enclosed among basaltic rocks, often even a peculiar formation of limestone."[2]

This was the one side of the picture. He could not yet break entirely the Wernerian bonds that held him to the

[1] *Op. cit.* p. 240. [2] *Op. cit.* p. 309.

beliefs he had imbibed at Freiberg. He could not bring himself to admit that all that his master had taught him as to the origin of basalt, all that he had himself so carefully noted down from his extended journeys in Germany, was radically wrong. He, no doubt, felt that it was not merely a question of the mode of origin of a single kind of stone. The whole doctrine of the chemical precipitation of the rocks of the earth's crust was at stake. If he surrendered it at one point, where was he to stop? We cannot wonder, therefore, that he still refused to permit himself to question the truth of the Wernerian faith in so far as the old basalts of Saxony and Silesia were concerned. He comforted himself with the belief that they at least, with all their associated sedimentary strata, must have been deposited by water.

But when he turns round again to the clear evidence displayed in Central France, he asks, "Is it the fault of the geologist in Auvergne that the arguments which are powerful in Germany have no effect on him here, even though he does not dispute them? May he not be allowed in retort to ask whether the principles which so obviously arise from the phenomena in Central France are not also applicable to the German basalts? At all events, he may contend, we see very little connection between these basalts and ours as regards relations of structure. Would you have us give up our convictions as to the principles which give grandeur, consistency, and simplicity to the explanation of our Auvergne mountains, and adopt views founded on relations which are not to be seen here?"[1]

Well might Von Buch conclude by saying that he

[1] *Op. cit.* p. 310.

"stands perplexed and embarrassed." Whatever he may think of the basalts of Auvergne, he will not allow the Vulcanist to wrest his admissions to any general conclusion with regard to the German basalts. "Opinions are in opposition which only new observations can remove."

Von Buch's faith in the Wernerian interpretation of volcanoes and basalt-hills had a rude shaking from his excursions in Italy and Central France. His next great journey taught him that Werner's scheme of geological succession could not be maintained. Before his volume descriptive of the Italian tour was published, he had started for Norway, where he remained hard at work for no less than two years. Among the vast mass of important observations which he made, one that must have greatly impressed him was that in which he satisfied himself that the rocks in the Christiania district could not be arranged according to the Wernerian plan. His master's scheme of succession completely broke down. Von Buch found a mass of granite lying among fossiliferous limestones which were manifestly metamorphosed, and were pierced by veins of granite, porphyry, and syenite. Such observations did not lead him, any more than those in Central France, to a formal renunciation of Wernerianism. But they enabled him to take a wide and independent view of nature, and gradually to emancipate himself from the narrower views in which he had been trained at Freiberg.[1]

Von Buch's memorable investigation of the proofs of the recent uprise of Scandinavia contributed still further

[1] See his "Reise nach Norwegen und Lappland," *Gesammelte Schriften*, vol. ii. p. 109.

to expand his geological horizon. When he announced that the whole of the continent of Sweden from Frederikshald to Abo is now slowly rising above the sea, he did as much as any Vulcanist of his day in support of the Huttonian theory.

A further emancipation from the tenets of Freiberg was displayed by a series of papers on the mountain-system of Germany, wherein Von Buch gave the first clear description of the geological structure of Central Europe. He declared that the more elevated mountains had never been covered by the sea, as Werner had taught, but were produced by successive ruptures and uplifts of the terrestrial crust. In 1824 he produced a geological map of the whole of Germany in forty-two sheets, the first large map of its kind to illustrate a great area of the European continent, and a signal monument of its author's unwearied research and of his geological acumen. For more than sixty years this distinguished man continued to enrich geological literature with memoirs contributed to scientific societies and journals, and with independent works. His earliest writings stamped him as an observer of great sagacity and independence, and his reputation rose higher every year, until he came to be the acknowledged leader of geological science in Germany. Pressing forward into every department of the science, he illuminated it with the light of his penetrating intellect. From the North Cape to the Canary Islands there was hardly a region that he did not personally explore, and not many that he did not describe. With ceaseless industry and exhaustless versatility, he ranged from the structure of the Alps to that of the Cystideans, from the distribution of volcanoes to that of Ammonites, from the details of minerals and rocks to the deepest problems in the history of the globe.[1]

His influence in his time was great. Though he began as a Wernerian, he gradually and almost unconsciously passed into the ranks of the vulcanists. In no respect did he show his independence and love of truth more than in his long and enthusiastic researches among volcanoes. No Vulcanist could have worked out more successfully than he did the structure and history of the Canary Islands.

Among the leaders of geology in the first half of this century there was no figure more familiar all over Europe than that of Von Buch. Living as a bachelor, with no ties of home to restrain him, he would start off from Berlin, make an excursion to perhaps a distant district or foreign country, for the determination of some geological point that interested him, and return, without his friends knowing anything of his movements. He made most of his journeys on foot, and must have been a picturesque object as he trudged along, stick in hand. He wore knee-breeches and shoes, and the huge pockets of his overcoat were usually crammed with note-books, maps, and geological implements. His luggage, even when he came as far as England, consisted only of a small baize bag, which held a clean shirt and silk stockings. Few would have supposed that the odd personage thus accoutred was one of the greatest men of science of his time, an honoured

[1] Von Buch's collected writings form four large closely-printed octavo volumes. The Royal Society's Catalogue assigns 153 separate papers to him. For a biographical account of Von Buch see the sketch by W. Haidinger in *Jahrb. k. k. geol. Reichsanst.* Band iv. (1853), p. 207, and the notices prefixed to his collected works.

and welcome guest in every learned society of Europe. He was not only familiar with the writings of the geologists of his day, but knew the men personally, visited them in their own countries, and with many of them kept up a friendly and lively correspondence. He had an extensive knowledge of the languages of Europe, and had read widely not only in his own subjects, but in allied sciences, in history, and in literature, ancient and modern. Kindly, frank, outspoken, and fearless, he was beloved and honoured by those who deserved his friendship, and dreaded by those who did not. With tender self-sacrifice he would take his blind brother every year to Carlsbad, and with endless benefactions did he brighten the lives of many who survived to mourn his loss. He died on 4th March 1853, in the seventy-ninth year of his age. A fitting monument to his memory was raised by subscriptions from all over Europe. In the picturesque region of Upper Austria, not far from Steyer, a granite boulder 16 feet high that had been borne by a former glacier from the Alps was chosen as his cenotaph. The stone, chiselled into a flat surface, bears inscribed upon it, with the reverence of admirers in Germany, Belgium, France, England, and Italy, the immortal name of Leopold von Buch.[1]

[1] An account of the movement for the preparation of this monument will be found in *Das Buch-Denkmal*, a pamphlet by Ritter von Hauer and Dr. Hörnes, published in Vienna in 1858. It gives a portrait of Von Buch, and a view of the monument, with a map showing the position of the site.

LECTURE IV

Rise of the modern conception of the theory of the earth—Hutton, Playfair—Birth of experimental geology—Sir James Hall.

WHILE the din of geological warfare resounded across Europe, and the followers of Werner, flaunting the Neptunist flag in every corner of the continent, had succeeded in making the system promulgated from Freiberg almost supplant every other, a series of quiet and desultory researches was in progress, which led to the establishment of the fundamental principles of modern geology. We have now to turn our eyes to the British Isles, and to trace the career of a man who, with singular sagacity, recognizing early in life the essential processes of geological change, devoted himself with unwearied application to the task of watching their effects and collecting proofs of their operation, and who combined the results of his observations and reasoning in a work which will ever remain one of the great classics of science. In following the course of his researches, we shall see another illustration of the influence of environment on mental tendencies, and mark how the sea-shores and mountains, glens and lowlands of Scotland have given tone and colour to the development of geological theory.

James Hutton (1726-1797) was born in Edinburgh on the 3rd June 1726, and was educated at the High School and University of that city.[1] His father was a worthy citizen there, who had held the office of City Treasurer, but died while the son was still very young, to whom he left a small landed property in Berwickshire. While attending the logic lectures at the University, Hutton's attention was arrested by a reference to the fact that, although a single acid suffices to dissolve the baser metals, two acids must combine their strength to effect the solution of gold. The professor, who had only used this illustration in unfolding some general doctrine, may or may not have made his pupil a good logician, but he certainly made him a chemist, for from that time the young student was drawn to chemistry by a force that only became stronger as years went on. When at seventeen years of age he had to select his profession in life, he was placed as an apprentice in a lawyer's office. But genius is irrepressible, and amid the drudgery of the law the young clerk's chemistry not infrequently came to the surface. He would be found amusing himself and his fellow-apprentices with chemical experiments, when he should have been copying papers or studying legal proceedings, so that finally his master, seeing that law was evidently not his bent, released him from his engagement, and advised him to seek some other employment more suited to his turn of mind.

Hutton accordingly, after a year's drudgery at law, made choice of medicine as the profession most nearly

[1] For the biographical details in this sketch I am indebted to the admirable "Biographical Account of Dr. James Hutton" by his friend and illustrator, Playfair. This was first printed in the *Transactions of the Royal Society of Edinburgh*, and will be found in vol. iv. of Playfair's collected works.

allied to chemistry, and most likely to allow him to indulge his predilection for science. For three years he prosecuted his medical studies at Edinburgh, and thereafter, as was then the custom, repaired to the continent to complete his professional training. He remained nearly two years in Paris, pursuing there with ardour the studies of chemistry and anatomy. Returning to Scotland by way of the Low Countries, he took the degree of Doctor of Medicine at Leyden in September 1749.

But the career of a physician seems to have grown less attractive to him as the time came on for his definitely settling in life. He may have been to some extent influenced by the success of certain chemical researches which he had years before begun with a friend of kindred tastes —researches which had led to some valuable discoveries in connection with the nature and production of sal ammoniac, and which appeared to offer a reasonable prospect of commercial success. In the end he abandoned all thought of practising as a medical man, and resolved to apply himself to farming. He was a man never disposed to do things by halves, and having made up his mind in favour of agriculture as his vocation, he determined to take advantage of the best practical instruction in the subject then available. Accordingly in 1752 he betook himself to Norfolk, lived with a Norfolk farmer, and entered with all the zest of a young man of six-and-twenty into the rural sports and little adventures which, in the intervals of labour, formed the amusement of his host and his neighbours.

It appears to have been during this sojourn in East Anglia that Hutton's mind first definitely turned to mineralogy and geology. He made many journeys on foot into different parts of England. In Norfolk itself there was much to interest his attention. Every here and there, the underlying white chalk came to the surface, with its rows of fantastically-shaped black flints. To the east, lay the Crag with its heaps of sea-shells, stretching over many miles of the interior. To the north, the sea had cut a range of cliffs in the boulder-clay which, with its masses of chalk and its foreign stones, presented endless puzzles to an inquirer. To the west, the shores of the Wash showed the well-marked strata of red chalk and carstone, emerging from underneath the white chalk of the interior.

Hutton tells, in one of his letters written from Norfolk, that he had grown fond of studying the surface of the earth, and was looking with anxious curiosity into every pit or ditch or bed of a river that fell in his way.

After spending about two years in Norfolk, he took a tour into Flanders, with the view of comparing the husbandry there with that which he had been studying in England. But his eyes were now turned to what lay beneath the crops and their soils, and he took note of the rocks and minerals of the districts through which he passed. At last, about the end of the summer of 1754, he settled down on his own paternal acres in Berwickshire, which he cultivated after the most approved methods. For fourteen years he remained immersed in rural pursuits, coming occasionally to Edinburgh and making, from time to time, an excursion to some more distant part of the kingdom. His neighbours in the country probably looked upon him only as a good farmer, with more intelligence, culture and knowledge of the world than were usual in their society, and displaying a playful humour and liveliness of manner which must have made his companionship extremely pleasant. Probably not one of the lairds and farmers in the South of Scotland, who met him at kirk and market, had the least suspicion that this agreeable neighbour of theirs was a man of surpassing genius, who at that very time, amidst all the rural pursuits in which he seemed to be absorbed, was meditating on some of the profoundest problems in the history of the earth, and was gathering materials for such a solution of these problems as had never before been attempted.

The sal ammoniac manufacture had proved successful, and from 1765 Hutton became a regular co-partner in it. His farm, now brought into excellent order, no longer afforded him the same interest and occupation, and eventually he availed himself of an opportunity of letting it to advantage. He determined about the year 1768 to give up a country life and establish himself in Edinburgh in order that, with uninterrupted leisure, he might devote himself entirely to scientific pursuits.

The Scottish capital had not yet begun seriously to suffer from the centripetal attractions of London. It was the social centre of Scotland, and retained within its walls most of the culture and intellect of that ancient kingdom. Hutton, from his early and close connection with Edinburgh, had many friends there, and, on his return for permanent residence, was received at once into the choicest society of the town. One of his most intimate associates was Dr. Joseph Black, the famous chemist to whom we owe the discovery of carbonic acid.

This sympathetic friend took the keenest interest in Hutton's geological theories, and was able to contribute to their formation and development. Hutton himself acknowledges that one of his doctrines, that of the influence of compression in modifying the action of heat, was suggested by the researches of Dr. Black. The chemist's calm judgment and extensive knowledge were always at the command of his more impulsive geological friend, and doubtless proved of essential service in guiding him in his speculations.

Another of Hutton's constant and intimate associates was John Clerk of Eldin, best known as the author of a work on naval tactics, and the inventor of the method of breaking the enemy's line at sea, which led to so many victories by the fleets of Great Britain. A third member of his social circle, who may be alluded to here, was the philosopher and historian Adam Ferguson, a man of remarkable force of character, who, to his various literary works, which were translated into French and German, added the distinction of a diplomatist, for in 1778-1779 he acted as Secretary of the Commission sent across the Atlantic by Lord North to try to arrange the matters in dispute between the mother country and her North American colonies.

When Hutton found himself in these congenial surroundings, with ample leisure at his command, he appears to have turned at once to his first love in science, by betaking himself to chemical experiment. Even without the testimony of his biographer, we have only to look at his published works to be impressed by his unwearied industry, and by the extraordinarily wide range of his studies. Though up to the time of his settling in Edinburgh he had published nothing, he had read extensively. There were hardly any of the sciences, except the mathematical, to which he did not turn his attention. He was a diligent reader of voyages, travels and books of natural history, carefully storing up the facts which seemed to him to bear on the problems of the earth's history. He not only prosecuted chemistry and mineralogy, but distinguished himself as a practical meteorologist by his important contribution to the theory of rain. He wrote a general system of physics and metaphysics in one quarto volume, and no fewer than three massive quartos were devoted by him to "An Investigation of the Principles of Knowledge, and of the Progress of Reason from Sense to Science and Philosophy." At the time of his death he was engaged upon a treatise on the "Elements of Agriculture."

Hutton was thus no narrow specialist, wrapped up in the pursuit of one circumscribed section of human inquiry. His mind ranged far and wide over all departments of knowledge. He took the keenest interest in them all, and showed the most vivid sympathy in their advancement. His pleasure in every onward step made by science and philosophy showed itself in the most lively demonstrations. "He would rejoice," we are told by Playfair, "over Watt's improvements on the steam-engine, or Cook's discoveries in the South Seas, with all the warmth of a man who was to share in the honour or the profit about to accrue from them. The fire of his expression, on such occasions, and the animation of his countenance and manner, are not to be described; they were always seen with great delight by those who could enter into his sentiments; and often with great astonishment by those who could not."

While so much was congenial to his mental habits in the friendly intercourse of Edinburgh society, there was not less in the scenery around the city that would stimulate his geological proclivities. He could not take a walk in any direction without meeting with illustrations of some of the problems for the solution of which he was seeking. If he turned eastward, Arthur Seat and Salisbury Crags rose in front of him, with their memorials of ancient volcanic eruptions. If he strolled westward, the ravines of the Water of Leith presented him with proofs of the erosive power of running water, and with sections of the successive sea-bottoms of the Carboniferous period. Even within the walls of the city, the precipitous Castle Rock bore witness to the energy with which in ancient times molten material had been thrust into the crust of the earth.

No more admirable environment could possibly have inspired a geologist than that in which Hutton now began to work more sedulously at the study of the former changes of the earth's surface. But he went far afield in search of facts, and to test his interpretation of them. He made many journeys into different parts of Scotland, where the phenomena which engaged his attention seemed most likely to be well displayed. He extended his excursions likewise into England and Wales. For about thirty years, he had never ceased to study the natural history of the globe, constantly seeking to recognize the proofs of ancient terrestrial revolutions, and to learn by what causes they had been produced. He had been led to form a definite theory or system which, by uniting and connecting the scattered facts, furnished an intelligible explanation of them. But he refrained from publishing it to the world. He had communicated his views to one or two of his friends, perhaps only to Dr. Black and Mr. Clerk, whose judgment and approval were warmly given to him. The world, however, might have had still a long time to wait for the appearance of his dissertation, had it not been for the interest that he took in the foundation of the Royal Society of Edinburgh, which was incorporated by Royal Charter in 1783.[1] At one of the early meetings

[1] The Royal Society had been preceded by the Philosophical Society, out of which it sprang. Edinburgh at that time was famous for the number of its clubs and convivial meetings, at some of which Black and Hutton were constant companions. Various anecdotes have been handed down of these two worthies and their intercourse, of which the following may suffice as a specimen. "These attached friends agreed in their opposition to the usual vulgar prejudices, and frequently discoursed together upon the absurdity of many generally received opinions, especially in regard to diet. On one occasion they had a disquisition upon the inconsistency of abstaining from feeding on the testaceous creatures of the land, while those of the sea were considered as delicacies. Snails, for instance—why not use them as articles of food? They were well known to be nutritious and wholesome—even sanative in some cases. The epicures, in olden time, esteemed as a most delicious treat the snails fed in the marble quarries of Lucca. The Italians still hold them in esteem. The two philosophers, perfectly satisfied that their countrymen were acting most absurdly in not making snails an ordinary article of food, resolved themselves to set an example; and accordingly, having procured a number, caused them to be stewed for dinner. No guests were invited to the banquet. The snails were in due season served up; but, alas! great is the difference between theory and practice—so far from exciting the appetite, the smoking dish acted in a diametrically opposite manner, and neither party felt much inclination to partake of its contents. Nevertheless, if they looked on the snails with disgust, they retained their awe for each other; so that each conceiving the symptoms of internal revolt peculiar to himself, began with infinite exertion to swallow in very small quantities the mess which he internally loathed. Dr. Black at length

of this Society he communicated a concise account of his Theory of the Earth, which appeared in the first volume of the *Transactions.* This essay was afterwards expanded, with much ampler details of observations and fuller application of principles to the elucidation of the phenomena, and the enlarged work appeared in two octavo volumes in the year 1795 with the title of *Theory of the Earth, with Proofs and Illustrations.* After Hutton's death his friend Playfair published in 1802 his classical *Illustrations of the Huttonian Theory.* We are thus in possession of ample information of the theoretical views adopted by Hutton, and of the facts on which he based them. Before considering these, however, it may be convenient to follow the recorded incidents of his quiet and uneventful life, that we may the better understand the manner in which he worked, and the nature of the material by which he tested and supported his conclusions.

It was one of the fundamental doctrines of Hutton's system that the internal heat of the globe has in past time shown its vigour by the intrusion of large masses of molten material into the crust. He found many examples of these operations on a small scale in the neighbourhood of Edinburgh and in the lowlands of Scotland. But he conceived that the same effects had been produced in a far more colossal manner by the protrusion of large bodies of granite. This rock, which Werner had so dogmatically

broke the ice, but in a delicate manner, as if to sound the opinion of his messmate, 'Doctor, do you not think that they taste a little—a very little—queer?' 'D—— queer, d—— queer, indeed; tak them awa', tak them awa'!' vociferated Dr. Hutton, starting up from table and giving full vent to his feelings of abhorrence."—*A Series of Original Portraits,* by John Kay (commonly known as Kay's *Edinburgh Portraits*), vol. i. p. 57.

affirmed to be the earliest chemical precipitate from his primeval ocean, was surmised by Hutton to be of igneous origin, and he believed that, if its junctions with the surrounding strata were examined, they would be found to furnish proofs of the correctness of his inference. The question could be easily tested in Scotland, where, both in the Highlands and among the Southern Uplands, large bodies of granite had long been known to form important groups of mountains. Accordingly, during a series of years, Hutton undertook a number of excursions into various parts of his native country, and returned from each of them laden with fresh illustrations of the truth of the conclusions at which he had arrived. At one time he was busy among the roots of the Grampian Hills, at another he was to be seen scouring the lonely moorlands of Galloway, or climbing the precipices and glens of Arran. His visit to Glen Tilt has been made memorable by Playfair's brief account of it.[1] He had conjectured that in the bed of the river Tilt actual demonstration might be found that the Highland granite has disrupted the surrounding schists. Playfair describes how "no less than six large veins of red granite were seen in the course of a mile, traversing the black micaceous schistus, and producing, by the contrast of colour, an effect that might be striking even to an unskilful observer. The sight of objects which verified at once so many important conclusions in his system, filled him with delight; and as his feelings, on such occasions, were always strongly expressed, the guides who accompanied him were convinced that it must be nothing less than the discovery of a vein of silver or gold

[1] Hutton's account is in the unpublished third volume of his *Theory*.

that could call forth such strong marks of joy and exultation."

Another of Hutton's fundamental generalizations was tested in as vivid and successful a manner. He taught that the ruins of an earlier world lay beneath the secondary strata, and that where the base of these strata can be seen, it will be found to reveal, by what is now known as an unconformability, its relation to the older rocks. He had at various points in Scotland satisfied himself by actual observation that this relation holds good. But he determined to verify it once more by examining the junction of the two groups of rock along the coast where the range of the Lammermuir Hills plunges into the sea. Accompanied by his friend Sir James Hall, whose property of Dunglass lay in the immediate neighbourhood, and by his colleague and future biographer Playfair, and favoured by calm weather, he boated along these picturesque shores until the unconformable junction was reached. The vertical Silurian shales and grits were found to protrude through and to be wrapped round by the red sandstone and breccia. "Dr. Hutton," Playfair writes, "was highly pleased with appearances that set in so clear a light the different formations of the parts which compose the exterior crust of the earth, and where all the circumstances were combined that could render the observation satisfactory and precise. On us who saw these phenomena for the first time, the impression made will not easily be forgotten. The palpable evidence presented to us of one of the most extraordinary and important facts in the natural history of the earth, gave a reality and substance to those theoretical speculations which, however probable, had never till now been directly authenticated by the testimony of the senses. We often said to ourselves, what clearer evidence could we have had of the different formation of these rocks, and of the long interval which separated their formation, had we actually seen them emerging from the bosom of the deep? . . . The mind seemed to grow giddy by looking so far into the abyss of time; and while we listened with earnestness and admiration to the philosopher who was now unfolding to us the order and series of these wonderful events, we became sensible how much further reason may sometimes go than imagination can venture to follow."

Hutton's lithe active body betokened the unwearied vigour of his mind. His high forehead, firmly moulded features, keen observant eyes, and well-shaped, rather aquiline nose, marked him out at once as a man of strong intellect, while the gentleness that beamed in his face was a reflex of the kindliness of his nature. His plain dress, all of one colour, gave a further indication of the unostentatious simplicity of his nature.

His mode of life was in harmonious keeping with these personal traits. After working in his study during the day he would invariably pass the evening with his friends. "A brighter tint of gaiety and cheerfulness spread itself over every countenance when the doctor entered the room; and the philosopher who had just descended from the sublimest speculations of metaphysics or risen from the deepest researches of geology, seated himself at the tea-table, as much disengaged from thought, as cheerful and gay, as the youngest of the company." His character was distinguished by its transparent simplicity, its frank openness, its absence of all that was little or selfish, and its overflowing enthusiasm and vivacity. In a company he was always one of the most animated speakers, his conversation full of ingenious and original observation, showing wide information, from which an excellent memory enabled him to draw endless illustrations of any subject that might be discussed, where, "when the subject admitted of it, the witty and the ludicrous never failed to occupy a considerable place."

Though his partnership in the chemical work brought him considerable wealth, it made no difference in the quiet unostentatious life of a philosopher, which he had led ever since he settled in Edinburgh. A severe attack of illness in the summer of 1793 greatly reduced his strength, and though he recovered from it and was able to resume his life of activity, a second attack of the same ailment in the winter of 1796 terminated at last fatally on the 26th March 1797, when he was in his seventy-first year.

Hutton's claim to rank high among the founders of geology rests on no wide series of writings, like those which Von Buch poured forth so copiously for more than two generations. Nor was it proclaimed by a host of devoted pupils, like those who spread abroad the fame of Werner. It is based on one single work,[1] and on the elucidations of two friends and disciples.

On the 7th of March and 4th of April 1785 there was read to the Royal Society of Edinburgh a memoir on a "Theory of the Earth; or an Investigation of the Laws observable in the Composition, Dissolution, and Restoration of Land upon the Globe." Extending to no more than 96

[1] The first sketch and the expansion of it into two volumes may be practically regarded as one work so far as the originality of conception is concerned.

quarto pages, it was written in a quiet logical manner, with no attempt at display, but with an apparent anxiety to state the author's opinions as tersely as possible. Probably no man realized then that this essay would afterwards be regarded as marking a turning-point in the history of geology. For some years it remained without attracting notice either from friend or foe.[1]

For this neglect various causes have been assigned. The title of the memoir was perhaps unfortunate. The words "Theory of the Earth" suggested another repetition of the endless speculations as to the origin of things, of which men had grown weary. System after system of this kind of speculation had been proposed, and had dropped into oblivion, and no doubt many of his contemporaries believed Hutton's "Theory" to be one of the same ill-fated brood. His friend Playfair admits that there were reasons in the construction of the memoir itself why it should not have made its way more speedily into notice. Its contents were too condensed, and contained too little explanation of the grounds of the reasoning. Its style was apt to be prolix and obscure. It appeared too in the *Transactions* of a learned Society which had only recently been founded, and whose publications were hardly yet known to the general world of science.

[1] It does not appear to be generally known that Desmarest, departing from his usual practice of not noticing the work of living writers, wrote a long and careful notice of Hutton's Memoir of 1785 in the first volume of his *Géographie Physique*, published in 1794-1795. He disagrees with many of Hutton's views, such, for instance, as that of the igneous origin of granite. But he generously insists on the value of the observations with which the Scottish writer had enriched the natural history of the earth and the physical geography of Scotland. "It is to Scotland," he says, "that Hutton's opponents must go to amend his results and substitute for them a more rational explanation" (p. 750).

At last, after an interval of some five years, De Luc assailed the "Theory" in a series of letters in the *Monthly Review* for 1790 and 1791. So far as we know, Hutton published no immediate reply to these attacks. He had often been urged by his friends to publish his entire work on the Theory of the Earth, with all the proofs and illustrations which had been accumulating in his hands for so many years. From year to year, however, he delayed the task, until during the convalescence from his first severe illness, he received a copy of a strenuous attack upon his system and its tendencies by Richard Kirwan, a well-known Irish chemist and mineralogist of that day.[1] This assailant not only misconceived and misrepresented the views which he criticized, but charged their author with atheistic opinions. Weakened as he was by illness, Hutton, with characteristic energy, the very day after he received Kirwan's paper, began the revisal of his manuscript, and worked at it until he was able to send it to the press. It appeared in 1795, that is, ten years after the first sketch of the subject was given to the Royal Society of Edinburgh. Besides embodying that sketch, it gave a much fuller statement of his views, and an ampler presentation of the facts and observations on which they were founded. It formed two octavo volumes. Playfair tells us that a third volume, necessary for the completion of the work, remained in manuscript.[2]

[1] "Examination of the Supposed Origin of Stony Substances," read to the Royal Irish Academy, 3rd February 1793, and published in vol. v. of their *Transactions*, p. 51. For a crushing exposure of Kirwan's mode of attack see Playfair's *Illustrations of the Huttonian Theory*, §§ 119, 418.

[2] A portion of this manuscript, containing six chapters (iv.-ix.), is in

If Hutton's original sketch was defective in style and arrangement, his larger work was even more unfortunate in these respects. Its prolixity deterred readers from its perusal. Yet it was a vast storehouse of accurate observation and luminous deduction, and it deserves to be carefully studied by every geologist who wishes to understand the history of his own science.

Fortunately for Hutton's fame, he numbered among his friends the illustrious mathematician and natural philosopher, John Playfair (1748-1819), who had been closely associated with him in his later years, and was intimately conversant with his geological opinions. Gifted with a clear penetrating mind, a rare faculty of orderly logical arrangement, and an English style of altogether remarkable precision and elegance, he was of all men best fitted to let the world know what it owed to Hutton. Accordingly, after his friend's death he determined to prepare a more popular and perspicuous account of Hutton's labours. He gave in this work, first a clear statement of the essential principles of Hutton's system, and then a series of notes or essays upon different parts of the system, combining in these a large amount of original observation and reflection of his own. His volume appeared in the spring of 1802, just five years after Hutton's death, with the title of *Illustrations of the Huttonian Theory of the Earth.* Of this great classic it is impossible to speak too highly. After the lapse of nearly a century it may be read with as much profit and pleasure as when it first appeared. For preci-

the possession of the Geological Society of London, but the rest seems to have disappeared. It is much to be desired that this precious fragment should be published.

sion of statement and felicity of language it has no superior in English scientific literature. To its early inspiration I owe a debt which I can never fully repay. Upon every young student of geology I would impress the advantage of reading and re-reading and reading yet again this consummate masterpiece. How different would geological literature be to-day if men had tried to think and write like Playfair!

There are thus three sources of information as to Hutton's geological system—his first sketch of 1785, his two octavo volumes of 1795 and Playfair's *Illustrations* of 1802. Let us now consider what were his fundamental doctrines.

Although he called his system a Theory of the Earth, Hutton's conceptions entirely differed from those of the older cosmologists, who thought themselves bound to begin by explaining the origin of things, and who proceeded on a foundation of hypothesis to erect a more or less fantastic edifice of mere speculation. He, on the contrary, believed that it is the duty of science to begin by ascertaining what evidence there is in the earth itself to throw light upon its history. Instead of invoking conjecture and hypothesis, he proceeded from the very outset to collect the actual facts, and to marshal these in such a way as to make them tell their own story. Unlike Werner, he had no preconceived theory about the origin of rocks, with which all the phenomena of nature had to be made to agree. His theory grew so naturally out of his observations that it involved no speculation in regard to a large part of its subject.

Hutton started with the grand conception that the past history of our globe must be explained by what can be seen to be happening now, or to have happened only recently. The dominant idea in his philosophy is that the present is the key to the past. We have grown so familiar with this idea, it enters so intimately into all our conceptions in regard to geological questions, that we do not readily realize the genius of the man who first grasped it with unerring insight, and made it the chief corner-stone of modern geology.

From the time of his youthful rambles in Norfolk, Hutton had been struck with the universal proofs that the surface of the earth has not always been as it is to-day. Everywhere below the covering of soil he found evidence of former conditions, entirely unlike those visible now. In the great majority of cases, he noticed that the rocks there to be seen consist of strata, disposed in orderly arrangement parallel with each other. Some of these strata are formed of pudding-stone, others of sandstone, of shale, of limestone, and so forth, differing in many respects from each other, but agreeing in one essential character, that they are composed of fragmentary or detrital material, derived from rocks older than themselves. He saw that these various strata could be exactly paralleled among the accumulations now taking place under the sea. The pudding-stones were in his eyes only compacted gravels, the sandstones were indurated sand, the limestones were in great part derived from the accumulation of the remains of marine calcareous organisms, the shales from the consolidation of mud and silt. The wide extent of these strata, forming, as they do, most of the dry land, seemed to him to point to the sea as the only large expanse of water in which they could have been deposited.

Thus the first conclusion established by the Scottish philosopher was that the greater part of the land consisted of compacted sediment which had been worn away from some pre-existing continent, and had been spread out in strata over the bed of the sea. He realized that the rocks thus formed were not all of the same age, but, on the contrary, bore witness to a succession of revolutions. He acknowledged the existence of a series of ancient rocks which he called Primary, not that he believed them to be the original or first-formed rocks in the structure of the planet, but that they were the oldest that had then been discovered. They included the various schists and slates which Werner claimed as chemical precipitates, but in which Hutton could only see the hardened and altered mechanical sediments of a former ocean. Above them, and partly formed out of them, came the Secondary strata that constitute the greater part of the land.

But all these sedimentary deposits have passed from their original soft condition into that of solid stone. Hutton attributed this change to the action of subterranean heat. In his day the chemistry of geology was exceedingly imperfect. The solubility of silica, for instance, and its capacity for being introduced in solution into the minutest crevices and pores of a rock were not known. It need not, therefore, surprise us to find that in the Huttonian conception the flints in chalk were injected into the rock in a molten state, and that the agate of fossil wood bore marks of igneous fusion. Hutton did not realize to what an extent mere compression could solidify the materials of sedimentary strata, nor how much may be done by infiltration and deposition between the clastic grains. But there was one effect of compression which, though not perhaps at first sight obvious, was clearly perceived by him in its geological relations. He saw that the influence of heat upon rocks must be largely modified by pressure. The more volatile components, which would speedily be driven off by a high temperature at the surface, may be retained under great pressure below that surface. Hutton conceived that limestone might even be fused in this way, and yet still keep its carbonic acid. This idea was ridiculed at the time, but its truth was confirmed afterwards by Hall's experiments, to which I shall allude in a later part of this lecture.

The next step in his reasoning was that whereby Hutton sought to account for the present position of the strata which, originally deposited under the sea, are now found even on mountain crests 15,000 feet above sea-level. We have seen how Werner looked on his vertical primitive strata as having been precipitated from solution in that position, and as having been uncovered by the gradual subsidence and disappearance of the water. Hutton attacked the problem in a very different fashion. He saw that if the exposure of the dry land were due merely to the subsidence of the sea, it would involve no change in the positions of the strata relatively to each other. What were first deposited should lie at the bottom, what were last deposited at the top; and the whole should retain their original flatness.

But the most cursory examination was, in his opinion, sufficient to show that the actual conditions in nature were entirely different from any such arrangement. Where-ever he went, he found proofs that the sedimentary strata now forming most of the land had generally lost the horizontal position in which they were accumulated. He saw them usually inclined, sometimes placed on end, or even stupendously contorted and ruptured. It was manifestly absurd, as De Saussure had shown in the Alps, to suppose that the pebbles in vertical beds of conglomerate could ever have been deposited in such positions. And if some of the vertical strata could thus be demonstrated to have been originally horizontal, there could be no reason for refusing to concede that the same alteration had happened to the other vertical strata, even though they did not supply such convincing proofs of it. No stratum could have ended off abruptly at the time of its formation, nor could it have been accumulated in plicated layers. But nothing is more common than to find strata presenting their truncated ends to the sky, while in some districts they are folded and wrinkled, like irregular piles of carpets. Not only so, but again and again they are found to be sharply dislocated, so that two totally different series are placed parallel to each other.

Hutton recognized that these changes, which were probably brought about at different periods, must be attributed to some great convulsions which, from time to time, have shaken the very foundations of the earth. He could prove that, in some places, the Primary rocks had in this way been broken up and placed on end before the Secondary series was laid down, for, as on the Berwickshire coast, he had traced the older vertical strata overlain and wrapped round by the younger horizontal deposits, and had also observed, from the well-worn fragments of the former enclosed in the latter, that the interval of time represented by the break between them must have been of considerable duration.

Having, by this admirable train of observation and deduction, been led to the demonstration of former gigantic disturbances, by which the bed of the sea had been upheaved and its hardened sediments had been tilted, plicated and fractured, to form the existing dry land, Hutton had next to look round for some probable cause for these phenomena. He inferred that the convulsions could only have been produced by some force that acted from below upward, but was so combined with the gravity and resistance of the mass to which it was applied, as to create a lateral and oblique thrust that gave rise to the contortions of the strata. He did not pretend to be able to explain the nature and operation of this subterranean force. He believed that it was essentially due to heat, and he connected with it the operations of volcanoes, which he regarded as general over the globe, and as "spiracles to the subterranean furnace in order to prevent the unnecessary elevation of land, and fatal effects of earthquakes."[1]

Unlike Werner, Hutton recognized the fundamental importance of the internal high temperature of the globe, of which volcanoes are one of the proofs. He saw that no mere combustion of substances could account for this temperature, which arose from causes so far different from ordinary combustion, that it might require no circulation of air and no supply of combustible materials to support it. The nucleus of the globe might accordingly "be a

[1] *Theory of the Earth*, vol. i. p. 146.

fluid mass, melted, but unchanged by the action of heat."

In this way, appealing at every step to the actual facts of nature, Hutton built up the first part of his immortal Theory. Most of these facts were more or less familiar to men; and some of the obvious inferences to be drawn from them had been noted by several observers before his time. But no one until then had grouped them into a coherent system by which the earth became, as it were, her own interpreter. The very obviousness and familiarity of the doctrine at the present time, when it is the groundwork of modern geology, are apt to blind us to the genius of the man who first conceived it, and worked it into a harmonious and luminous whole.

In the course of his journeys in Scotland, Hutton had come upon many examples of rocks that were not stratified. Some of these occurred among the Primary masses; others were observable in the Secondary series. Reflecting deeply on the probable reaction of the heated interior of the globe upon its outer cooler shell or crust, he had come to the conclusion that many, if not all, of these unstratified rocks were to be regarded as material that had once been in a molten condition, and had been injected from below during some of the great convulsions indicated by the disturbed strata. He distinguished three principal kinds of such intrusive rocks — "whinstone," under which term he included a miscellaneous series of dark, heavy, somewhat basic rocks, now known as dolerites, basalts, diabases and andesites; porphyry, which probably comprised such rocks as felsite, orthophyre and quartz-porphyry; and granite, which, though generally used in its modern sense, embraced some rocks of more basic character.

He showed that the whinstones correspond so closely to modern lavas in structure and composition, that they may be regarded as probably also of volcanic origin. But he did not suppose that they had actually been erupted at the surface, like streams of lava. He found them to occur sometimes in vertical veins, known in Scotland as *dykes*—a term now universal in English geological literature—and sometimes as irregular bosses, or interposed as sheets between the strata. He regarded these rocks as masses of subterranean or unerupted lava, but the grounds on which he reached this conclusion were not always such as the subsequent progress of inquiry has justified. The deduction was correct, but the reasoning that led up to it was partly fallacious. Hutton argued, for instance, that the carbonate of lime, so commonly observable in his "whinstones," indicated that the rock had been fused deep within the earth, under such pressure as to keep that mineral in a molten state, without the loss of its carbonic acid. Like the other mineralogists of his day, he was not aware that the calcite of the amygdales has been subsequently introduced in solution, and that the diffused calcite in the body of the rocks generally results from their decomposition by infiltrating water. Much more appropriate were his observations that the whinstone has greatly indurated the strata into which it has been injected, even involving fragments of them, and reducing carbonaceous substances, such as coal, to the condition of coke or charcoal; that it has been inserted among the strata with such violence as to shift, upraise, bend, or otherwise disturb them, and that it can be seen to have come in abruptly in one continuous succession of strata, which, above and below it, are exactly alike, and have obviously been at one time in contact with each other.

Granite, as Hutton pointed out, differs in many important respects from "whinstone," more particularly in its position, for it was then believed to lie beneath all the known rocks, rising to higher elevations and sinking to greater depths than any other material in the crust of the earth. Yet though he admitted its infra-position, he differed from the Neptunists in regard to its relative antiquity. He believed it to be younger than the strata that rest upon it, for he regarded it as a mass that had once been melted, and had been intruded among the rocks with which it is now found associated. He based this conclusion upon various observations, chief among which was the occurrence of abundant veins that diverge from the granite and ramify through the surrounding rocks, diminishing in width as they recede from their parent mass.

Properly to appreciate the value of these doctrines in regard to the development of a sound geological philosophy, we must bear in mind what were the prevalent views entertained on the subject when Hutton's work was published. We have seen that granite, generally regarded as an aqueous formation, was affirmed by Werner to have been the first precipitate that fell from his univeral ocean. De Saussure, who had seen more of granite and its relations than Werner, or indeed than any other geologist of his time, remained up to the last a firm believer in the aqueous origin of that rock. Even after the death of the great Swiss geologist, Cuvier, sharing his opinions on these matters, proclaimed as late as 1810 his belief that De Saussure overthrew the doctrine of central fire, or of a source of heat within the earth's interior, demonstrated granite to be the first-formed rock, and proved it to have been formed in strata deposited in water.[1] Nobody before Hutton's time had been bold enough to imagine a series of subterranean intrusions of molten matter. Those who adopted his opinion on this subject were styled Plutonists, and were looked upon as a section of the Vulcanists, but as carrying out their doctrines to still greater extravagance, "attributing to the action of fire widely-diffused rocks which nobody had till then ever dreamt of removing from the domain of water."[2]

According to the Huttonian theory, fissures and openings have from time to time arisen in the external crust of the earth, and have reached down to the intensely hot nucleus. Into these the molten material has ascended, forming veins of whinstone underground, and, where it has reached the surface, issuing there in the form of lava and the other phenomena of volcanoes. Every geologist recognizes these generalizations as part of the familiar teachings of modern geology.

We have seen that Werner made no distinction, as regards origin, between what we now call mineral veins and the dykes and veins of granite, basalt, or other eruptive rocks. He looked upon them all as the results of chemical precipitation from an ocean that covered the rocks in which fissures had been formed. Hutton, in like manner, drew no line between the same two well-marked

[1] Cuvier, "Éloge de De Saussure," *Éloges*, i. p. 427.

[2] Cuvier, *op. cit.* ii. p. 363.

series of veins, but regarded them all as due to the introduction of igneous material. Though more logical than Werner, he was, as we now know, entirely in error in confounding under one denomination two totally distinct assemblages of mineral matter. Werner correctly referred veins of ores and spars to deposition from aqueous solution, but was completely mistaken in attributing the same origin to veins of massive rock. Hutton, on the other hand, went as far astray in regard to his explanation of mineral veins, but he made an important contribution to science in his insistence upon the truly intrusive nature of veins of granite and whinstone.

There was another point of difference between the views of Werner and of Hutton in regard to mineral veins. One of the undoubted services of the Freiberg professor was his clear demonstration that veins could be classified according to their directions, that this arrangement often sufficed to separate them also according to age and material, those running along one parallel, and containing one group of minerals, being intersected by, and therefore older than, another series following a different direction, and consisting of other metals and vein-stones. This important distinction found no place in Hutton's system. To him it was enough that he was able to show that the veins known to him were intrusive masses of igneous origin.[1]

In the Huttonian theory we find the germ of the

[1] In Playfair's *Illustrations*, however, the successive origin of mineral veins is distinctly affirmed, § 226. Reference is there made to the coincidence between the prevalent direction of the principal Cornish veins and the general strike of the strata, and to the intersection of these by the cross-courses at nearly right angles.

Lyellian doctrine of metamorphism. Hutton, having demonstrated that granite is not an aqueous but an igneous rock, further showed that the "Alpine schistus," that is, the series of crystalline schists, being stratified, could not be original or primitive, but must have been deposited in successive layers like more recent sediments, and were invaded and altered by the granite. Let me quote a passage from his chapter "On the Primary Part of the Present Earth" in illustration of the sagacity of his judgment on this subject: "If, in examining our land, we shall find a mass of matter which had been evidently formed originally in the ordinary manner of stratification, but which is now extremely distorted in its structure and displaced in its position,—which is also extremely consolidated in its mass and variously changed in its composition,—which, therefore, has the marks of its original or marine composition extremely obliterated, and many subsequent veins of melted mineral matter interjected, we should then have reason to suppose that here were masses of matter which, though not different in their origin from those that are gradually deposited at the bottom of the ocean, have been more acted upon by subterranean heat and the expanding power, that is to say, have been changed in a greater degree by the operations of the mineral kingdom."[1] Hutton here compresses into a single, though somewhat cumbrous, sentence the doctrine to which Lyell in later years gave the name of metamorphism.

Hutton's vision not only reached far back into the geological past, it stretched into the illimitable future,

[1] *Theory of the Earth*, vol. i. pp. 375, 376.

and it embraced also a marvellously broad yet minute survey of the present. From his early youth he had been struck with the evidence of incessant decay upon the surface of the dry land. With admirable insight he kept hold of this cardinal fact, and followed it fearlessly from mountain-top to sea-shore. Wherever we may go, on each variety of rock, in every kind of climate, the doom of dissolution seemed to him to be written in ineffaceable characters upon the whole surface of the dry land. No sooner was the bed of the ocean heaved up into mountains, than the new terrestrial surface began to be attacked. Chemical and mechanical agents were recognized as concerned in this disintegration, though the precise nature and extent of their several operations had not then been studied. The general result produced by them, however, was never appreciated by any observer more clearly than by Hutton. From the coast, worn into stack and skerry and cave, by the ceaseless grinding of the waves, he had followed the progress of corrosion up to the crests of his Scottish hills. No rock, even the hardest, could escape, though some resisted more stubbornly than others.

The universality of this terrestrial waste had been more or less distinctly perceived by previous writers. But Hutton saw a meaning in it which no one before him had suspected. To his eye, while the whole land undergoes loss, it is along certain lines traced by running water that this loss reaches its greatest amount. In the channels of the streams that carry off the drainage of the land he recognized the results of a constant erosion of the rocks by the water flowing over them. As the generalization was beautifully expressed by Playfair: "Every river appears to consist of a main trunk, fed from a variety of branches, each running in a valley proportioned to its size, and all of them together forming a system of valleys, communicating with one another, and having such a nice adjustment of their declivities, that none of them join the principal valley, either on too high or too low a level, a circumstance which would be infinitely improbable if each of these valleys were not the work of the stream that flows in it.

"If, indeed, a river consisted of a single stream without branches, running in a straight valley, it might be supposed that some great concussion, or some powerful torrent, had opened at once the channel by which its waters are conducted to the ocean; but, when the usual form of a river is considered, the trunk divided into many branches, which rise at a great distance from one another, and these again subdivided into an infinity of smaller ramifications, it becomes strongly impressed upon the mind that all these channels have been cut by the waters themselves; that they have been slowly dug out by the washing and erosion of the land; and that it is by the repeated touches of the same instrument that this curious assemblage of lines has been engraved so deeply on the surface of the globe."[1]

The whole of the modern doctrine of earth-sculpture is to be found in the Huttonian theory. We shall better appreciate the sagacity and prescience of Hutton and Playfair, if we remember that their views on this subject were in their lifetime, and for many years after-

[1] *Illustrations of the Huttonian Theory*, p. 102.

wards, ignored or explicitly rejected, even by those who accepted the rest of their teaching. Hall, their friend and associate, could not share their opinions on this subject. Lyell too, who adopted so much of the Huttonian theory and became the great prophet of the Uniformitarian school, never would admit the truth of Hutton's doctrine concerning the origin of valleys. Nor even now is that doctrine universally accepted. It was Jukes who in 1862 revived an interest in the subject, by showing how completely the valley system in the south of Ireland was due to the action of the rivers.[1] Ramsay soon after followed with further illustrations of the principle.[2] But unquestionably the most effective support to Hutton's teaching has been given by the geologists of the United States, who, among the comparatively undisturbed strata of the Western Territories, have demonstrated, by proofs which the most sceptical must receive, the potency of denudation in the production of the topography of the land.

To the Huttonian school belongs also the conspicuous merit of having been the first to recognize the potency of glaciers in the transport of detritus from the mountains. Playfair, in his characteristically brief and luminous way, proclaimed at the beginning of this century that "for the removing of large masses of rock the most powerful engines without doubt which nature employs are the glaciers, those lakes or rivers of ice which are formed in the highest valleys of the Alps, and other mountains of the first order. . . . Before the valleys were cut out in the form they now are, and when the mountains were

[1] *Quart. Journ. Geol. Soc.* xviii. (1862).
[2] *The Physical Geology and Geography of Great Britain*, 1863.

still more elevated, huge fragments of rock may have been carried to a great distance; and it is not wonderful if these same masses, greatly diminished in size, and reduced to gravel or sand, have reached the shores or even the bottom of the ocean."[1] Here the former conception of the greater extension of the glaciers was foreshadowed as a possible or even probable event in geological history. Yet for half a century or more after Playfair's time, men were still speculating on the probability of the transport of the erratics by floating icebergs during a submergence of Central Europe under the sea,—an hypothesis for which there was not a particle of evidence. No geologist now questions the truth of Playfair's suggestion.

In the whole of Hutton's doctrine he rigorously guarded himself against the admission of any principle which could not be founded on observation. He made no assumptions. Every step in his deductions was based upon actual fact, and the facts were so arranged as to yield naturally and inevitably the conclusion which he drew from them. Let me quote from the conclusion of his work a few sentences in illustration of these statements. In the interpretation of nature, he remarks, "no powers are to be employed that are not natural to the globe, no action to be admitted of except those of which we know the principle, and no extraordinary events to be alleged in order to explain a common appearance. The powers of nature are not to be employed in order to destroy the very object of those powers; we are not to make nature act in violation to that order which we actually observe, and in subversion of that end which is to be perceived in

[1] *Illustrations*, p. 388.

the system of created things. In whatever manner, therefore, we are to employ the great agents, fire and water, for producing those things which appear, it ought to be in such a way as is consistent with the propagation of plants and the life of animals upon the surface of the earth. Chaos and confusion are not to be introduced into the order of nature, because certain things appear to our practical views as being in some disorder. Nor are we to proceed in feigning causes when those seem insufficient which occur in our experience."[1]

No geologist ever lived among a more congenial and helpful group of friends than Hutton. While they had a profound respect for his genius, they were drawn towards him by his winning personality, and he became the centre of all that was bright, vivacious and cheerful in that remarkable circle of eminent men. If he wanted advice and assistance in chemical questions, there was his bosom-friend Joseph Black, ever ready to pour out his ample stores of knowledge, and to test every proposition by the light of his wide experience and his sober judgment. If he needed companionship and assistance in his field journeys, there was the sagacious Clerk of Eldin, willing to join him, to examine his evidence with judicial impartiality, and to sketch for him with an artistic pencil the geological sections on which he laid most stress. If he felt himself in need of the counsel of a clear logical intellect, accustomed to consider physical problems with the precision of a mathematician, there was the kindly sympathetic Playfair, ever prompt and pleased to do him a service. With such companions he discussed

[1] *Theory of the Earth*, vol. ii. p. 547.

his theory in all its bearings. Their approval was ample enough for his ambition. He was never tempted to court publicity by frequent communications to learned societies, or the issue of independent works treating of his geological observations and discoveries. But for the establishment of the Royal Society of Edinburgh, he might have delayed for years the preparation of the first sketch of his theory, and had it not been for the virulent attacks of Kirwan, he might never have been induced to finish the preparation of his great work. He was a man absorbed in the investigation of nature, to whom personal renown was a matter of utter indifference.

Among Hutton's friends there was one in particular to whom a distinguished place in the list of the founders of geology must be assigned—Sir James Hall of Dunglass (1761-1832). To this original investigator we owe the establishment of experimental research, as a branch of geological investigation. Inheriting a baronetcy and a landed estate in East Lothian, not far from the picturesque cliffs of St. Abb's Head, and possessed of ample leisure for the prosecution of intellectual pursuits, he was led to interest himself in geology. His father, a man of scientific tastes, became acquainted with Hutton when the future philosopher was a farmer in the neighbouring county of Berwick. From these early days Hutton found the hospitality of Dunglass always open to him. It will be remembered that the famous visit to Siccar Point, described by Playfair, was made with Sir James from that house.

At first Sir James Hall could not bring himself to accept Hutton's views. "I was induced," he tells us, "to reject his system entirely, and should probably have continued still to do so, with the great majority of the world, but for my habits of intimacy with the author, the vivacity and perspicuity of whose conversation formed a striking contrast to the obscurity of his writings. I was induced by that charm, and by the numerous original facts which his system had led him to observe, to listen to his arguments in favour of opinions which I then looked upon as visionary. After three years of almost daily warfare with Dr. Hutton on the subject of his theory, I began to view his fundamental principles with less and less repugnance."[1]

As his objections diminished, Hall's interest in the details of the system increased. His practical mind soon perceived that some of the principles, which Hutton had established by reasoning and analogy, might be brought to the test of direct experiment. And he urged his friend to make the attempt, or allow him to carry out the necessary researches. The proposal received little encouragement from the philosopher. Hutton believed that the scale of nature's processes was so vast that no imitation of them, on the small scale of a laboratory, could possibly lead to any reliable results, or as he afterwards expressed himself in print, "there are superficial reasoning men who, without truly knowing what they see, think they know those regions of the earth which can never be seen, and who judge of the great operations of the mineral kingdom from having kindled a fire and looked into the bottom of a little crucible."[2]

Sir James Hall, notwithstanding his veneration for his

[1] *Trans. Roy. Soc. Edin.* vi. (1812), pp. 71-186.

[2] *Theory of the Earth*, vol. i. p. 251.

master, could not agree with him in this verdict. He was confirmed in his opinion by an accident which had occurred at the Leith glass-works, where a large mass of common green glass, that had been allowed to cool slowly, was found to have lost all the properties of glass, becoming opaque, white, hard and crystalline. Yet a piece of this substance, when once more melted and rapidly cooled, recovered its true vitreous characters. Hall's shrewd instinct at once applied this observation to the Huttonian doctrine of the igneous origin of granite and other rocks. It had been objected to Hutton's views that the effect of great heat on rocks was to reduce them to the condition of glass, but that granite and whinstone, being crystalline substances, could never possibly have been melted. Yet here, in this glass-house material, it could be demonstrated that a thoroughly molten glass could, by slow cooling, be converted into a crystalline condition, and could be changed once more by fusion into glass. Hutton had overlooked the possibility that the results of fusion might be modified by the rate of cooling, and Hall at once began to test the matter by experiment. He repeated the process by which the devitrified glass had been accidentally obtained at the glass-house, and found that he could at will produce, from the same mass of bottle glass, either a glass or a stony substance, according to the rate at which he allowed it to cool.

Sir James was too loyal a friend and too devoted an admirer of the author of the *Theory of the Earth* to pursue these researches far during the philosopher's lifetime. "I considered myself as bound," he tells us, "in practice to pay deference to his opinion, in a field which he had already so nobly occupied, and I abstained during the remainder of his life from the prosecution of experiments which I had begun in 1790."[1]

The death of Hutton in 1797 allowed the laird of Dunglass to resume the experiments on which he had been meditating during the intervening years. Selecting samples of "whinstones," that is, intrusive dolerites and basalts, from the dykes and sills in the Carboniferous strata around Edinburgh, he reduced them in the reverberatory furnace of an iron-foundry to the condition of perfect glass. Portions of this glass were afterwards re-fused and allowed to cool very slowly. There was thus obtained "a substance differing in all respects from glass, and in texture completely resembling whinstone." This substance had a distinctly crystalline structure, and Hall gave it the name of *crystallite*, which had been suggested by the chemist, Dr. Hope.

Before he was interested in the defence of the Huttonian theory, Sir James had made a journey into Italy in the year 1785, visiting Vesuvius, Etna, and the Lipari Isles, and having for part of the time the advantage of the company of Dolomieu. He could not help being much struck with the resemblance between the lavas of these volcanic regions and the familiar "whinstones" of his own country. So close was this resemblance in every respect that he felt "confident that there was not a lava in Mount Etna to which a counterpart might not be produced from the whinstones of Scotland." At Monte Somma he noted the abundant "vertical lavas" which, in bands from two

[1] For Hall's papers see *Trans. Roy. Soc. Edin.* iii. (1790), p. 8; v. (1798), p. 43; vi. (1812), p. 71; vii. (1812), pp. 79, 139, 169; x. (1825), p. 314.

to twelve feet broad, run up the old crater-wall. These bands seemed to him at the time "to present only an amusing variety in the history of volcanic eruptions," and, like Dolomieu and Breislak, he looked on them as marking the positions of rents which, formed in the mountain during former volcanic explosions, had been filled in from above by the outflow of lava down the outer fissured surface of the cone. Subsequent reflection, however, led him to reconsider this opinion, and to realize that these "vertical lavas" were "of the utmost consequence in geology, by supplying an intermediate link between the external and subterraneous productions of heat. I now think," he remarks, "that though we judged rightly in believing those lavas to have flowed in crevices, we were mistaken as to their direction; for instead of flowing downwards, I am convinced they have flowed upwards, and that the crevices have performed the office of pipes, through which lateral explosions have found a vent." He had observed, also, that the outer margins of some of these dykes, in contact with the surrounding rock, were vitreous, while the central parts presented the ordinary lithoid texture. This difference, he saw, was fully explained by his fusion experiments, the lava having risen in a cold fissure, and having been suddenly chilled along its outer surface, while the inner parts cooled more slowly and took a crystalline structure.

These observations are of historic interest in the progress of volcanic geology. Hall had sagaciously found the true interpretation of volcanic dykes, and he at once proceeded to apply it to the explanation of the abundant dykes of Scotland. He thus brought to the support of

Hutton's doctrine of the igneous intrusion of these rocks a new and strong confirmation from the actual crater of an ancient volcano.

When engaged upon his fusion experiments with Scottish whinstones, it occurred to Hall to subject to the same processes specimens of the lavas which he had brought from Vesuvius and Etna. The results which he thus obtained were precisely similar to those which the rocks from Scotland had yielded. He was able to demonstrate that lavas may be fused into a perfect glass, and that this glass, on being re-melted and allowed to cool gradually, passes into a stony substance not unlike the original lava. In this manner, the close agreement between modern lavas and the ancient basalts of Scotland was clearly proved, while their identity in chemical composition was further shown by some analyses made by Dr. Robert Kennedy. Sir James Hall had thus the satisfaction of showing that a fresh appeal to direct experiment and observation furnished further powerful support to some of the disputed doctrines in the theory of his old friend Hutton.[1]

There was another and still more important direction in which it seemed to this original investigator that the Huttonian doctrines might be subjected to the test of experiment. It was an important feature in these doctrines that the effects of heat upon rocks must differ very much according to the pressure under which the heat is applied. Hall argued, like Hutton, that within the earth's crust the

[1] "Experiments on Whinstone and Lava," read before the Royal Society of Edinburgh 5th March and 18th June 1798, *Trans. Roy. Soc. Edin.* vol. v. p. 43.

influence of great compression must retard the fusion of mineral substances, and retain within them ingredients which, at the ordinary atmospheric pressure above ground, are rapidly volatilized. He thus accounted for the retention of carbonic acid by calcareous rocks, even at such high temperatures as might melt them. Here then was a wide but definite field for experiment, and Hall entered it with the joy of a first pioneer. As soon as he had done with his whinstone fusions, he set to work to construct a set of apparatus that would enable him to subject minerals and rocks to the highest obtainable temperatures in hermetically closed tubes. For six or seven years, he continued his researches, conducting more than 500 ingeniously devised experiments. He enclosed carbonate of lime in firmly secured gun-barrels, in porcelain tubes, in tubes bored through solid iron, and exposed it to the highest temperatures he could obtain.

He was able to fuse the carbonate without the loss of its carbonic acid, thus practically demonstrating the truth of Hutton's contention. He obtained from pounded chalk a substance closely resembling marble. Applying these results to the Huttonian theory, he contended that the effects shown by his experiments must occur also on a great scale at the roots of volcanoes; that subterranean lavas may melt limestone; that where the molten rock comes in contact with shell-beds, it may either drive off their carbonic acid or convert them into limestone, according to the heat of the lava and the depth under which it acts; and that his experiments enabled him to pronounce under what conditions the one or the other of these effects would be produced. He concluded that having succeeded in fusing limestone under pressure, he could adduce in that single result "a strong presumption in favour of the solution which Dr. Hutton has advanced of all the geological phenomena; for the truth of the most doubtful principle which he has assumed has thus been established by direct experiment."[1]

Hardly less striking were Hall's experiments in illustration of the processes whereby strata, originally horizontal, have been thrown into plications. His machine for contorting layers of clay is familiar to geological students from the illustrations of it given in text-books.[2] He showed how closely the convolutions of the Silurian strata of the Berwickshire coast could be experimentally imitated by the lateral compression of layers of clay under considerable vertical pressure. In this, as in his other applications of experiment, he led the way, and laid the foundation on which later observers have built with such success.

There was thus established at Edinburgh a group of earnest and successful investigators of the history of the earth, who promulgated a new philosophy of geology, based

[1] "Account of a series of experiments showing the effects of compression in modifying the action of heat," read to the Royal Society of Edinburgh, 3rd June 1805.—*Trans. Roy. Soc. Edin.* vi. p. 71. The same ingenious observer subsequently instituted a series of experiments to imitate the consolidation of strata. By filling an iron vessel with brine and having layers of sand at the bottom he was able to keep the lower portions of the sand at a red heat, while the brine at the top was not too hot to let the hand be put into it. In the end the sand at the bottom was found compacted into sandstone.—*Op. cit.* x. (1825), p. 314.

[2] *Trans. Roy. Soc. Edin.* vol. vii. p. 79 and Plate iv. As already remarked, Hall differed from his master and from Playfair in regard to their views on the efficacy of subaerial denudation. He preferred to invoke gigantic debacles, and to these he attributed the transport of large boulders and the smoothing and striation of rocks, now attributed to the action of glaciers and ice-sheets.

upon close observation and carefully devised experiment. Among these men there was only one teacher—the gentle and eloquent Playfair ; but his functions at the University were to teach mathematics and natural philosophy. He had thus no opportunity of training a school of disciples who might be sent forth to combat the errors of the dominant Wernerianism. He did what he could in that direction by preparing and publishing his admirable "Illustrations," which were widely read, and, as Hall has recorded, exerted a powerful influence on the minds of the most eminent men of science of the day.

But another influence strongly antagonistic to the progress of the Huttonian philosophy was established in Edinburgh at the very time when the prospect seemed so fair for the creation of a Scottish school which might do much to further the advance of sound geology. Robert Jameson (1774-1854) studied for nearly two years at Freiberg under Werner, and after two more years spent in continental travel, full of enthusiasm for his master's system, returned to the Scottish capital in 1804, when he was elected to the Chair of Natural History in the University. His genial personal character, and his zeal for the Freiberg faith soon gathered a band of ardent followers around him. He had much of Werner's power of fostering in others a love of the subjects that interested himself. Travelling widely over Scotland, from the southern borders to the furthest Shetland Isles, he everywhere saw the rocks through Saxon spectacles. From the very beginning, the books and papers which he wrote were drawn up after the most approved Wernerian method, pervaded by the amplest confidence in that method, and by hardly disguised contempt for every other. Nowhere indeed can the peculiarities of the Wernerian style be seen in more typical perfection than in the writings of the Edinburgh professor.[1]

In the year 1808, Jameson founded a new scientific association in Edinburgh, which he called the "Wernerian Natural History Society," with the great Werner himself at the head of its list of honorary members. So far as geology was concerned, the original aim of this institution appears to have been to spread the doctrines of Freiberg. I know no more melancholy contrast in geological literature than is presented when we pass from the glowing pages of Playfair, or the suggestive papers of Hall, to the dreary geognostical communications in the first published Memoirs of this Wernerian Society. On the one side, we breathe the spirit of the most enlightened modern geological philosophy, on the other we grope in the darkness of a Saxon mine, and listen to the repetition of the familiar shibboleths, which even the more illustrious of Werner's disciples were elsewhere beginning to discard.

The importation of the Freiberg doctrines into Scotland by an actual pupil of Werner, carried with it the controversy as to the origin of basalt. This question might have been thought to have been practically settled there by the writings of Hutton, Playfair, and Hall, even if it had not been completely solved by Desmarest, Von Buch, and D'Aubuisson on the Continent. But the advent of Jameson

[1] See, for instance, the way in which he dismisses the observations of Faujas de St. Fond on Scottish rocks, and the unhesitating declaration that there is not in all Scotland the vestige of a volcano.—*Mineralogy of the Scottish Isles* (1800), p. 5. He never looses an opportunity of a sneer at the "Vulcanists" and "fire-philosophers."

rekindled the old fires of controversy. The sections around Edinburgh, which display such admirable illustrations of eruptive rocks, were confidently appealed to alike by the Vulcanists and the Neptunists. Jameson carried his students to Salisbury Crags and Arthur Seat, and there demonstrated to them that the so-called igneous rocks were manifestly merely chemical precipitates in the "Independent Coal formation." The Huttonians were glad to conduct any interested stranger to the very same sections to prove that the whinstone was an igneous intrusion. There is a characteristic anecdote told of one of these excursions by Dr. Fitton in the *Edinburgh Review*. One of the Irish upholders of the aqueous origin of basalt, Dr. Richardson, had attained some notoriety from having found fossils in what he called basalt at Portrush, on the coast of Antrim. His discovery was eagerly quoted by those who maintained the aqueous origin of that rock, and though eventually Playfair showed that the fossils really lie in Lias shale which has been baked into a flinty condition by an intrusive basaltic sheet, this explanation was not accepted by the other side, and the fossiliferous basalt of Antrim continued to be cited as an indubitable fact by the zealous partizans of Werner. While these were still matters of controversy Dr. Richardson of Portrush paid a visit to Scotland, chiefly with reference to fiorin grass, in which he was interested. The writer in the *Edinburgh Review* was asked, he tells us, by Sir James Hall, to meet Dr. Hope and the Irish geologist. "It was arranged that the party should go to Salisbury Crags, to show Dr. Richardson a junction of the sandstone with the trap, which was regarded as an instructive example of that class of facts. After reaching the spot, Sir James pointed out the great disturbance that had taken place at the junction, and particularly called the attention of the doctor to a piece of sandstone which had been whirled up during the convulsion and enclosed in the trap. When Sir James had finished his lecture, the doctor did not attempt to explain the facts before him on any principle of his own, nor did he recur to the shallow evasion of regarding the enclosed sandstone as contemporaneous with the trap; but he burst out into the strongest expressions of contemptuous surprise that a theory of the earth should be founded on such small and trivial appearances! He had been accustomed, he said, to look at Nature in her grandest aspects, and to trace her hand in the gigantic cliffs of the Irish coast; and he could not conceive how opinions thus formed could be shaken by such minute irregularities as those which had been shown to him. The two Huttonian philosophers were confounded; and, if we recollect rightly, the weight of an acre of fiorin and the number of bullocks it would feed formed the remaining subjects of conversation."[1]

It is not needful to follow into further detail the history of the opposition encountered by the Huttonian theory of the earth. Some of the bitterest antagonists of Hutton hailed from Ireland. Besides Richardson, with his fossiliferous basalt, there was Kirwan, President of the Royal Irish Academy, whose ungenerous attacks stung Hutton into the preparation of his larger treatise. In England and on the Continent another determined opponent was found in the versatile and prolific De Luc. But though

[1] *Edinburgh Review*, No. lxv. 1837, p. 9.

these men wielded great influence in their day, their writings have fallen into deserved oblivion. They are never read save by the curious student, who has leisure and inclination to dig among the cemeteries of geological literature.

The gradual decay of Wernerianism is well indicated by the eight volumes of *Memoirs* published by Jameson's Wernerian Society, which ranged from 1811 to 1839, an interval of less than a generation. The early numbers might have emanated from Freiberg itself. Not a sentiment is to be found in them of which Werner himself would not have approved. How heartily, for example, Jameson must have welcomed the concluding sentence of a paper by one of the ablest of his associates when, after a not very complimentary allusion to Hutton's views about central heat, the remark is made—"He who has the boldness to build a theory of the earth without a knowledge of the natural history of rocks, will daily meet with facts to puzzle and mortify him."[1] The fate which this complacent Wernerian here predicted for the followers of Hutton, was now surely and steadily overtaking his own brethren. One by one the faithful began to fail, and those who had gone out to preach the faith of Freiberg came back convinced of its errors, and of the truth of much which they had held up to scorn in the tenets of Hutton. Even among Jameson's own students, defections began to appear. His friends might translate into English, and publish at Edinburgh, tracts of the most orthodox Wernerianism, such as Werner's *Treatise on Veins*, or Von Buch's *Description of Landeck*, or D'Aubuisson's *Basalts of Saxony*. But his

[1] The Rev. John Fleming, *Mem. Wer. Soc.* vol. ii. (1813), p. 154.

pupils, who went farther afield, who came into contact with the distinct current of opposition to some of the doctrines of the Freiberg school that was now setting in on the Continent, who set themselves seriously to study the Huttonian theory, and who found at every turn facts that could not be fitted into the system of Freiberg, gradually, though often very reluctantly, went over to the opposite camp. Men like Ami Boué would send him notes of their travels full of what a devout Wernerian could not but regard as the rankest heresy.[1] But Jameson with great impartiality printed these in the Society's publications. And so by degrees the *Memoirs* of the Wernerian Society ceased to bear any trace of Wernerianism, and contained papers of which any Huttonian might have been proud to be the author.[2]

So long as Werner lived, however, his school remained predominant. Loyalty to their master kept his pupils from openly rejecting his doctrines, even when they could no longer accept them. His death in 1817 was felt to bring a relief from the despotism which he had so long exercised.[3] And from that time his system began rapidly to decline in favour even in Germany.

But even while Werner was in the full meridian of his influence, various observers in Europe, in addition to Von Buch and D'Aubuisson, without definitely becoming con-

[1] See *Mem. Wer. Soc.* vol. iv. (1822), p. 91.

[2] See for example the excellent papers by Hay Cunningham in vols. vii. and viii.

[3] One of Jameson's ablest pupils, Ami Boué, trained in the Wernerian faith, confessed, but with evident reluctance, and "as a truth which others may be unwilling to make public," that Werner's death had greatly contributed to the progress of geology in Germany.—*Journ. Phys.* xciv. (1822), p. 298.

troversialists, were providing a large body of material which eventually proved of great service in the establishment of a sound geology. Chief among them were those who devoted themselves with such ardour to the study of the Italian volcanoes. One of the most active and interesting of them was the Frenchman Dolomieu (1750-1801), who died at the early age of fifty-one, after a strangely eventful life. He travelled much and wrote largely, specially devoting his attention to the active and extinct volcanoes of the Mediterranean. As far back as 1783 he published a little volume on the Lipari Isles. Afterwards he followed Desmarest in describing the old volcanoes of Auvergne.[1] Though his theoretical views were not always sound, he was a careful and indefatigable observer, and provided copious material towards the establishment of the principles of geology. To him more than perhaps to any of his contemporaries is science indebted for recognizing and enforcing the connection of volcanoes with the internal heat of the globe.

Faujas de St. Fond (1742-1819) did excellent service by his splendid folio on the old volcanoes of the Vivarais and the Velay—a work lavishly illustrated with engravings, which, by showing so clearly the association of columnar lavas with unmistakable volcanic cones, ought to have done much to arrest the progress of the Freiberg doctrine of the aqueous origin of basalt.[2] The same good observer undertook a journey into the Western Isles of Scotland towards the end of last century,[3] when that region was much less

[1] *Journ. des Mines*, vol. vii. (1798), pp. 393-405.

[2] *Recherches sur les Volcans éteints du Vivarais et du Velay*, folio, 1778.

[3] *Voyage en Angleterre, en Écosse, et aux Iles Hébrides*, 2 vols. 8vo, 1797.

easily visited than it now is, and convinced himself of the volcanic origin of the basalts there, thus adding another important contribution to the literature of volcanic geology.

Spallanzani (1729-1799), the illustrious professor of Pavia, Reggio, and Modena, born as far back as 1729, devoted his earlier life to animal and vegetable physiology, and was fifty years of age before he began to turn his attention to geological questions. But from that period onward he made many journeys in the basin of the Mediterranean from Constantinople to Marseilles. Of especial interest were his minute and picturesque descriptions of the eruptions of Stromboli, which at not a little personal risk he watched from a crevice in the lava. His *Travels in the Two Sicilies and in some Parts of the Apennines* contained a vast assemblage of careful observations among the recent and extinct volcanoes of Italy.[1]

Another Italian geologist, Scipio Breislak (1748-1826), did good service in making known the volcanic phenomena of his native country, and in publishing two general treatises on geology, in which he ranged himself among the Vulcanists. "I respect," he wrote, "the standard raised by Werner, but the flag of the marvellous and mysterious will never be that which I shall choose to follow."[2]

The days of mere theorizing in the cabinet or the study had now passed away. Everywhere there was aroused a spirit of inquiry into the evidence furnished by the earth

[1] *Viaggi alle due Sicilie*, 1792-93.

[2] *Introduzione alla Geologia*, 2 vols. 8vo, 1811, translated into French, 1 vol. 1812. Breislak was the author of a valuable treatise on the physical and lithological topography of Campania, and of other works on Italian and general geology.

itself as to its history. The main theoretical principles of the science had been established, but there remained to be discovered and applied the fruitful doctrine of stratigraphy. How this doctrine, which has done more than any other for the progress of geological investigation, was developed will be the subject of the next lecture.

LECTURE V

The rise of stratigraphical geology—The work of Giraud-Soulavie, Cuvier, Brongniart and Omalius d'Halloy in France; the labours of Michell and William Smith in England.

THAT the rocks around and beneath us contain the record of terrestrial revolutions before the establishment of the present dry land, was an idea clearly present to the minds of the early Italian geologists, and was generally admitted, before the end of last century, by all who interested themselves in minerals and rocks. The Neptunists and Vulcanists might dispute vigorously over their respective creeds, but they all agreed in maintaining the doctrine of a geological succession. Werner made this doctrine a cardinal part of his system, and brought it into greater prominence than it had ever held before his time. His sequence of formations from granite, at the base, to the youngest river-gravel or sea-formed silt betokened, in his view, a gradual development of deposits, which began with the chemical precipitates of a universal ocean, and ended with the modern mechanical and other accumulations of terrestrial surfaces, as well as of the sea-floor. But, as we have seen, the lithological characters on which he based the discrimination of his various formations proved to be unreliable. Granite was soon found not always to lie at the bottom. Basalt, at first placed by him among the oldest formations, turned up incontinently among the youngest. He and his disciples were consequently obliged to alter and patch the Freiberg system, till it lost its simplicity and self-consistence, and was still as far as ever from corresponding with the complex order which nature had followed. Obviously the Wernerian school had not found the key to the problem, though it had done service in showing how far a lithological sequence could be traced among the oldest rocks.

Hutton's views on this question were in some respects even less advanced than Werner's. He realized, as no one had ever done before him, the evidence for the universal decay of the land. At the same time, he perceived that unless some compensating agency came into play, the whole of the dry land must eventually be washed into the sea. The upturned condition of the Primary strata, which had once been formed under the sea, furnished him with proofs that in past time the sea-floor has been upheaved into land. Without invoking any fanciful theory, he planted his feet firmly on these two classes of facts, which could be fully demonstrated. To his mind the earth revealed no trace of a beginning, no prospect of an end. All that he could see was the evidence of a succession of degradations and upheavals, by which the balance of sea and land and the habitable condition of our globe were perpetuated. Hutton was unable to say how many of these revolutions may be chronicled among the rocks of the earth's crust.[1] Nor did he discover any method by

[1] Playfair thought that the revolutions may have been often repeated, and that our present continents appear to be the third in succession, of which relics may be observed among the rocks.—*Works*, vol. iv. p. 55.

which their general sequence over the whole globe could be determined.

A totally new pathway of investigation had now to be opened up. The part that had hitherto been played by species of minerals and rocks was henceforth to be taken by species of plants and animals. Organic remains, imbedded in the strata of the earth's crust, had been abundantly appealed to as evidence of the former presence of the sea upon the land, or as proofs of upheaval of the sea-floor. But they were now to receive far closer attention, until they were found to contain the key to geological history, and to furnish a basis by which the past revolutions of the globe could be chronologically arranged and accurately described.

Apart altogether from questions of cosmogony or of geological theory, some of the broad facts of stratigraphy could not but, at an early time, attract attention. In regions of little-disturbed sedimentary rocks, the superposition of distinct strata, one upon another, was too obvious to escape notice. A little travel and observant eyes would enable men to see that the same kinds of strata, accompanied by the same topographical characters, ranged from district to district, across wide regions. We have found that it was in countries of regular and gently-inclined stratified rocks that Lehmann and Fuchsel made their observations, which paved the way for the development of the idea of geological succession. We have now to trace the growth of this idea, and the discovery that organic remains furnish the clue to the relative chronology of the strata in which they are imbedded.

There were two regions of Europe well fitted to furnish any observant inquirer with the means of establishing this supremely important section of modern geology. In France, the Secondary and Tertiary formations lie in undisturbed succession, one above another, over hundreds of square miles. They come to the surface, not obscured under superficial deposits, but projecting their escarpments to the day, and showing, by their topographical contours, the sharply defined limits of their several groups. Again, in England, the same formations cover the southern and eastern parts of the country, displaying everywhere the same clear evidence of their arrangement. Let us trace the progress of discovery in each of these regions. To a large extent this progress was simultaneous, but there is no evidence that the earlier workers in the one country were aware of what was being done in the other.

To the Abbé Giraud-Soulavie (1752-1813) the merit must be assigned of having planted the first seeds from which the magnificent growth of stratigraphical geology in France has sprung. Among other works, he wrote a *Natural History of Southern France* in seven volumes, of which the first two appeared in the year 1780. He gave much of his attention to the old volcanoes of his native country, and devoted several of his volumes entirely to their description. But his chief claim to notice here lies in a particular chapter of his work which, he tells us, was read before the Royal Academy of Sciences of Paris on 14th August 1779.[1] In describing the calcareous mountains of the Vivarais, he divides the limestones into five epochs or ages, the strata in each of which are marked by a distinct

[1] *Histoire Naturelle de la France Méridionale*, tome i. 2^me partie, chap. viii. p. 317.

assemblage of fossil shells. The first of these ages, he declared, was represented by limestone containing organic remains with no living analogues, such as ammonites, belemnites, terebratulæ, gryphites, etc. Having no more ancient strata in the district, the Abbé called this oldest limestone primordial. His second age was indicated by limestone, in which the fossils of the preceding epoch were still found, but associated with some others now living in our seas. Among the new forms of life that appeared in these secondary strata he enumerated chamas, mussels, comb-shells, nautili, etc. These, he said, inhabited the sea, together with survivors from the first age, but the latter at the end of the second age disappeared. Above their remains other races established themselves, and carried on the succession of organized beings.

The third age was one in which the shells were of recent forms, with descendants that inhabit our present seas. The remains of these shells were found in a soft white limestone, but not a trace of ammonite, belemnite, or gryphite was to be seen associated with them. Among the organisms named by the Abbé were limpets, whelks, volutes, oysters, sea-urchins, and others, the number of species increasing with the comparative recentness of the formation. He thought that the most ancient deposits had been accumulated at the highest levels, when the sea covered the whole region, and that, as the waters sank, successively younger formations were laid down at lower and lower levels.

From the occurrence of worn pebbles of basalt in the third limestone, Giraud-Soulavie inferred that volcanic eruptions had preceded that formation, and that an enormous duration of time was indicated by the erosion of the lavas of these volcanoes, and the transport and deposit of their detritus in the white limestone.

The fourth age in the Vivarais was represented by certain carbonaceous shales or slates, containing the remains of primordial vegetation to which it was difficult to discover the modern analogues. Giraud-Soulavie believed that he could observe among these slates a succession of organic remains similar to that displayed by the limestones, those strata which lay on the oldest marble containing ammonites, while the most recent enclosed, but only rarely, unknown plants mingled with known forms. It would thus appear that the deposits of the so-called fourth age were more or less equivalents of those of the three calcareous ages.

The fifth age was characterized by deposits of conglomerate and modern alluvium, containing fossil trees, together with bones and teeth of elephants and other animals. "Such is the general picture," the Abbé remarks, "presented by our old hills of the Vivarais, and of the modern plains around them. The progress of time and, above all, of increased observation will augment the number of epochs which I have given, and fill up the blanks; but they will not change the relative places which I have assigned to these epochs."[1] He felt confident that if the facts observed by him in the Vivarais were confirmed in other regions, a historical chronology of fossil and living organisms would be established on a basis of incontestible truth. In his last volume, replying to some objections made to his opinions regarding the succession of animals in time, he

[1] *Op. cit.* p. 350.

contends that the difference between the fossils of different countries is due not to a geographical but to a chronological cause. "The sea," he says, "produces no more ammonites, because these shells belong to older periods or other climates. The difference between the shells in the rocks rests on the difference in their relative antiquity, and not on mere local causes. If an earthquake were to submerge the ammonite-bearing rocks of the Vivarais beneath the Mediterranean, the sea returning to its old site would not bring back its old shells. The course of time has destroyed the species, and they are no longer to be found in the more recent rocks."[1]

The sagacity of these views will at once be acknowledged. Yet they seem to have made no way either in France or elsewhere. The worthy Abbé, though a good observer and a logical reasoner, was a singularly bad writer. At the end of the eighteenth century a wretched style was an unpardonable offence even in a man of science.[2] Whatever may have been the cause, Giraud-Soulavie has fallen into the background. His fame has been eclipsed, even in France, by the more brilliant work of his successors. Yet, in any general survey of geological progress, it is only just to acknowledge how firmly he had grasped some of the fundamental truths of stratigraphical geology, at a time when the barren controversy about the origin of basalt was the main topic of geological discussion throughout Europe.

We have seen that the distinctness, regularity, and persistence of the outcrops of the various geological for-

[1] *Op. cit.* tome vii. (1784), p. 157.
[2] D'Archiac, *Géologie et Paléontologie*, 1866, p. 145.

mations of the Paris basin suggested to Guettard the first idea of depicting on maps the geographical distribution of rocks and minerals. The same region and the same features of topography and structure inspired long afterwards a series of researches that contributed in large measure to the establishment of the principles of geological stratigraphy. No fitter birthplace could be found in Europe for the rise of this great department of science. Around the capital of France, the Tertiary and Secondary formations are ranged in orderly sequence, group emerging from under group, to the far confines of Brittany on the west, the hills of the Ardennes and the Vosges on the east, and the central plateau on the south. Not only is the succession of the strata clear, but their abundant fossils furnish a most complete basis for stratigraphical arrangement and comparison.

Various observers had been struck with the orderly sequence of rocks in this classic region. Desmarest tells us that the chemist Rouelle was so impressed with its symmetry of structure that, though he never wrote anything on the subject, he used to discourse on it to his students at the Jardin des Plantes, of whom Desmarest himself appears to have been one. He would enlarge to them upon the significance of the masses of shells imbedded in the rocks of the earth's surface, pointing out that these rocks were not disposed at random, as had been supposed. He saw that the shells were not the same in all regions, that certain forms were always found associated together, while others were never to be met with in the same strata or layers. He noticed, as Guettard had done before him, that in some districts the fossil shells were grouped in exactly the same kind of arrangement and distribution as on the floor of the present sea—a fact which, in his eyes, disproved the notion that these marine organisms had been brought together by some violent deluge; but, on the other hand, showed that the present land had once been the bottom of the sea, and had been laid dry by some revolution that took place without producing any disturbance of the strata. Rouelle recognized a constant order in the arrangement of the shells. Thus, immediately around Paris, he found certain strata to be full of screw shells (*Turritella*, *Cerithium*, etc.), and to extend to Chaumont, on the one side, and to Courtagnon near Rheims, on the other. He pointed to a second deposit, or "mass" as he called it, full of belemnites, ammonites, gryphites, etc. (Jurassic), forming a long and broad band outside the eastern border of the Chalk, and stretching north and south beyond that formation up to the old rocks of the Morvan. Desmarest's account of his teacher's opinion was published in the third year of the Republic.[1] It is thus evident that Rouelle had formed remarkably correct views of the general stratigraphy of the Paris basin probably long before 1794.

Desmarest himself published many valuable observations regarding the rocks of the Paris basin in separate articles in his great *Géographie Physique*. Lamanon had written on the gypsum deposits of the region, which he regarded as marking the sites of former lakes, and from which he described and figured the remains of mammals, birds and fishes. Noting the alternations of gypsum and

[1] *Géographie Physique* (*Encyclopédie Méthodique*), tome i. (1794), pp. 409-431.

marls, he traced what he believed to be the limits of the sheets of freshwater in which they were successively deposited. Still more precise was the grouping adopted by Lavoisier. This great man, who, if he had not given himself up to chemistry, might have become one of the most illustrious among the founders of geology, was, as you will remember, associated early in life with Guettard in the construction of mineralogical maps of France. As far back as the year 1789, he distinguished between what he called littoral banks and pelagic banks, which were formed at different distances from the land, and were marked by distinct kinds of sediment and peculiar organisms. He thought that the different strata, in such a basin as that of the Seine, pointed to very slow oscillations of the level of the sea, and he believed that a section of all the stratified deposits between the coasts and the mountains would furnish an alternation of littoral and pelagic banks, and would reveal by the number of strata the number of excursions made by the waters of the ocean. Lavoisier accompanied his essay with sections which gave the first outline of a correct classification of the Tertiary deposits of the Paris region. His sketch was imperfect, but it represented in their true sequence the white Chalk supporting the Plastic Clay, lower sands, Calcaire Grossier, upper sands and upper lacustrine limestone.[1]

A few years later, a still more perfect classification of the Tertiary deposits around Paris was published by

[1] *Mém. Acad. Roy. Sciences* (1789), p. 350, pl. 7. This memoir of Lavoisier on modern horizontal strata and their disposition is fully noticed by Desmarest in the first volume of his *Géographie Physique*, p. 783.

Coupé, but without sufficiently detailed observations to convince his contemporaries that the work was wholly reliable.[1]

It was not until the year 1808 that the Tertiary stratigraphy of the basin of the Seine was worked out in some detail, and that a foundation was thereby furnished for the establishment of a general system of stratigraphical geology in France. The task was accomplished by two men who have left their mark upon the history of the science, Cuvier and Brongniart.

Georges Chrétien Leopold Dagobert Cuvier (1769-1832) came of an old Protestant family in the Jura which in the sixteenth century had fled from persecution and settled at Montbéliard, then the chief town of a little principality belonging to the Duke of Würtemberg. He was born at that place on 23rd August 1769, and after a singularly brilliant career at school and at the Caroline Academy of Stuttgart, became tutor in a Normandy family living near Fécamp. He had been drawn into the study of natural history, when a mere child, by looking over the pages of Buffon, and had with much ardour taken to the observation of insects and plants. In Normandy, the treasures of the sea were opened to him. Gradually his dissections and descriptions, though not published, came to the notice of some of the leading naturalists of France, and he was eventually induced to come to Paris, where, after filling various appointments, he was elected to the chair of Comparative Anatomy in 1795.

Cuvier's splendid career belongs mainly to the history of biology. We are only concerned here in noting how he

[1] *Journ. de Physique*, tome lix. (1804), pp. 161-176.

came to be interested in geological questions. He tells himself that some *Terebratulæ* from the rocks at Fécamp suggested to him the idea of comparing the fossil forms with living organisms. When he settled in Paris, he pursued this idea, never losing an opportunity of studying the fossils to be found in the different collections. He began by gathering together as large a series as he could obtain of skeletons of living species of vertebrate animals, as a basis for the comparison and determination of extinct forms. As a first essay in the new domain which he was to open up to science, he read to the Institute, at the beginning of 1796, a memoir in which he demonstrated that the fossil elephant belonged to a different species from either of the living forms. Two years later, having had a few bones brought to him from the gypsum quarries of Montmartre, he saw that they indicated some quite unknown animals. Further research qualified him to reconstruct the skeletons, and to demonstrate their entire difference, both specifically and generically, from any known creatures of the world of to-day. He was thus enabled to announce the important conclusion that the globe was once peopled by vertebrate animals which, in the course of the revolutions of its surface, have entirely disappeared.

These discoveries, so remarkable in themselves, could not but suggest many further inquiries to a mind so penetrating and philosophical as that of Cuvier. He narrates how he was pursued and haunted by a desire to know why these extinct forms disappeared, and how they had come to be succeeded by others. It was at this point that he entered upon the special domain of geology. He found that besides studying the fossil bones in the cabinet it was needful to understand, in the field, the conditions under which they have been entombed and preserved. He had himself no practical acquaintance with the structure and relations of rocks, but he was fortunate in securing the co-operation of a man singularly able to supply the qualifications in which he was himself deficient.

Alexandre Brongniart (1770-1847), Cuvier's associate, was a year younger than the great anatomist, having been born in Paris in 1770. He began his career early in life by endeavouring to improve the art of enamelling in France. Thereafter he served in the medical department of the army until he was attached to the Corps of Mines and was made director of the famous porcelain factory of Sèvres. He had long given his attention to minerals and rocks, and was eventually appointed professor of mineralogy at the Museum of Natural History. But his tastes led him also to study zoology. Thus, among his labours in this field, he worked out the zoological and geological relations of Trilobites. There was consequently in their common pursuits, a bond of union between the two observers. They had both entered upon a domain that was as yet almost untrodden; and each brought with him knowledge and experience that were needful to the other.

Accordingly they engaged in a series of researches in the basin of the Seine which continued for some years. Cuvier relates that during four years he made almost every week an excursion into the country around Paris, for the sake of studying its geological structure. Particular attention was given to two features,—the evidence of a definite succession among the strata, and the distinction of the organic remains contained in them.

At last the results of these investigations were embodied in a joint memoir by Cuvier and Brongniart, which first appeared in the year 1808.[1]

They seem to have continued their researches with great industry during the following years. An account of these additional observations was read by them before the Institute in April 1810, and was published as a separate work with a map, sections, and plate of fossils in 1811.[2] Referring afterwards to this conjoint essay and its subsequent enlargement, Cuvier generously wrote that though it bore his name, it had become almost entirely the production of his friend, from the infinite pains which, ever since the first conception of their plan, and during their various excursions, he had bestowed upon the thorough investigation of all the objects of the inquiry, and in the preparation of the essay itself.[3] Brongniart's experience as a mining engineer would naturally make him fitter than Cuvier for the requirements of stratigraphical research.

It is not necessary for our present purpose to trace the development of view of these observers during the three years that elapsed between the appearance of their first sketch and that of their illustrated quarto memoir. It will be enough to note the general characters of their first essay, and to see how far in advance it was of anything that had preceded it.

After briefly describing the limits and general features of the Seine basin, the authors proceed to show that the

[1] *Journal des Mines*, tome xxiii. (1808), p. 421.

[2] *Essai sur la Géographie Minéralogique des Environs de Paris avec une Carte géognostique et des Coupes de terrain*, 4to, 1811. An enlarged edition of this separate work appeared in 1822.

[3] *Discours sur les Révolutions de la Surface du Globe*, 6th edit. p. 294.

formations which they have to consider were deposited in a vast bay or lake, of which the shores consisted of Chalk. They point out that the deposits took place in a definite order and could be easily recognized by their lithological and palæontological characters throughout the district. They classify them first broadly into two great groups, which they afterwards proceed to subdivide into minor sections. The first of these groups, covering the Chalk of the lower grounds, consisted partly of the plateau of limestone without shells, and partly of the abundantly shell-bearing Calcaire Grossier. The second group comprised the gypseo-marly series, not found uniformly distributed, but disposed in patches.

Starting from the Chalk of the north of France, the two observers succinctly indicate the leading characters of that deposit, its feeble stratification chiefly marked by parallel layers of dark flints, the varying distances of these layers from each other, and the distinctive fossils. Putting together the organisms they had themselves collected, and those previously obtained by Defrance, they could speak of fifty species of organic remains known to occur in the Chalk—a small number compared with what has since been found. The species had not all been determined, but some of them, such as the belemnites, had been noted as different from those found in the "compact limestone" or Jurassic series.

From the platform of Chalk, Cuvier and Brongniart worked their way upward through the succession of Tertiary formations. At the base of these, and resting immediately on the Chalk, came the Plastic Clay—a deposit that in many respects presented strong contrasts to the white calcareous formation underneath it. It showed no passage into that formation, from which, on the contrary, it was always abruptly marked off, and it yielded no organic remains. The two geologists accordingly drew the sound inference that the clay and the chalk must have been laid down under very different conditions of water, and they believed that the animals which lived in the first period did not exist in the second. They likewise concluded that the abrupt line of junction between the two formations might indicate a long interval of time, and they inferred, from the occurrence of an occasional breccia of chalk fragments at the base of the clay, that the chalk was already solid when the clay was deposited.

The next formation in ascending order was one of sand and the Calcaire Grossier. It was shown to consist of a number of bands or alternations of limestone and marl; following each other always in the same order, and traceable as far as the two observers had followed them. Some of them might diminish or disappear, but what were below in one district were never found above in another. "This constancy in the order of superposition of the thinnest strata," the writers remark, "for a distance of at least 12 myriametres (75 English miles), is in our opinion one of the most remarkable facts which we have met with in the course of our researches. It should lead to results for the arts and for geology all the more interesting that they are sure."

One of the most significant parts of the essay is the account it gives of the method adopted by the explorers to identify the various strata from district to district. They had grasped the true principle of stratigraphy, and applied it with signal success. The passage deserves to be quoted from its historical importance in the annals of science: "The means which we have employed for the recognition, among so many limestones, of a bed already observed in a distant quarter, has been taken from the nature of the fossils contained in each bed. These fossils are generally the same in corresponding beds, and present tolerably marked differences of species from one group of beds to another. It is a method of recognition which up to the present has never deceived us.

"It must not be supposed, however, that the difference in this respect between one bed and another is as sharply marked off as that between the chalk and the limestone. The characteristic fossils of one bed become less abundant in the bed above and disappear altogether in the others, or are gradually replaced by new fossils, which had not previously appeared."[1]

The authors then proceed to enumerate the chief groups of strata composing the Calcaire Grossier, beginning at the bottom and tracing the succession upward. It is not necessary to follow them into these details. We may note that, even at that time, the prodigious richness of the lower parts of this formation in fossil shells had been shown by the labours of Defrance, who had gathered from them no fewer than 600 species, which had been described by Lamarck. It was noted by Cuvier and Brongniart that most of these shells are much more unlike living forms than those found in the higher strata. These observers also drew, from the unfossiliferous nature of the highest parts of the formation, the inference that while the Calcaire Grossier was deposited slowly, layer after layer, the number

[1] *Journal des Mines*, xxiii. p. 436.

of shells gradually diminished until they disappeared, the waters either no longer containing them or being unable to preserve them.

The gypseous series which succeeds offered to Cuvier and Brongniart an excellent example of what Werner termed a "geological formation," inasmuch as it presents a succession of strata very different from each other, yet evidently deposited in one continuous sequence. Cuvier had already startled the world by his descriptions of some of the extinct quadrupeds entombed in these deposits. In calling attention to the occurrence of these animals, the authors refer to the occasional discovery of fresh-water shells in the same strata, and to the confirmation thereby afforded to the opinion of Lamanon and others, that the gypsum of Montmartre and other places around Paris had been deposited in fresh-water lakes.

They saw the importance of a thin band of marl at the top of the gypseous series which, in spite of its apparent insignificance, they had found to be traceable for a great distance. Its value arose from its marking what would now be called a lithological horizon, but even more from its stratigraphical interest, inasmuch as it served to separate a lacustrine from a marine series. All the shells below this seam were found to be fresh-water forms. Those in the seam itself were species of *Tellina*, and all those in the strata above were, like that shell, marine. The two geologists, struck by the marked difference of physical conditions represented by the two sections of the gypseous series, had tried to separate it into two formations, but had not carried out the design.

Higher up in the series, above a group of sands and marine sandstones, an unfossiliferous siliceous limestone, and a sandstone formation without shells, Cuvier and Brongniart found a widespread fresh-water siliceous limestone or millstone, specially characterised by containing *Limnea*, *Planorbis*, and other lacustrine shells.

The youngest formation which they described was the alluvium of the valleys, with bones of elephant and trunks of trees.

Subsequent research has slightly altered and greatly elaborated the arrangement made by Cuvier and Brongniart of the successive Tertiary formations of the Paris basin. But although the subdivision of the strata into definite stratigraphical and palæontological platforms has been carried into far greater detail, the broad outlines traced by them remain as true now as they were when first sketched early in the century. These two great men not merely marked out the grouping of the formations in a limited tract of country. They established on a basis of accurate observation the principles of palæontological stratigraphy. They demonstrated the use of fossils for the determination of geological chronology, and they paved the way for the enormous advances which have since been made in that department of our science. For these distinguished labours they deserve an honoured place among the Founders of Geology. Cuvier's contributions to zoology, palæontology, and comparative anatomy were so vast and important that his share in the establishment of correct stratigraphy is apt to be forgotten. But his name must ever be bracketed with that of Brongniart for the service rendered to geology by their conjoint work among the Tertiary deposits of the Paris basin.

Although Cuvier's researches among fossil animals, and the principles of comparative anatomy which he established, contributed powerfully to the foundation and development of palæontology as a distinct department of biology, his services to geology proper may be looked upon as almost wholly comprised in the joint essay with Brongniart. Geology indeed had much fascination for him, and he wrote a special treatise on it entitled *A Discourse on the Revolutions of the Surface of the Globe.*[1] But though it contained some interesting reflections on his own palæontological discoveries, and displayed the eloquence and grace of his style, it really indicated no advance in geological theory. On the contrary, in many respects it fell behind the knowledge of his time. In spite of the popularity it attained, on account of the great celebrity of its author, it cannot be cited as one of the landmarks of geological progress.

Cuvier's brilliant career is well known, but I am only concerned at present with those parts of it which touch on geological progress. In 1802 he became perpetual Secretary of the Institute, and it was in this capacity that he

[1] In its first form it was prefixed to the *Recherches sur les Ossemens Fossiles* as a preliminary discourse on the Theory of the Earth. It was afterwards published separately as the *Discours sur les Révolutions de la surface du Globe* (1826). The work went through six editions in the author's lifetime, the latest (6th) corrected and augmented by him appearing in 1830. It was translated into English and German. The versions published in England were edited and copiously annotated by Prof. Jameson of Edinburgh, whose notes to the early editions supply some curious samples of his adherence to Wernerianism. Cuvier was also the author of a Report on the Progress of the Natural Sciences, presented to the Emperor Napoleon in 1808, in which he expressed various vague and indefinite opinions on geological questions. In his earlier years his geological bias was decidedly towards Wernerianism (see the references in his *Éloge* on De Saussure already cited).

composed that remarkable series of *éloges* in which so much of the personal history of the more distinguished men of science of his time is enshrined. Eloquent and picturesque, full of knowledge and sympathy, these biographical notices form a series of the most instructive and delightful essays in the whole range of scientific literature. They include sketches of the life and work of De Saussure, Pallas, Werner, Desmarest, Sir Joseph Banks, Haüy, and Lamarck.

Five years after the appearance of the earliest conjoint memoir by Cuvier and Brongniart, the structure of the country which they described was still further explored and elucidated by a man who afterwards rose to fill a leading place among the geologists of Europe — J. J. D'Omalius d'Halloy. In 1813 this able observer read to the Institute a memoir on the geology of the Paris basin and the surrounding regions.[1] It corrected and extended the work of his predecessors among the Tertiary formations, but its interest for our present purpose centres mainly in its important contribution to the stratigraphy of the Secondary rocks. He recognized the leading subdivisions of the Cretaceous series, and actually showed the extent of the system upon a map. He likewise ascertained the stratigraphical relations and range of the Jurassic system, which he called the "old horizontal limestone," and which he correctly depicted in its course outside the Chalk. His little map, with its clear outlines and colours, is of historical importance as being the first attempt to construct a true geological map of a large tract of France. It was not a mere chart of the surface rocks,

[1] *Ann. des Mines*, i. (1817), p. 251.

like Guettard's, but had a horizontal section, which showed the Jurassic series lying unconformably upon the edges of the Palæozoic slates, and covered in turn by the Gault and the Chalk.

While in France it was the prominence and richly fossiliferous character of the Tertiary strata which led to the recognition of the value of fossils in stratigraphy, and to the definite establishment of the principles of stratigraphical geology, in England the same result was reached by a study of the Secondary formations, which are not only more extensively developed there than the younger series, but display more clearly their succession and persistence. But in both countries the lithological sequence, being the more obvious, was first established before it was confirmed and extended by a recognition of the value of the evidence of organic remains.

As far back as the year 1760, in a remarkable and well-known paper on Earthquakes, the Rev. John Michell gave a clear account of the stratified arrangement of the rocks of England, describing their general characters and the persistence of these characters for great distances, and showing that while on the flat ground the strata remain nearly level, they gradually become inclined as they approach the mountains.[1] He pointed out that the same sets of strata, in the same order, are generally met with in crossing the country towards the sea, the direction of the ridge being towards the north-north-east and south-south-west. That he was familiar with the broad features of the succession of strata in England from the Coal-measures of Yorkshire up to the Chalk is shown by an interesting

[1] *Phil. Trans.* vol. li. (1760), part ii. p. 566.

table which seems to have been drawn up by him about 1788 or 1789, and which was published after his death.[1]

Michell enables us to form a clear conception of his views by the following illustration. "Let a number of leaves of paper," he remarks, "of several different sorts or colours, be pasted upon one another; then bending them up into a ridge in the middle, conceive them to be reduced again to a level surface, by a plane so passing through them as to cut off all the part that has been raised. Let the middle now be again raised a little, and this will be a good general representation of most, if not all, large tracts of mountainous countries, together with the parts adjacent, throughout the whole world. From this formation of the earth it will follow that we ought to meet with the same kinds of earths, stones, and minerals, appearing at the surface in long narrow slips, and lying parallel to the greatest rise of any long ridge of mountains; and so, in fact, we find them."

Contrast this clear presentation of the tectonic structure of our mountains and continents with the confused and contradictory explanation of the same structure subsequently promulgated from Freiberg. Michell clearly realized that the rocks of the earth's crust had been laid down in a definite order, that they had been uplifted along the mountain axes, that they had been subsequently planed down, and that their present disposition in parallel bands was the result partly of the upheaval and partly of the denudation.

The establishment of stratigraphy in England, and of the stratigraphical sequence of the Secondary, or at least

[1] See *Phil. Mag.* vol. xxxvi. p. 102.

the Jurassic, rocks for all the rest of Europe was the work of William Smith—usually known as the "Father of English geology." No more interesting chapter in scientific annals can be found than that which traces the progress of this remarkable man, who, amidst endless obstacles and hindrances, clung to the idea which had early taken shape in his mind, and who lived to see that idea universally accepted as the guiding principle in the investigation of the geological structure, not of England only, but of Europe and of the globe.

William Smith (1769-1839) came of a race of yeomen farmers who for many generations had owned small tracts of land in Oxfordshire and Gloucestershire.[1] He was born at Churchill, in the former county, on 23rd March 1769, the same year that gave birth to Cuvier. Before he was eight years old he lost his father. After his mother married for the second time, he seems to have been largely dependent upon an uncle for education and assistance. The instruction obtainable at the village school was of the most limited kind. With difficulty the lad procured means to purchase a few books from which he might learn the rudiments of geometry and surveying. Already he had taken to the observing and collecting of stones, particularly of the well-preserved fossils whereof the Jurassic rocks of his neighbourhood were full. He came to be interested in questions of drainage and other pursuits connected with the surface of the land, and in spite of want of encouragement, made such progress with his studies that at the age of eighteen he was taken as assistant to a

[1] The biographical details are derived from the *Memoirs of William Smith, LL.D.*, by his nephew and pupil, John Phillips, 1844.

surveyor. But he had no education beyond that of the village school and what he had been able to acquire through his own reading. This early defect crippled to the end of his life his efforts to make known to the world the scientific results he had obtained.

Smith's capacity and steady powers of application were soon appreciated in the vocation upon which he had entered. Before long he was entrusted with all the ordinary work of a land surveyor, to which were added many duties that would now devolve upon a civil engineer. From an early part of his professional career, his attention was arrested by the great varieties among the soils with which he had to deal, and the connection between these soils and the strata underlying them. He had continually to traverse the red ground that marks the position of the Triassic marls and sandstones in the south-west and centre of England, and to pass thence across the clays and limestones of the Lias, or to and fro among the freestones and shales of the Oolites. The contrasts of these different kinds of rock, the variations in their characteristic scenery, and the persistence of feature which marked each band of strata gave him constant subjects of observation and reflection.

By degrees his surveying duties took him farther afield, and brought him in contact with yet older formations, particularly with the Coal-measures of Somerset and their dislocations. At the age of four-and-twenty, he was engaged in carrying out a series of levellings for a canal, and had the opportunity of confirming a suspicion, which had been gradually taking shape in his mind, that the various strata with which he was familiar, though they seemed quite flat, were really inclined at a gentle angle towards the east, and terminated sharply towards the west, like so many "slices of bread and butter." He took the liveliest interest in this matter, and felt convinced that it must have a far deeper meaning and wider application than he had yet surmised.

His first start on geological exploration took place the following year (1794) when, as engineer to a canal that was to be constructed, he was deputed to accompany two of the Committee of the Company in a tour of some weeks' duration, for the purpose of gaining information respecting the construction, management, and trade of other lines of inland navigation. The party went as far north as Newcastle, and came back through Shropshire and Wales to Bath, having travelled 900 miles on their mission. The young surveyor made full use of the opportunities which this journey afforded him. He had by this time satisfied himself that the stratigraphical succession, which he had worked out for a small part of the south-west of England, had an important bearing on scientific questions, besides many practical applications of importance. But it needed to be extended and checked by a wider experience. "No journey, purposely contrived," so he wrote, "could have better answered my purpose. To sit forward on the chaise was a favour readily granted; my eager eyes were never idle a moment; and post-haste travelling only put me upon new resources. General views, under existing circumstances, were the best that could have been taken, and the facility of knowing, by contours and other features, what might be the kind of stratification in the hills is a proof of early advancement in the generalization of phenomena.

"In the more confined views, where the roads commonly climb to the summits, as in our start from Bath to Tetbury, by Swanswick, the slow driving up the steep hills afforded me distinct views of the nature of the rocks; rushy pastures on the slopes of the hills, the rivulets and kind of trees, all aided in defining the intermediate clays; and while occasionally walking to see bridges, locks, and other works, on the lines of canal, more particular observations could be made.

"My friends being both concerned in working coal, were interested in two objects; but I had three, and the most important one to me I pursued unknown to them; though I was continually talking about the rocks and other strata, they seemed not desirous of knowing the guiding principles or objects of these remarks; and it might have been from the many hints, perhaps mainly on this subject, which I made in the course of the journey, that Mr. Palmer jocosely recommended me to write a book of hints."[1]

We can picture the trio on this memorable journey—the young man in front eagerly scrutinizing every field, ridge, and hill along each side of the way, noting every change of soil and topography, and turning round every little while, unable to restrain his exuberant pleasure as his eye detected one indication after another of the application of the principles he had found to hold good at home, and pointing them out with delight to his two sedate companions, who looked at him with amusement, but with neither knowledge of his aims nor sympathy with his enthusiasm.

For six years William Smith was engaged in setting out and superintending the construction of the Somersetshire

[1] *Memoirs*, p. 10.

Coal Canal. In the daily engrossing cares of these duties it might seem that there could be little opportunity for adding to his stores of geological knowledge, or working out in more detail the principles of stratigraphy that he had already reached. But in truth these six years were among the most important in his whole career. The constant and close observation which he was compelled to give to the strata that had to be cut through in making the canal, led him to give more special attention to the organic remains in them. From boyhood he had gathered fossils, but without connecting them definitely with the succession of the rocks that contained them. He now began to observe more carefully their distribution, and came at last to perceive that, certainly among the formations with which he had to deal, "each stratum contained organized fossils peculiar to itself, and might, in cases otherwise doubtful, be recognized and discriminated from others like it, but in a different part of the series, by examination of them."[1]

It was while engaged in the construction of this canal that Smith began to arrange his observations for publication. He had a methodical habit of writing out his notes and reflections, and dating them. But he had not the art of condensing his material, and arranging it in literary form. Nevertheless, he could not for a moment doubt that the results which he had arrived at would be acknowledged by the public to possess both scientific importance and practical value. Much of his work was inserted upon maps, wherein he traced the position and range of each of the several groups of rock with which he had become

[1] *Memoirs*, p. 15.

familiar. He had likewise ample notes of local sections, and complete evidence of a recognizable succession among the rocks. Not only could he identify the strata by their fossils, but he could point out to the surveyors, contractors, and other practical men with whom he came in contact how useful in many kinds of undertakings was the detailed knowledge which he had now acquired. In agriculture, in mining, in road-making, in draining, in the construction of canals, in questions of water-supply, and in many other affairs of everyday life, he was able to prove that his system of observation possessed great practical utility.

In the year 1799, his connection with the Canal Company came to an end. He was thereafter compelled to put his geological knowledge to commercial use, and to undertake the laborious duties of an engineer and surveyor on his own account. Eventually he found considerable employment over the whole length and breadth of England, and showed singular shrewdness and originality in dealing with the engineering questions which came before him. He was a close observer of nature, and his knowledge of natural processes stood him in good stead in his professional calling. If he had to keep out the sea from low ground, he constructed his barrier as nearly as possible like those which the waves themselves had thrown up. If he was asked to prevent a succession of landslips, he studied the geological structure of the district and the underground drainage, and drove his tunnels so as to intercept the springs underneath. His nephew and biographer tells us that his engagements in connection with drainage and irrigation involved journeys of sometimes 10,000 miles in a year.

Such continuous travelling to and fro across the country served to augment enormously his minute personal acquaintance with the geological structure of England. He made copious notes, and his retentive memory enabled him to retain a vivid recollection even of the details of what he had once seen. But the leisure which he needed in order to put his materials together seemed to flee from him. Year after year passed away; the pile of manuscript rose higher, but no progress was made in the preparation of the growing mass of material for publication.

In the year 1799, William Smith made the acquaintance of the Rev. Benjamin Richardson, who, living in Bath, had interested himself in collecting fossils from the rocks in the neighbourhood. Looking over this collection, the experienced surveyor was able to tell far more about its contents than the owner of it knew himself. Writing long afterwards to Sedgwick, Mr. Richardson narrated how Smith could decide at once from what strata they had respectively come, and how well he knew the lie of the rocks on the ground. "With the open liberality peculiar to Mr. Smith," he adds, "he wished me to communicate this to the Rev. J. Townsend of Pewsey (then in Bath), who was not less surprised at the discovery. But we were soon much more astonished by proofs of his own collecting, that whatever stratum was found in any part of England, the same remains would be found in it and no other. Mr. Townsend, who had pursued the subject forty or fifty years, and had travelled over the greater part of civilized Europe, declared it perfectly unknown to all his acquaintance, and, he believed, to all the rest of the world. In consequence of Mr. Smith's desire to make so valuable a discovery universally known, I without reserve gave a card of the English strata to Baron Rosencrantz, Dr. Müller of Christiania, and many others, in the year 1801."[1]

The card of the English strata referred to in this letter was a tabular list of the formations from the Coal up to the Chalk, with the thicknesses of the several members, an enumeration of some of their characteristic fossils, and a synopsis of their special lithological peculiarities and scenery. This table was drawn up in triplicate by Mr. Richardson, at Smith's dictation, in the year 1799, each of the friends and Mr. Townsend taking a copy. Smith's copy was presented by him to the Geological Society of London in 1831.

Though not actually published, this table obtained wide publicity. It showed that the fundamental principles of stratigraphy had been worked out by William Smith alone, and independently, before the end of last century. Had it been printed and sold it would have established his claim to priority beyond all possibility of cavil. But even without this technical support, his place among the pioneers of stratigraphy cannot be gainsaid.

Notwithstanding the abundant professional employment which he obtained, Smith never abounded in money. So keenly desirous was he to complete his investigation of the distribution of the strata of England, for the purpose of constructing a map of the country, that he spent as freely as he gained, walking, riding, or posting in directions quite out of the way of his business. "Having thus emptied his pockets for what he deemed a public object, he

[1] *Memoirs*, p. 31.

was forced to make up, by night-travelling, the time he had lost, so as not to fail in his professional engagements."

Stimulated by the kindly urgency of his friend Richardson, who alarmed him by pointing out that if he did not publish his observations, some one else might anticipate him, Smith was prevailed upon to draw up a prospectus of a work in which he proposed to give a detailed account of the various strata of England and Wales, with an accompanying map and sections. A publisher in London was named, and the prospectus was extensively circulated ; but it led to nothing.

Eventually Smith established himself in London as the best centre for his professional work, and in 1805 he took a large house there, with room for the display of his collections and maps, which were open to the inspection of any one interested in such matters. Among his materials he had completed a large county map of Somersetshire, as a specimen of what might be done for the different counties of England. This document seems to have been exhibited at the Board of Agriculture, and a proposal was made that he should be permanently attached to the corps of engineers then engaged in surveying the island. But the idea never went farther. Not until thirty years later was it revived by De la Beche, and pressed with such perseverance as to lead in the end to the establishment of the present Geological Survey of Great Britain.

From 1799, when Smith first contemplated the publication of his observations, every journey that he took was as far as possible made subservient to the completion of his map of England. At last, but not until the end of the year 1812, he found a publisher enterprising enough to undertake the risk of engraving and publishing this map. The work was begun in January 1813, and was published in August 1815.[1] It was appropriately dedicated to Sir Joseph Banks, President of the Royal Society, who had encouraged and helped the author.

William Smith's map has long since taken its place among the great classics of geological cartography. It was the first attempt to portray on such a scale not merely the distribution, but the stratigraphy of the formations of a whole country. Well might D'Aubuisson say of it that "what the most distinguished mineralogists during a period of half a century had done for a little part of Germany, had been undertaken and accomplished for the whole of England by one man; and his work, as fine in its results as it is astonishing in its extent, demonstrates that England is regularly divided into strata, the order of which is never inverted, and that the same species of fossils are found in the same stratum even at wide distances."[2]

But it is not so much as a cartographical achievement that Smith's great map deserves our attention at present. Its appearance marked a distinct epoch in stratigraphical geology, for from that time some of what are now the most familiar terms in geological nomenclature passed into

[1] "A Geological Map of England and Wales, with Part of Scotland; exhibiting the Collieries, Mines, and Canals, the Marshes and Fen Lands originally overflowed by the Sea; and the Varieties of Soil, according to the Variations of the Substrata; illustrated by the most descriptive Names of Places, and of Local Districts; showing also the Rivers, Sites of Parks, and Principal Seats of the Nobility and Gentry; and the opposite Coast of France. By William Smith, Mineral Surveyor." The map consists of fifteen sheets on the scale of five miles to an inch, and measures 8 feet 9 inches in height by 6 feet 2 inches in width. It was accompanied with a quarto memoir or explanation of 50 pages.

[2] *Traité de Géognosie* (1819), tome ii. p. 253.

common use. Smith had no scholarship; he did not invent euphonious terms from Greek or Latin roots; he was content to take the rustic or provincial names he found in common use over the districts which he traversed. Hence were now introduced into geological literature such words as London Clay, Kentish Rag, Purbeck Stone, Carstone, Cornbrash, Clunch Clay, Lias, Forest Marble.

By ingeniously colouring the bottom of each formation a fuller tint than the rest, Smith brought the general succession of strata conspicuously before the eye. Further, by the aid of vertical tables of the formations and a horizontal section from Wales to the vale of the Thames, he was able to give the details of the succession which, for some twenty-four years, he had been engaged in unravelling in every part of the kingdom.

Of especial value and originality was his clear subdivision of what is now known as the Jurassic system. He did for that section of the geological record what Cuvier and Brongniart had done for the Tertiary series of Paris. After the first copies of the map had been issued, he was able still further to subdivide and improve his classification of these strata, introducing among the new bands, Crag, Portland Rock, Coral Rag, and Kellaways Stone.[1]

In the memoir accompanying the map, the tabular arrangement of the strata drawn up in 1799 was inserted, with its column giving the names, so far as he knew them, of the more characteristic fossils of each formation.

To the laborious researches of William Smith we are thus indebted for the first attempt to distinguish the

[1] Phillips, *Memoirs*, p. 146.

various subdivisions of the Secondary rocks, from the base of the New Red Sandstone up to the Chalk, and for the demonstration that these successive platforms are marked off from each other, not merely by mineral characters, but by their peculiar assemblages of organic remains. From his provincial terminology come the more sonorous names of Purbeckian, Portlandian, Callovian, Corallian, Bathonian, Liassic, which are now familiar words in every geological text-book.[1]

In his eagerness to make his map as complete and accurate as was possible to him, Smith spent so freely of his hardly-earned income that he accumulated no savings against the day of trial, which came only too soon. He had been induced to lay down a railway on a little property which early in life he had purchased near Bath, with the view of opening some new quarries and bringing the building-stone to the barges on the canal. Unfortun-

[1] Before passing from the subject of Smith's map I may refer to the map of England and Wales which was prepared by G. B. Greenough and published in 1819. Greenough, in the memoir accompanying this map, states that though he knew as early as 1804 that Smith had begun a similar work, he had been led to believe that the design was abandoned. Accordingly he undertook the task in 1808, and having been encouraged by the Geological Society, of which he was President, to complete it on a large scale, he proceeded with it, and the map as prepared by him had been more than a year in the hands of the engraver when Smith's map appeared in 1815. Greenough's is a better piece of engraving, and in some respects is more detailed, especially as regards the formations older than the Coal. It shows how much information as to English stratigraphy had become available, partly no doubt through Smith's labours before 1815. Greenough's map was taken over by the Geological Society, has been as far as possible kept up to date, and is still on sale. But the map in its present form differs much from its author's original version. The appearance of this map under the sanction of the Geological Society seems to have affected the sale of Smith's, which does not appear to have reached a second edition. A much reduced version of it was published in 1820.

ately the stone, on the continuance and quality of which the whole success of the enterprise rested, failed. It became necessary to sell the property, and thereafter the sanguine engineer was left with a load of debt under which most men would have succumbed. Struggling under this blow, he was first compelled to part with his collections of fossils, which were acquired by the Government and placed in the British Museum. Next he found himself no longer able to bear the expense of the house in London which he had occupied for fifteen years. Not only so, but hard fate drove him to sell all his furniture, books and other property, keeping only the maps, sections, drawings and piles of manuscript which were so precious in his own eyes, but for which nobody would have been likely to give him anything. For seven years he had no home, but wandered over the north of England, wherever professional engagements might carry him. His income was diminished and fluctuating, yet even under this cloud of trial he retained his quiet courage and his enthusiasm for geological exploration.

That a man of Smith's genius should have been allowed to remain in this condition of toil and poverty has been brought forward as a reproach to his fellow-countrymen. It may be doubted, however, whether a man of his strong independence of character would have accepted any pecuniary assistance, so long as he could himself gain by his own exertions a modest though uncertain income. It is not that his merits were unrecognized in England, though perhaps the appreciation of them was tardier than it might have been. In 1818 a full and generous tribute to his merits was written by Fitton, and appeared in the *Edin-*

burgh Review for February in that year.[1] But though his fame was thus well established, his financial position remained precarious. He had gradually formed a consulting practice as a mineral and geological surveyor in the north of England, and he eventually settled at Scarborough. From 1828 to 1834 he acted as land-steward on the estate of Hackness in the same district of Yorkshire. In 1831 he received from the Geological Society the first Wollaston Medal, and the President of the Society, Adam Sedgwick, seized the occasion to proclaim, in fervid and eloquent words, the admiration and gratitude of all the geologists of England towards the man whom he named "the father of English geology." Next year a pension of £100 was conferred upon him. Honours now came to him in abundance. But his scientific race was run. He continued to increase his piles of manuscript, but without methodically digesting them for publication. He died on 28th August 1839, in the seventy-first year of his age.

William Smith was tall and broadly built, like the English yeomen from whom he came. His face was that of an honest, sagacious farmer, whose broad brow and firm lips betokened great capacity and decision, but would hardly have suggested the enthusiastic student of science. His work, indeed, bears out the impression conveyed by his portrait. His plain, solid, matter-of-fact intellect never

[1] At the end of 1817 there seems to have been some inquiry as to priority of discovery in regard to Smith's work. In the following March Mr. John Farey contributed to Tilloch's *Philosophical Magazine* a definite statement of Smith's claims, showing that the fundamental facts and principles he had established had been freely made known by him to many people as far back as 1795, and that Farey himself, on 5th August 1807, had published an explicit notification of Smith's discoveries and conclusions as to fossil shells in the article on Coal in Rees' *Cyclopedia*.

branched into theory or speculation, but occupied itself wholly in the observation of facts. His range of geological vision was as limited as his general acquirements. He had reached early in life the conclusions on which his fame rests, and he never advanced beyond them. His whole life was dedicated to the task of extending his stratigraphical principles to every part of England. But this extension, though of the utmost importance to the country in which he laboured, was only of secondary value in the progress of science.

Besides his great map of England, Smith published also a series of geological maps, on a larger scale, of the English counties, comprising in some instances much detailed local information. He likewise issued a series of striking horizontal sections (1819) across different parts of England, in which the succession of the formations was clearly depicted. These sections may be regarded as the complement of his map, and as thus establishing for all time the essential features of English stratigraphy, and the main outlines of the sequence of the Secondary formations for the rest of Europe. In another publication, *Strata Identified by Organized Fossils* (1816), he gave a series of plates, with excellent engraved figures of characteristic fossils from the several formations. He adopted in this work the odd conceit of having the plates printed on variously coloured paper, to correspond with the prevalent tint of the strata from which the fossils came. He had no palæontological knowledge, so that the thin quarto, never completed, is chiefly of interest as a record of the organisms that he had found most useful in establishing the succession of the formations.

There is yet another name that deserves to be remembered in any review of the early efforts to group the Secondary formations—that of Thomas Webster. As far back as 1811, this clever artist and keen-eyed geologist began a series of investigations of the coast-sections of the Isle of Wight and of Dorset; and continued them for three years. They were published in 1815, the same year that saw Smith's map make its appearance.[1] They were thus independent of that work. Webster had already studied the Tertiary formations of the Isle of Wight and recognized their alternations of fresh-water and marine strata,[2] as had been done in the Paris basin. He now threw into tabular arrangement the whole succession of strata from the upper fresh-water (oligocene) group through the Lower Tertiary series to the Kimmeridge shale in the Jurassic system. He clearly defined each of the leading subdivisions of the Cretaceous series, and prepared the way for the admirable later and more detailed work of Fitton.

Before passing from the cartographical achievements of the earlier decades of this century, I must briefly allude to the remarkable maps and descriptions of Scotland for which geology is indebted to the genius and strenuous labour of John Macculloch. His account of the structure of the Western Isles, and the excellent maps and sections which accompanied it, had a powerful influence in promoting the progress of the study of igneous rocks, and have long since taken their place as classics in geological literature. The same indefatigable observer, after years of toil, prepared a geological map of the whole of Scotland—perhaps the

[1] See Englefield's *Isle of Wight* (1815), p. 117.

[2] *Trans. Geol. Soc.* vol. ii.

most remarkable achievement of the kind which up to that time had been accomplished by a single individual.[1]

We have now traced the slow and somewhat fitful progress of stratigraphical geology during the two last decades of the eighteenth century and the first two decades of the present. From the youngest alluvial deposits, through the Tertiary and Secondary formations, down to the Carboniferous system, the clue had been found by which the strata could be identified from one district and one country to another. A prodigious impetus was now given to the study of geology. The various stratified formations, arranged in their true chronological order, were now seen to contain the regular and decipherable records of the history of our globe, which could be put together with as much certainty as the faded manuscripts of human workmanship. The organic remains contained in them were found to be not random accumulations, heaped together by the catastrophes of bygone ages, but orderly chronicles of old sea-floors, lake-bottoms, and land-surfaces. The centre of gravity of geology was now rapidly altered, especially in France and in Britain. Minerals and rocks no longer monopolized the attention of those who interested themselves in the crust of the earth. The petrified remains of former plants and animals ceased to be mere curiosities. Their meaning as historical documents was at last realized. They were seen to have a double interest, for while they told the story of the successive vicissitudes which the surface of the earth had undergone, from remote ages down to the

[1] *A Description of the Western Isles of Scotland*, 1819; A map of Scotland, 1840; and *Memoirs to His Majesty's Treasury respecting the Geological Map of Scotland*, by J. Macculloch, 1836.

present, they likewise unfolded an altogether new and marvellous panorama of the progress of life upon that surface. They had hitherto shared with minerals and rocks the usage of the term "fossil." As their importance grew, they were discriminated as "organized fossils." But the rising tide of awakened interest soon swept away the qualifying participle, and the organic remains became sole possessors of the term, as if they were the only objects dug out of the earth that were any longer worthy to be denominated fossils.

While the whole science of geology has made gigantic advances during the nineteenth century, by far the most astonishing progress has sprung from the recognition of the value of fossils. To that source may be traced the prodigious development of stratigraphy over the whole world, the power of working out the geological history of a country, and of comparing it with the history of other countries, the possibility of tracing the synchronism and the sequence of the geographical changes of the earth's surface since life first appeared upon the planet. To the same source, also, we are indebted for the rise of the science of palæontology, and the splendid contributions it has made to biological investigation. In the midst of the profusion, alike of blossom and of fruit, let us not forget the work of those who sowed the seed of the abundant harvest which we are now reaping. Let us remember the early suggestive essays of Guettard, the pregnant ideas of Lehmann and Fuchsel, the prescient pages of Giraud-Soulavie, the brilliant work of Cuvier and Brongniart, and the patient and clear-sighted enthusiasm of William Smith.

LECTURE VI

The Transition or Greywacke formation resolved by Sedgwick and Murchison into the Cambrian, Silurian and Devonian systems —The pre-Cambrian rocks first begun to be set in order by Logan—Foundation of Glacial Geology, Agassiz—Rise of modern Petrography; William Nicol, Henry Clifton Sorby —The influence of Lyell and Darwin—Conclusion.

The determination of the value of fossils as chronological documents has done more than any other discovery to change the character and accelerate the progress of geological inquiry. No contrast can be more striking than the difference between the condition of the science before and after that discovery was made. Before that time, when the Wernerian classification of the rocks of the earth's crust everywhere prevailed, there was really little stimulus to investigate these rocks in their chronological relations to each other. They were grouped, indeed, in a certain order, which was believed to express their succession in time, but their identification from one country to another proceeded on no minute study of their internal structure, their fossil contents, or their tectonic relations. It was thought enough if they could be placed in one or other of the divisions of the Freiberg system. When an orthodox disciple of Werner had relegated a mass of deposits to the

Transition series, or the Floetz or the Independent Coal-formation, as the case might be, he considered that all that was really essential had been ascertained, and his interest in the matter came practically to an end.

But the extraordinary awakening which resulted from the labours of Cuvier, Brongniart and William Smith, invested the strata with a new meaning. As stratigraphical investigations multiplied, the artificiality and inadequacy of the Wernerian arrangement became every day more apparent. Even more serious than the attacks of the Vulcanists, and the disclosure of eruptive granites and porphyries among the Transition rocks, were the discoveries made among the fossiliferous stratified formations. It was no longer possible to crowd and crush these rocks within the narrow limits of the Wernerian system, even in its most modified and improved form. The necessity for expansion and for adopting a perfectly natural nomenclature and classification, based upon the actually observed facts, as these were successively ascertained, made itself felt especially in England and in France. Hence arose the curiously mongrel terminology which is now in use. Certain formations were named from some prominent mineral in them, such as Carboniferous. Others were discriminated by some conspicuous variety of rock, like the Cretaceous series. Some took their names from a characteristic structure, like Oolitic, others from their relative position in the whole series, as in the case of Old Red Sandstone and New Red Sandstone. Certain terms betrayed the country of their origin, as did William Smith's English provincial names, like Gault, Kellaways rock, and Lias.

The growth of stratigraphical nomenclature is thus eminently characteristic of the early rise and progress of the study of stratigraphy in Europe. Precisians decry this inartificial and haphazard language, and would like to introduce a brand new harmonious and systematic terminology. But the present arrangement has its historical interest and value, and so long as it is convenient and intelligible, I do not see that any advantage to science would accrue from its abolition. The method of naming formations or groups of strata after districts where they are typically developed has long been in use and has many advantages, but it has not supplanted all the original names, and I for my part hope that it never will.

With regard to what are now known as the Tertiary and Secondary formations, the Wernerian "Floetz," under which they were all comprised, soon sank into disuse.[1] But there was a long pause before the strata of older date were subjected to the same diligent study. For this delay various good reasons may be assigned. We have seen that William Smith's researches went down into the Coal-measures, but he had only a general and somewhat vague idea of the sequence of the rocks beneath that formation. These rocks were not wholly unfossiliferous, but in general, throughout Western Europe, they had been so disturbed and dislocated that they no longer presented the proofs of

[1] One of the latest adaptations of the word was that of Keferstein in his *Tabellen über die vergleichende Geologie* (1825). He frankly threw over Wernerianism, but stuck to the pre-Wernerian Floetz, which he arranged in five subdivisions. (1) Youngest Floetz,—alluvium, etc.; (2) Tertiary Floetz,—marls, gypsum, etc., of Paris, Brown coal; (3) Younger Floetz, or Chalk rocks,—Chalk, Jura Limestone, Greensand; (4) Middle Floetz, or Muschelkalk—Lias, Kenper marl, Bunter sandstone, Zechstein; (5) Old Floetz, or Mountain Limestone—Coal, Mountain Limestone.

their sequence in the same orderly manner as had led to the recognition of the succession of the younger formations.

It will be remembered that in his original scheme of classification Werner grouped some rocks as Primitive (*uranfängliche*), and classed together as Floetz the whole series of stratified formations between these and the alluvial deposits. Further experience led him to separate an intermediate group between the Primitive and the Floetz, which he denominated Transition. He believed that this group was "deposited during the passage or transition of the earth from its chaotic to its habitable state."[1] He recognized that it contains the earliest organic remains, and believed it to include the oldest mechanical deposits. He subdivided the Transition rocks rather by mineral characters than by ascertained stratigraphical sequence. The hardened variety of sandstone called greywacke formed by far the most important member of the whole series, and was believed by Werner to mark a new geognostic period when, instead of chemical precipitates, mechanical accumulations began to appear.

The two Wernerian terms Transition and Greywacke survived for some years after the commencement of the great stratigraphical revival in the early years of the present century. They formed a kind of convenient limbo or No-man's Land, into which any group of rocks might be thrown which obstinately refused to reveal its relations with the rest of the terrestrial crust. Down to the base of the Carboniferous rocks, or even to the bottom of the Old Red Sandstone, the chronological succession of geological history seemed tolerably clear. But beneath and beyond

[1] Jameson's *Geognosy*, p. 145 (1808).

that limit, everything betokened disorder. It appeared well-nigh hopeless to expect that rocks so broken and indurated, generally so poor in fossils, and usually so sharply cut off from everything younger than themselves, would ever be made to yield up a connected and consistent series of chapters to the geological record.

And yet these chapters, if only they could be written, would be found to possess the most vivid interest. They would contain the chronicles of the earlier ages of the earth's history, and might be expected to reveal the geography of the first dry land, the sites of the most ancient seas, the positions of the oldest volcanoes, the forms of the first plants and animals that appeared upon the planet. There was thus inducement enough to attack the old rocks that contained within their stony layers such precious memorials.

It is not that the Transition rocks were entirely neglected. The keen interest awakened in fossils led to renewed search among the fossiliferous members of that ancient series. A large number of organic remains had been collected from Devonshire, Wales, the Lake District, Rhineland, the Eifel, France, Sweden, Norway, Russia, as well as from New York and Canada. These fossils were distinct from those of the Secondary formations, and they were obviously distributed, not at random, but in groups which reappeared at widely separated localities.[1]

[1] The amount and nature of the information in existence regarding the Transition rocks or Greywacke, at the time when Murchison entered upon their investigation, may be gathered from the summaries contained in the contemporary general treatises on Geology. Even as late as the spring of 1833, Lyell, after devoting about 300 pages to the Tertiary formations, dismissed the Palæozoic series in twelve lines (*Principles of Geology*, vol. iii. (1833), p. 326). One of the fullest descriptions of the older fossiliferous

As yet, however, no clue had been found to their stratigraphical sequence. Specimens from what are now known as Cambrian, Silurian, Devonian, and even Lower Carboniferous strata were all thrown together as coming from the undefined region of the Greywacke or Transition rocks. A task worthy of the best energy of the most accomplished geologist lay open to any man bold enough to undertake to introduce among these rocks the same stratigraphical method which had reduced the Secondary and Tertiary formations to such admirable order, and had furnished the means of comparing and correlating these formations from one region to another. This task was at last accomplished by two men, working independently of each other in Wales and the border counties of England. Murchison and Sedgwick carried the principles of Cuvier, Brongniart and William Smith into the chaos of old Greywacke, and succeeded in adding the Devonian, Silurian and Cambrian chapters to the geological record, thus establishing a definite order among the oldest fossiliferous formations, and completing thereby Palæozoic stratigraphy.

Roderick Impey Murchison (1792-1871) belonged to a family that had lived for centuries among the wilds of the north-western Highlands of Scotland and had taken part in much of the rough life of that remote and savage region.[1] He was born in 1792, entered the army when he was only fifteen years of age, and served for a time in the Peninsular

rocks, with copious lists of fossils, will be found in the first edition of De la Beche's *Geological Manual* (1831), p. 433, under the head of "Grauwacke Group." But no attempt is there made to arrange the rocks stratigraphically, and the fossil lists comprise organisms from all the older Palæozoic formations without discrimination of their horizon.

[1] The biographical details are taken from my *Life of Sir Roderick I. Murchison*, 2 vols. 8vo, 1875.

war. He carried the colours of his regiment at the battle of Vimieira, took part in the retreat to Corunna and narrowly escaped being taken prisoner by the French. On the conclusion of the Napoleonic wars, seeing no longer any prospect of military activity or distinction, he quitted the army, married, and for some years devoted himself with ardour to fox-hunting, in which his love of an open-air life and of vigorous exercise could have full gratification. But he was made for a nobler kind of existence than that of a mere Nimrod. His wife, a woman of cultivated tastes, had led him to take much interest in art and antiquities, and when Sir Humphry Davy, who also recognized his qualities, urged him to turn his attention to science, she strenuously encouraged him to follow the advice. He at last sold his hunters, came to London, and began to attend lectures on chemistry and geology at the Royal Institution.

Murchison was thirty-two years old before he showed any interest in science. But his ardent and active temperament spurred him on. His enthusiasm was thoroughly aroused, and his progress became rapid. He joined the Geological Society, and having gained the goodwill of Buckland, went down to Oxford for his first geological excursions under the guidance of that genial professor. He then discovered what field-geology meant, and learnt how the several parts of a landscape depend for their position and form upon the nature of the rocks underneath. He returned to London with his zeal aflame, burning to put into practice the principles of observation he had now been taught. He began among the Cretaceous formations around his father-in-law's home in Sussex, but soon extended his explorations into Scotland, France and the

Alps, bringing back with him at the end of each season a bundle of well-filled note-books from which to prepare communications for the Geological Society. These early papers, meritorious though they were, do not call for any special notice here, since they marked no new departure in geological research, nor added any important province to the geological domain.

During six years of constant activity in the field, Murchison worked out with Sedgwick the structure of parts of the west and north of Scotland and toiled hard in disentangling the complicated structure of the eastern Alps; he also rambled with Lyell over the volcanic areas of Central and Southern France. Thereafter he determined to try whether the "interminable greywacke," as he called it, could not be reduced to order and made to yield a stratigraphical sequence, like that which had been so successfully obtained among younger formations. At the time when he began, that is, in the summer of 1831, absolutely nothing was known of the succession of rocks below the Old Red Sandstone. It was an unknown land, a pathless desert, where no previous traveller had been able to detect any trace of a practicable track towards order, or any clue to a system of arrangement that would enable the older fossiliferous rocks of one country to be paralleled with those of another.

Starting with his "wife and maid, two good grey nags and a little carriage, saddles being strapped behind for occasional equestrian use," Murchison made his way into South Wales. In that region, as was well known, the stratigraphical series could be followed down into the Old Red Sandstone, and within the frame or border of that formation, greywacke was believed to extend over all the rest of the Principality. Let me quote a few sentences in which Murchison describes his first entry into the domain with which his fame is now so inseparably linked. "Travelling from Brecon to Builth by the Herefordshire road, the gorge in which the Wye flows first developed what I had not till then seen. Low terrace-shaped ridges of grey rock, dipping slightly to the south-east, appeared on the opposite bank of the Wye, and seemed to rise quite conformably from beneath the Old Red Sandstone of Herefordshire. Boating across the river at Cavansham Ferry, I rushed up to these ridges, and, to my inexpressible joy, found them replete with Transition fossils, afterwards identified with those at Ludlow. Here then was a key, and if I could only follow this out on the strike of the beds to the north-east, the case would be good."[1]

With unerring instinct Murchison had realized that if the story of old Greywacke was ever to be told, a beginning must be made from some known and recognizable horizon. It would have been well-nigh useless to dive into the heart of the Transition hills and try to work out their complicated structure, for even if a sequence could then have been determined, there would have been no means of connecting it with the already ascertained stratigraphical series, unless it could be followed outwards to the Old Red Sandstone. But by commencing at the known base of that series, every stage conquered was at once a definite platform added to what had already been established.

The explorer kept along the track of the rocks for

[1] *Life*, vol. i. p. 182.

many miles to the north. No hunter could have followed the scent of the fox better than he did the outcrop of the fossiliferous strata, which he saw to come out regularly from under the lowest members of the Old Red Sandstone. Directed to the Wye by Buckland, he had the good-fortune to come at once upon some of the few natural sections where the order of the higher Transition rocks of Britain, and their relations to the overlying formations, can be distinctly seen. He pursued the chase northwards until he lost the old rocks under the Triassic plains of Cheshire. "For a first survey," he writes, "I had got the upper grauwacke, so called, into my hands, for I had seen it in several situations far from each other, all along the South Welsh frontier, and in Shropshire and Herefordshire, rising out gradually and conformably from beneath the lowest member of the Old Red Sandstone. Moreover, I had ascertained that its different beds were characterized by peculiar fossils, . . . a new step in British geology. In summing up what I saw and realized in about four months of travelling, I may say that it was the most fruitful year of my life, for in it I laid the foundation of my Silurian system. I was then thirty-nine years old, and few could excel me in bodily and mental activity." [1]

Not only did the work of these four momentous months mark a new step in British geology. It began the lifting of the veil from the Transition rocks of the whole globe. It was the first successful foray into these hitherto intractable masses, and prepared the way for all that has since been done in deciphering the history of the most ancient

[1] *Op. cit.* pp. 183, 192.

fossiliferous formations, alike in the Old World and in the New.

Contenting himself with a mere announcement of his chief results at the first meeting of the British Association, held in York in 1831, Murchison gave a brief outline of his subdivisions of the upper Greywacke to the Geological Society in the spring of 1833.[1] He continued to toil hard in the field, mapping on the ground his various formations, and making careful sections of their relations to each other. Every fresh traverse confirmed the general accuracy of his first observations, and supplied him with further illustrations of the persistence and distinctness of the several groups into which he had subdivided the greywacke. At the beginning of 1834, he was able to present a revised and corrected table of his stratigraphical results, each formation being defined by its lithological characters and organic remains, and the subdivisions being nearly what they still remain.[2] The Ludlow rocks are shown to pass upward into the base of the Old Red Sandstone, and downward into the Wenlock group, which in turn is succeeded below by the Horderley and May Hill rocks, followed by the Builth and Llandeilo flags. By the summer of 1835, at the instigation of Élie de Beaumont and other geological friends, he had made up his mind as to the name that should be given to this remarkable assemblage of formations which he had disinterred from out of the chaos of greywacke. Following the good rule that stratigraphical terms are most fitly formed on a geographical basis with reference to the regions wherein the rocks are most typically developed, he

[1] *Proc. Geol. Soc.* vol. i. (1833), p. 474.
[2] *Ibid.* vol. ii. (1834), p. 11.

had looked about for some appropriate and euphonious term that would comprise his various formations and connect them with that borderland of England and Wales where they are so copiously displayed. This territory was in Roman times inhabited by the tribe of the Silures, and so he chose the term Silurian—a word that is now familiar to the geologists of every country.[1]

At the same time Murchison published a diagrammatic section of his classification which, except in one particular, has been entirely sustained by subsequent investigation. He there groups the whole series of formations as the Silurian system, which he divides into Upper and Lower, drawing the line of separation where it still remains. In the upper section come the Ludlow and Wenlock rocks; in the lower the Caradoc and Llandeilo. The base of the series, however, is made to rest unconformably on a series of ancient slaty greywackes. No such base exists, for the Llandeilo group passes downward into a vast series of older sediments. At that time, however, both Murchison and Sedgwick believed that a strongly marked separation lay between the Silurian System and the rocks lying to the west of it.

Murchison used to maintain, with perfect justice, that he had succeeded in his task, because he had followed the method which had led William Smith to arrange so admirably the Secondary formations of England. He was able to show that, apart from mere lithological differences, which might be of only local value, his formations were definitely characterized, each by its peculiar assemblage of organic remains. If Smith's labours had not only brought

[1] *Phil. Mag.* July 1835, p. 48.

the Mesozoic rocks of England into order, but had furnished a means of dealing in like fashion with the rocks of the same age in other countries, there seemed no reason why the palæontological succession, found to distinguish the greywacke in England and Wales, should not be equally serviceable among the Transition rocks of Europe and even of America. Murchison had thus added a series of new and earlier chapters to the geological history of the globe.

The various brief communications to the Geological Society, after the first discoveries in 1831, though they had made geologists familiar with the main results of Murchison's work, only increased their desire to know the detailed observations on which it was founded, and more particularly to have complete information as to the assemblages of organic remains which he had discovered. Previous collections from the Transition rocks were generally of little service for stratigraphical purposes, because those of widely separate horizons had all been mixed together. But Murchison's specimens had been carefully gathered, with the view of sustaining his classification, and for the purpose of forming a basis of comparison between the Transition rocks of Britain and those of other countries. Early in the course of his wanderings along the Welsh border, he had been urged to prepare a full and more generally accessible account of his labours than was offered in the publications of a learned Society. Accordingly, adding this task to his other engagements, he toiled at the making of a big book, until at last, towards the end of the year 1838, that is, about seven years from the time when he broke ground by the banks of the Wye, he published his great work, *The Silurian System*, a massive quarto of 800 pages, with an atlas of plates of fossils and sections, and a large coloured geological map.

The publication of this splendid monograph forms a notable epoch in the history of modern geology, and well entitles its author to be enrolled among the founders of the science. For the first time, the succession of fossiliferous formations below the Old Red Sandstone was shown in detail. Their fossils were enumerated, described and figured. It was now possible to carry the vision across a vast series of ages, of which hitherto no definite knowledge existed, to mark the succession of their organisms, and thus to trace backward, far farther than had ever before been possible, the history of organized existence on this globe.

While carefully working out the stratigraphy of the rocks, Murchison had come upon various masses of eruptive rock. Some of these he recognized as intrusive, others he saw to be lavas and ashes which had been ejected over the floor of the ancient ocean. In this way he was able to present a picture of extraordinary interest, in which the geologist could mark the position of the old seas, trace the distribution of their organisms, and note the sites of their volcanoes.

Even before the advent of his volume, his remarkable results had become widely known, and had incited other observers all over the world to attack the forbidding domain of Greywacke. In France, his classification had been adopted, and applied to the elucidation of the older fossiliferous rocks by Élie de Beaumont and Dufrénoy, who were engaged in constructing a geological map of that country. In Turkey it had been similarly made available by Boué and De Verneuil. Forchhammer had extended it to Scandinavia. Featherstonehaugh and Rogers had applied it in the United States. Thus within a few years, the Silurian system was found to be developed in all parts of the world, and Murchison's work furnished the key to its interpretation.

Let us now turn to the researches that were in progress by another great master of English geology, simultaneously with those of Murchison. Adam Sedgwick (1785-1873) belonged to a family that had been settled for 300 years or more in the Dale of Dent, a picturesque district which lies along the western border of Yorkshire. To the end of his long and active life his heart ever turned with fondness to the little valley where he first saw the light, and to the kindly dalesmen among whom he spent his boyhood. He remained to the end a true dalesman himself, with all the frankness of nature, mirthfulness and loyalty, so often found among the natives of these pastoral uplands. He was born in the year 1785, his father being the Vicar of Dent. After receiving his school education at the neighbouring little town of Sedbergh, he went to Trinity College, Cambridge, which thenceforth became his home to the end of his life. At the age of thirty-three he was elected to the Woodwardian Professorship of Geology. Up to that time, however, he had shown no special interest in geological pursuits, and though he may have read a little on the subject, his knowledge of it was probably not greater than that of the average college Fellow of his day. His appointment as Professor, however, awakened his dormant scientific proclivities, and he at once threw himself with all his energy and enthusiasm into the duties of his new vocation. Gifted with mental power of no common order, which had been sedulously trained in a wide range of studies, with a keen eye for the geological structure of a region, and with abundant bodily prowess to sustain him in the most arduous exertions in the field, eloquent, witty, vivacious, he took at once the place of prominence in the University which he retained to the last, and he came with rapid strides to the front of all who in that day cultivated the infant science of geology in England.

What little geology Sedgwick knew, when he became a professor of the science, seems to have been of a decidedly Wernerian kind. He began his geological writings with an account of the primitive ridge and its associated rocks in Devon and Cornwall. His earliest paper might have been appropriately printed in the first volume of the *Memoirs of the Wernerian Society*. In later years, referring to his Neptunist beginnings, he confessed that "for a long while I was troubled with water on the brain, but light and heat have completely dissipated it," and he spoke of "the Wernerian nonsense I learnt in my youth."[1] It was by his own diligent work in the field that he came to a true perception of geological principles. His excursions carried him all over England, and enabled him to bring back each season a quantity of specimens for his museum, and a multitude of notes from which he regaled the Cambridge Philosophical Society with an account of his doings. Eventually he joined the Geological Society of London,

[1] *Life and Letters of Adam Sedgwick*, by J. W. Clark and T. M'K. Hughes, vol. i. p. 284.

and found there a wider field of action. After a time Murchison also became a fellow of the Society, and he and Sedgwick soon formed a close intimacy. This friendship proved to be of signal service to the cause of geological progress. The two associates were drawn towards the same departments of investigation. They began their co-operation in the year 1827 by a journey through the west and north of Scotland, and from that time onward for many years they were constantly working together in Britain and on the Continent of Europe.

It would be interesting, but out of place here, to linger over the various conjoint labours of these two great pioneers in Palæozoic geology. We are only concerned with what they did, separately and in conjunction, towards the enlargement of the geological record and the definite establishment of the Palæozoic systems. Sedgwick began his work among the older fossiliferous formations by attacking the rugged and complicated region of Cumberland and Westmoreland, commonly known as the Lake District, and in a series of papers communicated to the Geological Society he worked out the general structure of that difficult tract of country. Though fossils had been found in the rocks, he did not at first make use of them for purposes of stratigraphical classification. He ascertained the succession of the great groups of strata by noting their lithological characters. One of the most remarkable features of his investigation was the recognition of volcanic rocks intercalated among the ancient marine sediments of the Lake District. These rocks, since so fully worked out, and now known as the "Borrowdale Volcanic Series," of Lower Silurian age, were first assigned to their true origin by Sedgwick, who thus made an important contribution to the progress of volcanic geology.

By a curious coincidence, Sedgwick and Murchison broke ground in Wales during the same summer of 1831. But while Murchison determined to work his way downward, from the known horizons of the Old Red Sandstone of South Wales into the greywacke below, Sedgwick, with characteristic dash, made straight for the highest, ruggedest and most complicated tract of North Wales. Returning to the same ground the following year, he plunged into the intricacies of the older Palæozoic rocks, and succeeded in disentangling their structure, tracing out their flexures and dislocations, and ascertaining the general sequence of their principal subdivisions. It was a splendid achievement, which probably no other man in England at that time could have accomplished.

But valuable as this work was, as a contribution to the elucidation of the tectonic geology of a part of Britain, it had not yet acquired importance in general stratigraphy. In the first place, Sedgwick's groups of strata were mere lithological aggregates. They possessed as yet no distinctive characters that would allow of their being adopted in the interpretation of other countries, or even of distant parts of Britain. They contained fossils, but these had not been made use of in defining the subdivisions. There was thus neither a basis for comparison with other regions, nor for the ascertainment of the true position of the North Welsh rocks in the great territory of Greywacke. In the second place, there was no clue to the connection of these rocks with any known formation, for they were separated from everything younger than themselves by a strong unconformability. The Carboniferous and Old Red Sandstone strata were found to lie on the upturned edges of the older masses, and it was impossible to say how many intervening formations were missing.

Murchison's researches brought to light the actual transition from the base of the Old Red Sandstone into an older series of fossiliferous formations underneath. There could, therefore, be no doubt that part at least of his Silurian system was younger than Sedgwick's series in North Wales. And as he found what appeared to be older strata emerging from underneath his system, and seeming to stretch indefinitely into the heart of Wales, he naturally believed these strata to be part of his friend's domain, and at first left them alone. Such, too, was Sedgwick's original impression. The two fellow-workers had not drawn a definite boundary between their respective territories, but they agreed that the Silurian series was less ancient than the rocks of North Wales.

As a distinct name had been given to the younger series, Murchison urged his associate to choose an appropriate designation for the older, and in the summer of 1835 the term "Cambrian" was selected.[1] By this time Murchison had learnt that no hard and fast line was to be drawn between the bottom of the Silurian and the top of the Cambrian series. "In South Wales he had traced many distinct passages from the lowest member of the 'Silurian system' into the underlying slaty rocks now

[1] Brit. Assoc. August 1835, *Phil. Mag.* vol. vii. (December 1835), p. 483, "On the Silurian and Cambrian Systems" by A. Sedgwick and R. I. Murchison.

named by Professor Sedgwick the Upper Cambrian." Sedgwick, on the other hand, confessed that neither in the Lake District nor in North Wales was the stratigraphical succession unbroken, and that in these regions it was impossible to tell "how many terms are wanting to complete the series to the Old Red Sandstone and Carboniferous Limestone."[1] He adopted a threefold subdivision into Lower, Middle, and Upper Cambrian, but this classification rested merely on mineral characters, no attempt having yet been made by Sedgwick to determine how far each of his subdivisions was defined by distinctive fossils.

Eventually it was ascertained that the organic remains in the upper part of the Cambrian system were the same as those found in the Lower Silurian formations as defined by Murchison. It was obvious that the one series was really the equivalent of the other, and that they ought not to be classed under separate names. The officers of the Geological Survey, working from the clearly defined Silurian formations, could draw no line between these and those of North Wales, which Sedgwick had classed as Cambrian. Finding the same fossils in both, they felt themselves constrained to class them all under the same designation of Silurian. Murchison, of course, had no objection to the indefinite extension of his system. Sedgwick, however, after some delay, protested against what he considered to be an unjustifiable appropriation of territory which he had himself conquered. And thus arose a misunderstanding between these two old comrades, which deepened ere long into a permanent estrangement.

It is not my intention to enter here into the details of

[1] *Op. cit.*

this unhappy controversy.[1] My only object in referring to it is to point out how far we are indebted to Sedgwick for the establishment of the Cambrian system. He eventually traced through a part of the Welsh border a marked unconformability between the Upper Silurian formations and everything below them, and he proposed that his Cambrian system should be carried up to that physical break and thus include Murchison's Lower Silurian formations. But as these formations had been defined stratigraphically and palæontologically before he had been able to get his fossils from North Wales examined, they obviously had the right of priority. And the general verdict of geologists went in favour of Murchison.

While this dispute was in progress in Britain, a remarkable series of investigations by Joachim Barrande had made known the extraordinary abundance and variety of Silurian fossils in Bohemia. This distinguished observer not only recognized the equivalents of Murchison's Upper and Lower Silurian series, but found below that series a still older group of strata, characterized by a different assemblage of fossils, which he termed the first or primordial fauna. It was ascertained that representatives of this fauna occurred in Wales among some of the divisions of Sedgwick's Cambrian system, far below the Llandeilo group which formed the original base of the Silurian series. Eventually, therefore, since the death of the two great disputants, there has been a general consensus of opinion that the top of the Cambrian system should be drawn at the upper limit of the primordial fauna.[2]

[1] I have already given a full and, I believe, impartial account of it in my *Life of Murchison*.

[2] It has been proposed by Professor Lapworth that the strata named

By this arrangement, Sedgwick's name is retained for an enormously thick and varied succession of strata which possess the deepest interest, because they contain the earliest records yet discovered of organized existence on the surface of our globe. It was Sedgwick who first arranged the successive groups of strata in North Wales, from the Bala and Arenig rocks down into the depths of the Harlech anticline. His classification, though it has undergone some slight modification, remains to this day essentially as he left it. And thus the name which he selected for his system, and which has become one of the household words in geological literature, remains with us a memorial of one of the most fearless, strenuous, gentle and lovable of all the master minds who have shaped our science into its present form.

By the establishment of the Cambrian and Silurian systems a vast stride was made in the process of reducing the chaos of greywacke into settled order. But there still remained a series of rocks in that chaos which could not be claimed as either Cambrian or Silurian and did not yield fossils which would show them to be Carboniferous. Before any dispute arose between Sedgwick and Murchison as to the respective limits of their domains in Wales, they were led to undertake a conjoint investigation which resulted in the creation of the Devonian system. The story of the addition of this third chapter to early Palæozoic history may be briefly told.

by Murchison Lower Silurian and claimed by Sedgwick as Upper Cambrian, should be taken from both and be given a new name, "Ordovician." But this proposal is fair to neither disputant. By all the laws that regulate scientific priority the strata which were first separated by Murchison and distinguished by their fossils, should retain the name of Lower Silurian which he gave them.

It had long been known that greywacke or Transition rocks covered most of the counties of Devon and Cornwall. Closer examination of that region had shown that a considerable tract of greywacke, now known as Culm-measures, contained abundant carbonaceous material and even yielded fossil plants that appeared to be identical with some of those in the Carboniferous system. It was at first supposed by De la Beche that these plant-bearing rocks lay below the rest of the greywacke of that part of the country. Murchison, however, from the evidence of his clear sections in the Silurian territory, felt convinced that there must be some mistake in regard to the supposed position of these rocks, for he had traversed all the upper greywacke along the Welsh border, and had found it to contain no land-plants at all, but to be full of marine shells. He induced Sedgwick to join him in an expedition into Devonshire. The two associates, in the course of the year 1836, completely succeeded in proving that the Culm-measures, or Carboniferous series, lay not below but above the rest of the greywacke of the south-west of England. But what was that greywacke and what relation did it bear to the rocks which had been reduced to system in Wales?

The structure of the ground in the south-west of England is by no means simple, and, indeed, is not completely understood even now. The rocks have been much folded, cleaved and crushed. But besides these subsequent changes, they present a great contrast in their lithological characters to the Old Red Sandstone on the opposite side of the Bristol Channel. Neither Sedgwick nor Murchison could find any analogy between the Devonshire greywacke and the red sandstones, conglomerates and marls which expand into the Old Red Sandstone of South Wales, and lie so clearly between the Carboniferous Limestone above and the Upper Silurian formations below. Nor could Murchison see a resemblance between that greywacke, or its fossils, and any of his Silurian rocks. With their twisted and indurated aspect, the Devonshire rocks looked so much older than the gently inclined Silurian groups by the banks of the Wye, that both he and Sedgwick thought they more resembled the crumpled and broken rocks of North Wales, and they accordingly first placed them in the upper and middle parts of the Cambrian system.[1]

This correlation, however, was made mainly on lithological grounds. The Devonshire rocks were not without fossils, and considerable collections of these had already been gathered by different residents in the county, but no one had yet endeavoured to make a comparison between them and those of known stratigraphical horizons elsewhere. This task was undertaken at last by William Lonsdale, who towards the end of the year 1837 came to the conclusion that the greywacke and limestone of South Devonshire, judged by their fossil contents, must be intermediate between the Silurian and the Carboniferous formations, that is, on the parallel of the Old Red Sandstone of other parts of Britain.

Such a decision from a skilled palæontologist raised up some serious difficulties, which completely nonplussed the two able geologists who the year before had gone so gaily down to the south-west of England to set matters right there. It seemed to them as if Lonsdale's opinion was opposed to what had been regarded as definitely settled in

[1] *Proc. Geol. Soc.* ii. (1837), p. 560.

the stratigraphy of the older stratified rocks. For two years they continued in complete uncertainty as to the solution of the problem. But at last after the examination of innumerable specimens, endless discussion, and interminable correspondence, they came to adopt Lonsdale's views. They saw that the abundantly fossiliferous rocks of South Devon contained, in their lower members, fossils that reminded them of Silurian types, while in their upper members, they yielded species that were common also to the Carboniferous Limestone. The two geologists therefore recognized in these rocks an intermediate series of strata, containing a fauna which must have flourished between the Silurian and the Carboniferous periods. That fauna was not represented in the Old Red Sandstone, which, with its traces of land-plants and remains of ganoid fishes, appeared to have been accumulated under other geographical conditions. To distinguish the series of rocks containing this well-marked facies of marine organisms, they chose the name "Devonian," from the county where they were originally studied and where their true position was first ascertained.[1] The authors claimed that the establishment of the Devonian system was "undoubtedly the greatest change which has ever been attempted at one time in the classification of British rocks." But it was far more than that. It was the determination of a new geological series of world-wide significance, the unfolding of a new chapter in the geological annals of our globe. Soon after Sedgwick and Murchison had finally announced to the Geological Society their reform of the geology of Devonshire, they

[1] *Trans. Geol. Soc.*, 2nd ser. vol. v. pp. 688, 701 (April 1839).

started for Rhineland, the Harz and Fichtelgebirge, and succeeded in demonstrating that the Devonian system is more extensively and completely developed there than in its original Devonshire home.

I have dwelt on the labours of Sedgwick and Murchison which more especially place their names among those of the founders of geology. But besides these exploits they each accomplished a vast amount of admirable work, and helped thereby to widen the bounds and strengthen the foundations of the science to which they devoted their lives. To enter upon the consideration of these further achievements, however, would lead us far beyond the limits of our time.

Murchison succeeded De la Beche as Director-General of the Geological Survey of Great Britain, and held that office until his death in 1871. To the last, he retained the erect military bearing of his youth, and even under the weight of threescore years and ten could walk a dozen of miles and keep a keen eye on all the topographical and geological features of the surrounding hills. Tall and dignified in manner, with much of the formal courtesy of an older time, he might seem to those who only casually met him to be proud or even haughty. But under this outer crust, which soon dropped away in friendly intercourse, there lay a friendly and helpful nature. Indomitable in his power of work, restless in his eager energy in the pursuit of his favourite science, full of sympathies for realms of knowledge outside of his own domain, wielding wide influence from his social position, he did what no other man of his time could do so well for the advance of science in England. And his death at the ripe age of seventy-nine left a blank in that country which has never since been filled.

Sedgwick was in many respects a contrast to Murchison. His powerful frame reminded one of the race of dalesmen from which he sprang. His eagle eyes seemed as if they could pierce into the very heart of the stiffest geological problem. In his prime, he always made straight for the roughest ground, the steepest slopes, or the highest summits, and his bodily strength bore him bravely through incredible exertion. Unfortunately his health, always uncertain, would react on his spirits, and times of depression and lethargy would come to interrupt and retard his work, whether with hammer or pen. But even his gloomiest fits he could turn into merriment, and he would laugh at them and at himself, as he described his condition to some friend. His gaiety of spirit made him the centre and life of every company of which he formed part. His frank manliness, his kindliness of heart, his transparent childlike simplicity, his unwearied helpfulness and his gentle tenderness, combined to form a character altogether apart. He was admired for his intellectual grasp, his versatility, and his eloquence, and he was beloved, almost worshipped, for the overflowing goodness of his character.

When in the early part of this century, one discovery after another was made which showed that Werner's so-called Primitive rocks reappeared among his Transition and Floetz formations, a doubt began to arise whether there were any primitive rocks at all.[1] We have traced

[1] Thus D'Aubuisson wrote in 1819—"Geology no longer possesses a single rock essentially primitive" (*Traité de Géognosie*, tome ii. p. 197).

how Murchison and Sedgwick cleared up the confusion of the Transition series and created the Devonian, Silurian and Cambrian systems. In Wales certain schists had been detected by Sedgwick below his Cambrian rocks, but they did not greatly interest him, and he never tried to make out their structure and history. Afterwards A. C. Ramsay and his associates claimed these schists as metamorphosed parts of the Cambrian system. To this day their true position has not been settled further than that they are known to be pre-Cambrian.

The vast and varied series of rocks, which have now been ascertained to underlie the oldest Cambrian strata, have undergone much scrutiny during the last quarter of a century, and their true nature and sequence are beginning to be understood. The first memorable onward step in this investigation was taken in North America by William Edmond Logan (1798-1875). Many years before his time, the existence of ancient gneisses and schists had been recognized both in the United States and in Canada. At the very beginning of the century, the wide extent of these rocks had been noted by W. Maclure, the father of American geology, who was the first to produce a general geological sketch-map of a large part of the United States. In 1824 and afterwards, Bigsby spent much time among these rocks to the north of Lake Superior. Subsequently the gneisses of the Adirondack Hills were described by Eaton. At the very beginning of his connection with the Geological Survey of Canada in 1843, Logan confirmed the observation that the oldest fossiliferous formations of North America lie unconformably on a vast series of gneisses and other crystalline rocks, to which he continued at first to apply the old term Primary. By degrees, as he saw more evidence of parallel structures in these masses, he thought that they were probably altered sediments, and he referred to them as Metamorphic. That portion of the series which includes thick bands of limestone he proposed to consider as a separate and overlying group. In the course of years, working with his associates Alexander Murray and Sterry Hunt, he was able to show the enormous extent of these primary rocks, covering as they do several hundred thousand square miles of the North American continent and stretching northwards to the borders of the Arctic Ocean. He proposed for these most ancient mineral masses the general appellation of Laurentian, from their development among the Laurentide mountains. Afterwards he thought it possible to subdivide them into three separate groups, which he designated Upper, Middle and Lower. In the course of his progress, he came upon a series of hard slates and conglomerates, containing pebbles and boulders of the gneiss, and evidently of more recent origin, yet nowhere, so far as he could see, separable by an undoubted unconformability. These rocks, being extensively displayed along the northern shores of Lake Huron, he named Huronian. He afterwards described a second series of copper-bearing rocks lying unconformably on the Huronian rocks of Lake Superior. He thus recognized the existence of at least three vast systems older than the oldest fossiliferous formations. He may be said to have inaugurated the detailed study of Pre-Cambrian rocks. Subsequent investigation has shown the structure of the regions which he explored to be even more complicated and difficult than he believed it to be, and various modifications have been proposed in his work and terminology. But he will ever stand forward as one of the pioneers of geology who in the face of incredible difficulties first opened the way towards a comprehension of the oldest rocks of the crust of the earth.

If a geologist were asked to point out what departments of his science had made the most signal progress during the present century, he would undoubtedly place first the extraordinary development of stratigraphy and its palæontological accompaniments. But were he to continue his selection, he would probably point to glaciation and petrography as the two sections which display the most remarkable advance, the former created within the lifetime of many geologists still living, the latter, though not actually founded, yet vivified with a new life within the memory of most of us, and by a man whom we can count among our living associates.

The original suggestion of Playfair that the erratic blocks of Switzerland had been transported by glaciers, during a former vast extension of the ice of the Alps, had passed out of mind. Venetz and Charpentier were the first to take up anew this interesting department of geology, and to trace the dispersal of the crystalline rocks of the Central Alps outward across the great Swiss plain to the flanks of the Jura mountains.[1] It was reserved, however, for Agassiz to perceive the wide significance of the facts observed, and to start the investigations which culminated in the recognition of an Ice Age that involved the whole

[1] *Schweizer. Gesell. Verhandl.* 1834, p. 23; *Ann. des Mines*, viii. (1835) p. 219; *Leonhard und Bronn, Neues Jahrb.* 1837, p. 472.

of the northern part of our hemisphere, and in the voluminous literature which has recorded the rapid progress of this department of geology.

Jean Louis Rodolphe Agassiz (1807-1873) was born in Switzerland, and rose to distinction by his scientific work in Europe, but he went to the United States when he was still only forty-two years of age, and spent the last twenty-seven years of his life as an energetic and successful leader of science in his adopted home. His fame is thus both European and American, and the geologists of New England, not less than those of Switzerland, may claim him as one of their most distinguished worthies.

We must pass over the brilliant researches into the history of fossil fishes which placed the name of Agassiz high among the palæontologists of Europe when he was still a young man. What we are more particularly concerned with here is the share he had in founding the modern school of glacial geology. As far back as the summer of 1837, when he was only thirty-three years of age, Agassiz, as President of the Helvetian Society of Natural Science, struck, with the hand of a master, the keynote of all his future research in glaciation. Tracing the distribution of the erratic blocks above the present level of the glaciers, and far beyond their existing limits, he connected these transported masses with the polished and striated rock-surfaces which were known to extend even to the summits of the southern slopes of the Jura. He showed, from the nature of these smoothed surfaces, that they could not have been worn into their characteristic forms by any current of water. The fine striæ, engraven on them as with a diamond-point, he proved to be precisely similar to those now being scratched on the rocky floors of the modern glaciers, and he inferred that the polished and striated rocks of the Jura, even though now many leagues from the nearest glacier, must have acquired their peculiar surface from the action of ice moving over them, as modern glaciers slide upon their beds. He was thus led to conclude that the Alpine ice, now restricted to the higher valleys, once extended into the central plain, crossed it, and even mounted to the southern summits of the Jura chain.

Before Agassiz took up the question, there were two prevalent opinions regarding the transport of the erratics. One of these called in the action of powerful floods of water, the other invoked the assistance of floating ice. Agassiz combated these views with great skill. His reasoning ought to have convinced his contemporaries that his explanation was the true one. But the conclusions to which he was led seemed to most men of the day extravagant and incredible. Even a cautious thinker like Lyell saw less difficulty in sinking the whole of Central Europe under the sea, and covering the waters with floating icebergs, than in conceiving that the Swiss glaciers were once large enough to reach to the Jura. Men shut their eyes to the meaning of the unquestionable fact that, while there was absolutely no evidence for a marine submergence, the former track of the glaciers could be followed mile after mile, by the rocks they had scored and the blocks they had dropped, all the way from their present ends to the far-distant crests of the Jura.

Agassiz felt that the question was connected with large problems in geology. The former vast extension of the Swiss glaciers could be no mere accidental or local phenomenon, but must have resulted from some general lowering of temperature. He coupled with this deduction certain theoretical statements regarding former climates and faunas, which have not been supported by subsequent research.

The main conclusions which the Swiss naturalist drew, so greatly interested him that he spent part of five successive summers investigating the vestiges of the old glaciers, and the operations of those of the present time. He convinced himself that the great extension of the ice was connected with the last great geological changes on the surface of the globe, and with the extinction of the large pachyderms, whose remains are so abundant in Siberia. He believed that the glaciers did not advance from the Alps into the plains, but rather that ice once covered all the lower grounds, and finally retreated into the mountains.

Having arrived at these conclusions from studies in his native country, Agassiz was naturally desirous to see how far his views could be tested or confirmed in a region far removed from any existing glaciers. Accordingly, in the year 1840, three years after his address at Neufchâtel, he had an opportunity of visiting Britain, and took advantage of it to examine a considerable part of Scotland, the north of England, and the north, centre, west, and south-west of Ireland. The results of this investigation were of remarkable influence in the progress of glacial geology. Agassiz demonstrated the identity of the phenomena in Britain with those in Switzerland, and claimed "that not only glaciers once existed in the British Islands, but that large sheets (*nappes*) of ice covered all the surface."[1]

[1] *Proc. Geol. Soc.* vol. iii. (1840), p. 331.

These researches started the study of ancient glaciation. Buckland, Lyell, Darwin, Chambers and others took up the question, and added to the evidence adduced by Agassiz from his rapid traverses. At first the existence of former glaciers in the valleys of Britain was the conclusion chiefly sought to be established. British geologists, and indeed geologists generally, were for many years unwilling to admit that not only the mountain-valleys, but even the lowlands of the northern hemisphere were at a late geological period buried under sheets of ice. They preferred to call in the action of floating ice, without perceiving that in so doing they involved themselves in far more serious physical difficulties than those which they sought to avoid. But for many years past the teaching of Agassiz in all its essential elements has been generally accepted, and his name is now enshrined in the records of our science as that of the true founder of glacial geology.

I turn now to the petrographical department of geological inquiry, as exhibiting the last great forward stride which our science has taken. We have seen how greatly geology and mineralogy were indebted to Werner for his careful and precise definitions. The impulse which he gave to the study of Petrography continued to show its effects long after his time, more particularly in Germany. Methods of examination were improved, chemical analysis was more resorted to, and the rocks of the earth's crust, so far as related to their ultimate chemical constitution, were fairly well known and classified. Their internal structure, however, was very imperfectly understood. Where they were coarsely crystalline, their component minerals might be readily determined; but where they became fine-grained, nothing more could be said about the nature and association of their constituents than might be painfully deciphered with the help of a hand-lens. Hence though not actually at a standstill, petrography continued to make but slow progress. In some countries indeed, notably in Britain, it was almost entirely neglected in favour of the superior attractions of fossils and stratigraphy. But at last there came a time of awakening and rapid advance.

In order to trace the history of this petrographical resuscitation, I must ask you to accompany me to the workshop of an ingenious and inventive mechanician, William Nicol, who was a lecturer on Natural Philosophy at Edinburgh in the early part of this century. Among his inventions was the famous prism of Iceland spar that bears his name.[1] Every petrographer will acknowledge how indispensable this little piece of apparatus is in his microscopic investigations. He may not be aware, however, that it was the same skilful hands that devised the process of making thin slices of minerals and rocks whereby the microscopic examination of these substances has become possible.

In the course of his experiments, Nicol hit upon the plan of cutting sections of fossil wood, so as to reveal their minutest vegetable structures. He took a slice from the specimen to be studied, ground it perfectly flat, polished it, and then cemented it by means of Canada balsam to a piece of plate-glass. The exposed surface of the slice was

[1] See Nicol's original account of his prism in *Edin. New Phil. Journ.* vol. vi. (1829), p. 83.

then ground down, until the piece of stone was reduced to a thin pellicle adhering to the glass, and the requisite degree of transparency was obtained. Nicol himself prepared a large number of slices of fossil and recent woods. Many of these were described by Henry Witham in his *Observations on Fossil Vegetables* (1831), to which Nicol supplied the first published account of his process.

Here then geologists were provided with a method of investigating the minutest structures of rocks and minerals. It was now possible to subject any part of the earth's crust to investigation with the microscope. It might have been thought that those who devoted themselves to the study of that crust, especially those who were more particularly interested in the structure, composition and history of rocks, would have hastened to avail themselves of the new facilities for research thus offered to them.

It must be confessed, I am afraid, that geologists are about as difficult to move as their own erratic blocks. They took no notice of the possibilities put in their way by William Nicol. And so for a quarter of a century the matter went to sleep. When Nicol died, his instruments and preparations passed into the hands of the late Mr. Alexander Bryson of Edinburgh who, having considerable dexterity as a manipulator, and being much interested in the process, made many additions to the collections which he had acquired. In particular, he made numerous thin slices of minerals and rocks for the purpose of exhibiting the cavities containing fluid, which had been described long before by Brewster[1] and by Nicol.[2] In my boy-

[1] *Trans. Roy. Soc. Edin.* vol. x. (1824), p. 1.
[2] *Edin. New. Phil. Jour.* vol. v. (1828), p. 94.

hood I had frequent opportunities of seeing these and the other specimens in Mr. Bryson's cabinet, as well as the fine series of fossil woods sliced so long before by Nicol.

At last Mr. Henry Sorby came to Edinburgh, and had an opportunity of looking over the Bryson collection. He was particularly struck with the series of slices illustrating "fluid-cavities," and at once saw that the subject was one of which the further prosecution could not fail to "lead to important conclusions in geological theory."[1] He soon began to put the method of thin slices into practice, made sections of mica-schist,[2] and found so much that was new and important, with a promise of such a rich harvest of results, that he threw his whole energy into the investigation for several years, and produced at last in 1858 the well-known memoir, *On the Microscopical Structure of Crystals*,[3] which marks one of the most prominent epochs of modern geology. I have always felt a peculiar satisfaction in the reflection that though the work of William Nicol was never adequately recognized in his lifetime, nor for many years afterwards, it was his thin slices, prepared by his own hands, that eventually started Mr. Sorby on his successful and distinguished career, and thus opened out a new and vast field for petrographical investigation.

It is not necessary here to recapitulate the achievements which have placed Mr. Sorby's name at the head of modern petrographers. He, for the first time, showed how,

[1] *Quart. Jour. Geol. Soc.* vol. xiv. (1858), p. 454.
[2] *Brit. Assoc. Reports,* 1856, sections, p. 78.
[3] *Quart. Journ. Geol. Soc.* vol. xiv. (1858), p. 453.

by means of the microscope, it was possible to discover the minute structure and composition of rocks, and to learn much regarding their mode of origin. He took us, as it were, into the depths of a volcanic focus, and revealed the manner in which lavas acquire their characters. He carried us still deeper into the terrestrial crust, and laid open the secrets of those profound abysses in which granitic rocks have been prepared. His methods were so simple, and his deductions so startling, that they did not instantly carry conviction to the minds of geologists, more particularly to those of his own countrymen. The reproach that it was impossible to look at a mountain through a microscope was brought forward in opposition to the new departure which he advocated. Well did he reply by anticipation to this objection. "Some geologists, only accustomed to examine large masses in the field, may perhaps be disposed to question the value of the facts I have described, and to think the objects so minute as to be quite beneath their notice, and that all attempts at accurate calculations from such small data are quite inadmissible. What other science, however, has prospered by adopting such a creed? What physiologist would think of ignoring all the invaluable discoveries that have been made in his science with the microscope merely because the objects are minute? . . . With such striking examples before us, shall we physical geologists maintain that only rough and imperfect methods of research are applicable to our own science? Against such an opinion I certainly must protest; and I argue that there is no necessary connection between the size of an object and the value of a fact, and that, though the objects I have described are minute, the conclusions to be derived from the facts are great."[1]

Professor Zirkel was the first geologist of note who took up with zeal the method of investigation so auspiciously inaugurated by Mr. Sorby. But some five years had elapsed before he made his communication on the subject to the Academy of Sciences of Vienna.[2] From that date (1863) he devoted himself with much zeal and success to the investigation, and produced a series of papers and volumes which gave a powerful impetus to the study of petrography. This department of geology was indeed entirely reconstituted. The most exact methods of optical research were introduced into it by Professor Rosenbusch, Professor Fouqué, M. Michel Lévy and others, and the study of rocks once more competed with that of fossils in attractiveness. We have only to look at the voluminous literature which has sprung up in the last thirty years devoted to the investigation of rocks, to see how great a revolution has been effected by the introduction of the microscope into the equipment of the geologist. For this transformation we are, in the first instance, indebted to William Nicol and Henry Clifton Sorby.

In bringing to a close my outline of the work of those who deserve to be remembered as the founders of geology, I

[1] *Quart. Journ. Geol. Soc.* xiv. (1858), p. 497. See also Mr. Sorby's Presidential Addresses to the Geological Society for 1879 and 1880.

[2] *Sitzungsber Math. Naturwiss.* vol. xlvii. 1st part (1863), p. 226. In this paper the author refers to previous occasional use of the microscope for determining the mineralogical composition of rocks by Gustav Rose, G. vom Rath, G. Jenzsch, M. Deiters and others. In England the first geologist who published the results of his microscopical examination of rocks was David Forbes, *Popular Science Review* (October 1867), vol. vi. p. 355.

am conscious that many names which I have omitted might fitly have found a place in my list. But there are still two which I must not pass over, and with these I shall conclude. They are Charles Lyell and Charles Darwin—two illustrious men who were linked together in their lives by many ties of sympathy, and whom it is a gratification to place side by side on the bede-roll of geological fame.

Charles Lyell (1797-1875) exercised a profound influence on the geology of his time in all English-speaking countries. Adopting the principles of the Huttonian theory, he developed them until the original enunciator of them was nearly lost sight of. Lyell, with unwearied industry, marshalled in admirable order all the observations that he could collect in support of the doctrine that the present is the key to the past. With inimitable lucidity, he traced the operation of existing causes, and held them up as the measure of those which have acted in bygone time. He carried Hutton's doctrine to its logical conclusion, for not only did he refuse to allow the introduction of any process which could not be shown to be a part of the present system of nature, he would not even admit that there was any reason to suppose the degree of activity of the geological agents to have ever seriously differed from what it has been within human experience. He became the great high priest of Uniformitarianism — a creed which grew to be almost universal in England during his life, but which never made much way in the rest of Europe, and which in its extreme form is probably now held by few geologists in any country. Lyell's *Principles of Geology* will, however, always rank as one of the classics of geology, and must form an early part of the reading of every man who would wish to make himself an accomplished geologist.

Lyell's work was mainly that of a critic and exponent of the researches of his contemporaries, and of a philosophical writer thereon, with a rare faculty of perceiving the connection of scattered facts with each other, and with the general principles of science. As Ramsay once remarked to me, "We collect the data, and Lyell teaches us to comprehend the meaning of them." But Lyell, though he did not, like Sedgwick and Murchison, add new chapters to geological history, nevertheless left his mark upon the nomenclature and classification of the geological record. Conceiving, as far back as 1828, the idea of arranging the whole series of Tertiary formations in four groups, according to their affinity to the living fauna, he established, in conjunction with Deshayes, who had independently formed a similar opinion, the well-known classification into Eocene, Miocene, and Pliocene. The first of these terms, as we all know, was proposed for strata containing an extremely small proportion of living species of shells; the second for those where the percentage of recent species was considerable, but still formed the minority of the whole assemblage, while the third embraced the formations in which living forms were predominant. The scheme was a somewhat artificial one, and the original percentages have had to be modified from time to time, but the terms have kept their place, and are now firmly planted in the geological language of all corners of the globe.

Charles Darwin (1809-1882) contributed many valuable works to the literature of geology. But it is not for these that I cite his name on the present occasion. His two geological chapters in the *Origin of Species* produced the greatest revolution in geological thought which has occurred in my time. Younger students, who are familiar with the ideas there promulgated, can hardly realize the effect of them on an older generation. They seem now so obvious and so well-established, that it may be difficult to conceive a philosophical science without them.

To most of the geologists of his day, Darwin's contention for the imperfection of the geological record, and his demonstration of it, came as a kind of surprise and awakening. They had never realized that the record was so fragmentary. And yet when Darwin pointed this out to them they were compelled, sometimes rather reluctantly, to admit that he was right. Some of them at once adopted the idea, as Ramsay did, and carried it much further.[1]

Until Darwin took up the question, the necessity for vast periods of time, in order to explain the characters of the geological record, was very inadequately realized. Of course, in a general sense, the great antiquity of the crust of the earth was everywhere admitted. But no one before his day had perceived how enormous must have been the periods required for the deposition of even some thin continuous groups of strata. He supplied a criterion by which, to some degree, the relative duration of formations might perhaps be apportioned. When he declared that the intervals that elapsed between consecutive formations may sometimes have been of far longer duration than the formations themselves, contemporary geologists could only smile incredulously in their bewilderment, but in a few

[1] See his two Presidential Addresses to the Geological Society, *Quart. Journ. Geol. Soc.* vols. xix. (1863), xx. (1864).

years Ramsay showed by a detailed examination of the distribution of fossils in the sedimentary strata that Darwin's suggestion must be accepted as an axiom in geological theory. Again, the great naturalist surmised that, before the deposition of the oldest known fossiliferous strata, there may have been antecedent periods, collectively far longer than from the date of these strata up to the present day, and that, during these vast, yet quite unknown, periods, the world may have swarmed with living creatures. But his contemporaries could only shrug their shoulders anew, and wonder at the extravagant notions of a biologist. But who nowadays is unwilling to grant the possibility, nay probability, of Darwin's surmise? Who can look upon the earliest Cambrian fauna without the strongest conviction that life must have existed on this earth for countless ages before that comparatively well-developed fauna came into existence? For this expansion of our geological vision, and for the flood of light which has been thrown upon geological history by the theory of evolution, we stand indebted to Charles Darwin.

In the account which I have now placed before you of the work of some of the more notable men who have created the science of geology, one or two leading facts stand out prominently before us. In the first place, even in the restricted list of names which we have considered, it is remarkable how varied have been the employments of these men, and how comparatively few of them could be called professional geologists. The majority of them have been men engaged in other pursuits, who have devoted their leisure to the cultivation of science. Guettard, Pallas, and Fuchsel were physicians, led by their medical training to interest themselves in natural history. Giraud-Soulavie and Michell were clergymen. Murchison was a retired soldier. Alexander Brongniart was at first engaged in superintending the porcelain manufactory of Sèvres. Desmarest was a hard-worked civil servant who snatched his intervals for geology from the toils of incessant official occupation. William Smith found time for his researches in the midst of all the cares and anxieties of his profession as an engineer and surveyor. Hutton, Hall, De Saussure, Von Buch, Lyell and Darwin were men of means, who scorned a life of slothful ease and dedicated themselves and their fortune to the study of the history of the earth. Playfair and Cuvier were both teachers of other branches of science, irresistibly drawn into the sphere of geological inquiry and speculation. Of the whole gallery of worthies that have passed before us there are only three that can strictly be considered as professional goologists—Werner, Sedgwick and Logan. Were we to step outside of that gallery, and select as many names of hardly inferior lustre, we should find the proportions not to be seriously different.

From the beginning of its career, geology has owed its foundation and its advance to no select and privileged class of experts. It has been open to all who cared to undergo the trial which its successful prosecution demands. And what it has been in the past, it remains to-day. No branch of natural knowledge lies more invitingly open to every student who, loving the fresh face of Nature, is willing to train his faculty of observation in the field, and to discipline his mind by the patient correlation of facts and the fearless dissection of theories. To such an inquirer no limit can be set. He may be enabled to rebuild parts of the temple of science, or to add new towers and pinnacles to its superstructure. But even if he should never venture into such ambitious undertakings, he will gain, in the cultivation of geological pursuits, a solace and enjoyment amid the cares of life, which will become to him a source of the purest joy.

In the second place, the history of our science presents some conspicuous examples of the length of time that may elapse before a fecund idea comes to germinate and bear fruit. Consider for a moment how many years passed before the stratigraphical conceptions of Fuchsel and Giraud-Soulavie took more definite shape in the detailed investigations of Cuvier, Brongniart and Smith, and how many more years were needed before the Secondary and Tertiary formations were definitely arranged and subdivided. Remember too that even after the principles of stratigraphy had been settled, a quarter of a century had slipped away before they were successfully applied to the Transition rocks. Mark the history of physiographical geology, and note that though the principles of this branch of science were firmly grasped by Desmarest and Hutton in last century, their work was neglected and forgotten until the whole subject has been revived and marvellously extended in our own day. Again let me recall how slowly the key that unlocks the innermost mysteries of rock-structure was made use of. Five-and-twenty years elapsed after William Nicol had shown how stony substances could be investigated by means of the microscope, before Mr. Sorby called the attention of geologists to the enormous value of the method thus put into their hands.

Other five years had to pass before the method began to be taken up in Germany, and a still longer time before it came into general use all over the world.

Such instances as these lead to two reflections. On the one hand, they assure us of the permanent vitality of truth. The seed may be long in showing signs of life, but these signs come at last. On the other hand, we are warned to be on the outlook for unrecognized meanings and applications in the work of our own day and in that of older date. We are taught the necessity not only of keeping ourselves abreast of the progress of science at the present time, but also of making ourselves acquainted as far as we possibly can with the labours of our predecessors. It is not enough to toil in our little corner of the field. We must keep ourselves in touch with what is going on now, and what has been done during the past in that and surrounding parts of the domain of science. Many a time we may find that the results obtained by some fellow-labourer, though they may have had but little significance for him, flash a flood of light on what we have been doing ourselves.

I am only too painfully aware how increasingly difficult it is to keep pace with the ever-rising tide of geological literature. The science itself has so widened, and the avenues to publication have so prodigiously multiplied, that one is almost driven in despair to become a specialist, and confine one's reading to that portion of the literature which deals with one's own more particular branch of the science. But this narrowing of our range has a markedly prejudicial effect on the character of our work. The only consolation we can find is the conviction, borne in upon us by ample and painful experience, that a very large mass of the geological writing of the present time is utterly worthless for any of the higher purposes of the science, and that it may quite safely and profitably, both as regards time and temper, be left unread. If geologists, and especially young geologists, could only be brought to realize that the addition of another paper to the swollen flood of our scientific literature involves a serious responsibility; that no man should publish what is not of real consequence, and that his statements when published should be as clear and condensed as he can make them, what a blessed change would come over the faces of their readers, and how greatly would they conduce to the real advance of the science which they wish to serve.

In the third and last place, it seems to me that one important lesson to be learnt from a review of the early history of geology is the absolute necessity of avoiding dogmatism. Let us remember how often geological theory has altered. The Catastrophists had it all their own way until the Uniformitarians got the upper hand, only to be in turn displaced by the Evolutionists. The Wernerians were as certain of the origin and sequence of rocks as if they had been present at the formation of the earth's crust. Yet in a few years their notions and overweening confidence became a laughing-stock. From the very nature of its subject, as I have already remarked, geology does not generally admit of the mathematical demonstration of its conclusions. They rest upon a balance of probabilities. But this balance is liable to alteration, as facts accumulate or are better understood. Hence what seems to be a well-established deduction in one age may be seen to be entirely erroneous in the next. Every year, however, the data on which these inferences are based are more thoroughly comprehended and more rigidly tested. Geology now possesses a large and ever-growing body of well-attested fact which will be destroyed by no discovery of the future, though it will doubtless be vastly augmented, while new light may be cast on many parts of it now supposed to be thoroughly known.

Each of us has it in his power to add to this accumulation of knowledge. Careful and accurate observation is always welcome, and may eventually prove of signal importance. We must be on our guard, however, against premature speculation and theory. Let us do our utmost to eliminate hypothesis from our gathering of facts, or at least clearly to distinguish between what is fact and what may be our own gloss or interpretation of it. And, above all, let us preserve the modesty of the true student, face to face with the mysteries of nature. Proving all things and holding fast that which we believe to be true, let us look back with gratitude and pride to what has been achieved by our forerunners in the race, and while we labour to emulate their devotion, let us hold high the torch of science, and pass it on bright and burning to those who shall receive it from our hands.

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THE END

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