SCIENCE

A WEEKLY JOURNAL DEVOTED TO THE ADVANCEMENT OF SCIENCE, PUBLISHING THE OFFICIAL NOTICES AND PROCEEDINGS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

FRIDAY, FEBRUARY 1, 1907.

CONTENTS

MSS. intended for publication and books, etc., intended for review should be sent to the Editor of SCIENCE, Garrison-on-Hudson, N. Y.

THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

THE CONTRIBUTIONS OF AMERICA TO GEOLOGY[1]

IN speaking of the contributions of America to geology, I do not propose to give an inventory of the geological facts which have been made known as the result of work in this country. An area of three million square miles has been covered by geological reconnoissance, and much of that area has been surveyed in detail. It is, moreover, an area interesting in many respects. It is a country of vast mineral wealth, leading the world in the production of coal, petroleum, salt, iron, copper, silver and lead, and ranking with Australia and South Africa as one of the three great gold-producing regions of the world. It is a country presenting a remarkable variety of topography and geological structure; and some of its scenic features can be adequately described only in superlatives. It includes vast prairies of great fertility; the Missouri-Mississippi river system, with thousands of miles of navigable waters; the broad and complex mountain mass of the Cordillera; the Great Lakes of the Canadian border, a chain of fresh-water seas; the sublime cataract of Niagara; the remarkable region of interior drainage of the Great Basin; the geysers of the Yellowstone; the cañon system of the Colorado and its tributaries, unparal-

[1] Address of the vice-president and chairman of Section E—Geology and Geography, American Association for the Advancement of Science, New York meeting, December, 1906.

leled in depth and extent; the lava flood of the northwest, surpassed in area only by that of the Deccan. The exploration of a region so vast and so varied must, of course, have made an important addition to the facts upon which geological science is founded.

Of these facts, however, I do not propose to give even an outline. I propose rather to ask the question, 'What has our country contributed to the stock of geological ideas?' In that classical history of geological science which Lyell has given us in his 'Principles of Geology,' he calls attention to the fact that the share which different nations bore in the early development of geological science was dependent not alone upon the genius of individual workers, but in large measure upon the peculiar geological conditions of the various countries in which they worked. It was in the presence of the varied mineral wealth of the Erzgebirge that Werner laid the foundations of mineralogy and lithology; the magnificent display alike of igneous and of aqueous agencies in the Highlands of Scotland helped to guide Hutton to those theoretical views which were the beginning of modern dynamical geology; the remarkable completeness with which, for an area so small, the series of stratified formations is developed in England, gave William Smith the opportunity to lay the foundations of stratigraphical geology; and the abundant vertebrate fossils in the Paris basin enabled Cuvier to create the science of paleontology. That history of the labors which founded our science in the close of the eighteenth and the beginning of the nineteenth century, suggests the question whether there have been developed any characteristically American ideas in geology.

Of course, it must be admitted that there is to-day no department of geological science which is as characteristically American as mineralogy was German, and dynamical geology was Scotch, and stratigraphical geology was English, and paleontology was French, a century ago. The conditions of life in colonial days and in the early decades of our national history did not afford the opportunity for fruitful scientific investigation. We had no universities, no libraries, no museums, that could be compared with those of the Old World. The conquest of the land from forest and wild beast and savage man, the achievement of national independence, the beginning of the development of national wealth, necessarily preceded the beginning of scientific investigation. When the time came for scientific work in this country, geology had already become a recognized science. That early stage of incompleteness in which one department was developed here and another there, had passed. It was too late for any country to create a new department in geology. I believe, nevertheless, that there have been certain contributions to the stock of geological ideas which are characteristically American; and that it is not fanciful, in the spirit of that passage of Lyell which I have cited, to connect these characteristically American ideas with our geological environment.

I. THE PERMANENCE OF CONTINENTS

Among the early state geological surveys there were two commenced in 1836 which were destined to have in different ways a surpassing influence upon the development of geological thought. Those were the surveys of New York and Pennsylvania. In passing southwestward across New York, from the Adirondacks to the Pennsylvania border, one would traverse substantially the whole series of Paleozoic formations from the Cambrian to the Carboniferous, most of these formations extending across the state in approximately parallel east and west bands. That remarkably complete development of the Paleozoic series made the stratigraphy of New York a standard of comparison for the subsequent study of the Paleozoic strata throughout the eastern United States, as the stratigraphic series of England had already become, in its broader outlines, a standard of comparison for the world. But that regular succession of Paleozoic strata from the Adirondacks to the Pennsylvania border was suggestive of a dynamic idea. It suggested a gradual emergence of the land from the waters of the sea. In later years, as geological investigation extended westward, a somewhat similar succession could be traced in the Mississippi basin, southward from the Archæan mass north of the Great Lakes. The doctrine of the permanence of continent and ocean—the gradual emergence of continental lands and the withdrawal of the waters into the deepening ocean basins—was first enunciated by Dana in 1846. He had just returned from his voyage around the world in Wilkes's exploring expedition. In that voyage he had studied the phenomena of barrier reefs and atolls, had adopted Darwin's theory of their origin by subsidence, and had defended and illustrated the theory by a far greater wealth of observation than Darwin's route had afforded him the opportunity to make. It was, apparently, the thought of the subsiding ocean bottom, rather than the thought of the emerging land, by which Dana was first led to the doctrine of the permanence of continent and ocean; but, in his presidential address before the American Association for the Advancement of Science, in 1855, Dana refers to the stratigraphy of New York as illustrating the idea of continental emergence. The generation of students who have learned geology from Dana's 'Manual' and 'Text-book,' will remember how prominently he brings forward the succession of Paleozoic formations in New York, as illustrating his conception of the gradual emergence of the land from a continental sea.

The doctrine of the permanence of continents when announced by Dana was essentially a new one. Geologists and pseudo-geologists of all classes had felt at liberty to redistribute continents and oceans according to their own sweet will. After the biblical pseudo-geologists had become convinced of the impossibility of the deposition of the whole series of fossiliferous strata in the Noachian deluge, their next shift was the supposition that the fossiliferous strata had been deposited in the ocean in the interval between the creation and the deluge, and that at the time of the latter event continent and ocean were reversed. Hutton believed that the débris of the continents was carried far out to sea by means of ocean currents, and was deposited over substantially the whole floor of the ocean; and, when one continent was worn away, another might be uplifted in some other part of the world. Lyell eliminated the catastrophic element of Hutton's theorizing; but, like his predecessor, Lyell believed in an indefinite amount of change in the distribution of continent and ocean. In attempting to find a geological explanation for changes of climate, he felt at liberty to speculate on a series of changes in the distribution of continent and ocean which would sometimes bunch the continents around the poles, and at other times girdle the earth with an equatorial belt of land. The readers of Darwin's 'Letters' will remember his half comic, half pathetic protest, in a letter to Lyell, that the disciples of the great geologist 'in a slow and creeping manner beat all the old catastrophists who ever lived.'

There is now little doubt that Dana was right in his general conception. The greater density of the suboceanic masses in comparison with the subcontinental masses, as shown by pendulum observations, indicates that the distinction between continent and ocean has its basis in the heterogeneity of the material in the interior of the earth; and the determining conditions must, therefore, have had their origin in the initial aggregation of that part of the primitive nebula which formed the earth; or, perhaps, as suggested by Chamberlin and Salisbury, in changes attendant upon the beginning of the formation of the ocean. The study of the sedimentary rocks which cover our existing continents shows that almost all of them were deposited in shallow waters; many of the strata, indeed, in waters so shallow that the layers of mud and sand were from time to time exposed by the receding tide or the subsiding freshet, to dry and crack in the sun or to be pitted by raindrops. None of the sedimentary deposits seem to have been formed in waters of truly oceanic depth.

Certain it is, however, that Dana made the evolution of the continents too simple an affair. He recognized, indeed, that the progressive emergence of the continental lands was attended by continual oscillation; yet, even in the last edition of his 'Manual,' it appears that he did not duly appreciate the magnitude of those oscillations. We now know that in early Cambrian time the Mississippian sea was only a sound or strait, most of the area in which the Trenton limestone was subsequently deposited being then dry land. Only gradually did the Appalachian strait widen out into the Mississippian sea. A true conception of continental evolution must recognize two complementary truths: a wide range of oscillatory movement, and yet on the whole a progressive deepening of ocean basins and a progressive emergence of continental lands. Chamberlin has formulated the doctrine of an alternation of marine and continental periods due to an intermittently progressive deepening of the ocean basins, and has connected therewith an ingenious and beautiful theory of climatic changes in geological time.

The doctrine of the progressive evolution of continents, as taught by Dana, gave new clearness and emphasis to the general conception of geology as a history of the globe. Le Conte, in his cordial and generous eulogy of Dana, declared that 'geology became one of the great departments of abstract science, with its own characteristic idea and its own distinctive method, under Dana.' There is certainly somewhat of exaggeration in this commendation, yet the statement contains an important truth. More or less clearly, all geological investigators must have felt that the distinctive idea of geology is that the structures of the rocks of the earth's crust have their supreme significance as monumental inscriptions, the deciphering of which may reveal to us the history of the earth. Yet this conception was never before so clearly formulated, and the whole treatment of the subject so consistently adjusted thereto, as in the writings of Dana. The portion of previous manuals dealing with the local distribution of the series of strata had generally borne some such title as 'Stratigraphical Geology'; and very commonly, as in the well-known works of Lyell and De la Beche, the series had been traced backward, beginning with the most recent strata. In Edward Hitchcock's 'Elementary Geology,' with which, in my boyhood, I commenced the study of the science, the stratigraphic chapter bears the title, 'Lithological Characters of the Stratified Rocks.' It occupies only twenty pages in a book of more than four hundred pages. It traces the formations backward in Lyellian fashion. Separate from the stratigraphic chapter, is another and longer chapter on paleontology, which is arranged botanically and zoologically, and not chronologically. The phrase, 'Historical Geology,' which forms the title of the largest section of Dana's 'Manual,' involves a distinct clarification of the general view of the science. Starting with this conception, he, of course, dealt with the earliest formations first. In the treatment of each era he endeavored to reconstruct, from the evidence afforded by the kinds and distribution of the rocks, the physical geography of the time. The subdivisions of that chapter of the 'Manual' are characterized, not as series, systems and groups of strata, but as eras, periods and epochs of time. The common use, in recent geological writings, of such phrases as 'Silurian era,' rather than 'Silurian system,' etc., is a testimony to the influence of Dana's mode of treatment.

II. THE THEORY OF MOUNTAIN-MAKING

The Geological Survey of Pennsylvania, to which I have already referred, made known the folded structure—the alternate anticlines and synclines—of the Appalachians. The beautiful sections of these folded strata, in the Atlas of that survey, reveal the thoroughness with which the structure of the mountains was investigated by Henry D. Rogers; and can be studied with delight to-day, in spite of the fantastic nomenclature, which the student has to translate into the familiar language of the New York Survey. In those sections it appears that, in general, each fold is unsymmetrical, the dip on the northwest side being steeper than that on the southeast, if, indeed, the dip on the northwest side is not carried beyond the perpendicular and reversed. While in each fold, as a rule, the dip is steeper on the northwest side than on the southeast, if we compare the successive folds, we find the dips growing more gentle as we go from southeast to northwest. The nearly vertical or overturned dips of the folds on the eastern border pass by a gradual transition into gentle undulations on the western border of the Appalachian zone.

While the stratigraphy was worked out so beautifully in the first Geological Survey of Pennsylvania, the dynamic conception derived from it was crude indeed. The conclusions of the author are summed up in the following sentences: "The wave-like structure of undulated belts of the earth's crust is attributed to an actual pulsation in the fluid matter beneath the crust, propagated in the manner of great waves of translation from enormous ruptures occasioned by the tension of elastic matter. The forms of the waves, the close plication of the strata, and the permanent bracing of the flexures, are ascribed to the combination of an undulating and a tangential movement, accompanied by an injection of igneous veins and dykes into the rents occasioned by the bendings. This oscillation of the crust, producing an actual floating forward of the rocky part, has been, it is conceived, of the nature of that pulsation which attends all great earthquakes at the present day." The wave-like form of the Appalachian anticlines and synclines is a beautiful generalization of accurate and conscientious observation; but the dynamic theory suggested for its explanation needs to-day no other refutation than its simple statement.

But, however completely the Pennsylvania geologists failed to construct a satisfactory theory of mountain-making, their observations of Appalachian structure were of immense value in their destructive effect upon some of the notions of mountain-making prevalent at the time. In the text-books in the early part of the nineteenth century, a diagram often appeared representing a transverse section of an ideal mountain range. A vertical wall of crystalline rock forms the center and the crest of the range. Against this wall, on each side, the oldest strata lean in a nearly vertical position; farther from the center, strata of less antiquity lean with more gentle dips against the steeper and older strata; and finally, on the flanks of the mountain range, still newer strata rest nearly horizontally. The diagram represents a dislocation of the strata by repeated vertical upthrusts of the crystalline core. Of course, it was seen at once that there was nothing common to these ideal sections and the real sections of the Appalachians revealed by the Pennsylvania Survey. The billowy form of the Appalachian folds meant something, and something very different from the prevalent conception of mountain ranges.

The true interpretation of the Appalachian waves is probably to be found in the contractional theory of mountain elevation, of which Dana was the leading expounder. The views of Le Conte on the subject of mountain-making were in most respects similar to those of Dana. But, while Le Conte's discussions were of great value, the priority in the general development of the theory belongs to Dana. "To the North American geologists," says von Zittel, "undoubtedly belongs the credit of founding the theory of horizontally acting forces and rock-folding upon an ample basis of observation."

That the main cause of mountain elevation is tangential pressure in the crust resulting from internal contraction, is now generally acknowledged, though there may be doubt whether the main cause of contraction is the cooling of the earth from an incandescent condition, as assumed in the commonly accepted form of the nebular theory, or the gravitational adjustment of an incoherent mass of meteors, as assumed in the more recent planetesimal hypothesis of Chamberlin and Moulton.

The idea of the contractional origin of mountains was not, indeed, original with Dana. There was a glimmer of the idea in the writings of Leibnitz, and Constant Prévost developed the idea into a definite scientific theory; but the elaboration of the theory into its present form we owe chiefly to Dana. His discussion of the subject began in the *Journal of Science*, in 1847. In later years he returned to the subject again and again; and the theory, as shaped by his maturest thought, appears in the last edition of the 'Manual.' In his earlier writings his views of the origin of continents and mountains were developed on the assumption of a liquid globe. In later years he abandoned that view, and adjusted his theories to the more probable doctrine of a globe substantially solid.

The conception of the subsidence of the suboceanic crust, which led Dana to his views of the permanence of continent and ocean, is an important element in his theory of mountain-making. In the contraction of the earth's interior, the suboceanic crust necessarily flattens in its subsidence, so that its section continually approaches the chord of the arc, thus exerting a tangential thrust toward the continental areas. The rather abrupt change in the radius of curvature in passing from the oceanic to the continental areas, makes the continental borders lines of weakness which determine in general the location of the great mountain wrinkles. In the two continents of North and South America, with their mountain borders on the east and west and their vast interior plains, Dana found exemplified the typical continent. Their isolated situation seemed to allow a more typical development than was possible in the Old World, where continents are massed together; as the laws of crystalline form can exhibit themselves in perfection only where a single crystal in a solution or magma is allowed to grow without interference of other growing crystals.

According to the views of Dana and

Le Conte, mountain ranges have been only exceptionally developed by geanticlinal uplifts of the earth's crust. A mountain range, in general, has its birth in a geosyncline—a downward folding of the crust, forming a trough, in which sedimentation goes on *pari passu* with the subsidence. At length, after long ages of subsidence and sedimentation, the strata in the trough are crushed together into alternate anticlines and synclines, or one part is forced over another in great thrust faults, while slaty cleavage and more decided metamorphism may be produced. The mountain range thus produced Dana called a 'synclinorium,' while he suggested the name 'anticlinorium' for a mountain range formed by a permanent geanticlinal elevation. Apparently the actual history of most mountain ranges is complex. The Appalachian range, for instance, was formed as a synclinorium at the close of the Paleozoic, degraded nearly to base-level in Mesozoic time, and again elevated by a broad geanticlinal movement early in Cenozoic time. It represents, therefore, in its complex history, Dana's two types of the synclinorium and the anticlinorium.

There are unquestionably weak points in the theory of mountain-making as developed by Dana and Le Conte; and, in our ignorance of the conditions in the interior of the earth, and of the forces there in action, it ill becomes us to be dogmatic; but the contractional theory seems worthy of provisional acceptance as the most plausible explanation of orogenic movements yet suggested.

III. GLACIAL GEOLOGY

Surely no part of the world affords better opportunity for the study of glacial geology than America. Its ice sheet, four million square miles in area, far exceeds any of the ice sheets of the Old World. The imbricated sheets of till in the Mississippi Valley afford clear evidence of the complex series of glacial and interglacial epochs. The driftless islands in the vast area of till, and the interlobate moraines, show the division of the marginal portion of the ice sheet into lobes determined by the topography. The old beaches along the shores of the Great Lakes, the living Niagara, and the various extinct Niagaras, record the stages of the melting of the ice. The Malaspina Glacier affords illustrations of the formation of eskers and of other phenomena which must have marked the stagnant margin of the waning ice sheet. Surely our country affords most favorable conditions for the study of the history of the Glacial period.

It is a curious fact that the first published suggestion of the agency of ice in connection with the drift came from a cotton manufacturer in Connecticut, Peter Dobson by name. In the *American Journal of Science,* in 1826, he gives a very clear and satisfactory description of the glaciated boulders observed in the drift, and gives as his conclusion: "I think we can not account for their appearances unless we call in the aid of ice along with water, and that they [the boulders] have been worn by being suspended and carried in ice, over rocks and earth, under water." It was his idea that these boulders were lifted from the bottom of the sea by sheets of anchor ice. This theory was certainly more satisfactory than most of the theories in vogue before that time, and more satisfactory than many of the opinions held at a later date.

The credit of the introduction and championship of the glacier theory of the drift belongs, not to a native, but to an adopted citizen of this country. Louis Agassiz came to this country in 1846. In his early home in Switzerland, he had already adopted the belief of Venetz and Charpentier in the former great extension of the Alpine glaciers, and their agency in the transportation of blocks from the Alps across the Swiss lowland to the Jura. It is, indeed, a curious fact that these Swiss geologists were anticipated, in the conception of the transportation of these blocks through the agency of glaciers, by Playfair, who suggested the idea in 1802. In the early papers of Agassiz the conception of the Glacial period took a form which he himself later recognized as an exaggeration. He conceived at first of a fall of temperature so wide-spread and so extreme that a polar ice cap extended southward over the whole breadth of Europe and across the Mediterranean, reaching the Atlas Mountains. Later he recognized the ice sheet that covered the Alps as entirely separate from the ice sheet of northern Europe. The tendency to an exaggerated view of the Glacial period overcame him again in later years, when he maintained that, at the climax of the Glacial period, there was 'floating ice under the equator, such as now exists on the coasts of Greenland.' Incidentally, he based upon this extravagant conception of the Glacial period an argument against Darwin's views of the origin of species, maintaining that the wide-spread cold of the Glacial period produced a general extermination of life, necessitating a new creation. But the extravagance of some of his conceptions, and his vain attempt to stop the resistless progress of the doctrine of evolution, may well be forgiven, in memory of the great service which he rendered in bringing into general acceptance the glacier theory of the drift.

As Agassiz traveled in various parts of his adopted country, he recognized everywhere in the northern states the traces of glaciation, already familiar to him in Switzerland and in Scotland; and his views found more ready acceptance in this country than in some of the countries of Europe. Guyot, who had been associated with Agassiz in the study of the glaciers of Switzerland, came to this country in 1848; and he was, of course, a strong ally in the defense of the glacier theory. As early as 1841 Edward Hitchcock had been so strongly impressed by the writings of Agassiz that he recognized clearly the traces of glaciers in Massachusetts; and, in his presidential address before the Association of American Geologists, and in the postscript to his 'Final Report on the Geology of Massachusetts,' he shows himself almost persuaded to adopt the glacier theory for the explanation of the drift in general. He could not, however, quite bring himself to the acceptance of the conception of a glacier capable of moving with so little slope as to be able to transport material southward over the whole of northeastern America; and he accordingly limited the action of glaciers to those cases in which the drift appeared to be dispersed somewhat radially from local centers in mountainous or hilly regions. The general southward movement of the drift seemed to him to require the conception of submergence of the land, and transportation by icebergs floating southward from the Arctic regions. As early as 1852, one of the best and most popular text-books of the time, that of Gray and Adams, gave the preference to the glacier theory of the drift. Dana, in his presidential address before the American Association for the Advancement of Science, in 1855, manifestly inclined to the glacier theory; and, in the first edition of his 'Manual of Geology,' in 1862, he gave his adhesion more decidedly to that view. The next generation of American students of geology were brought up on the various editions of Dana's 'Manual' and 'Text-book,' so that thenceforward the glacier theory was recognized in this country as orthodox. In some of the countries of Europe the theory of submergence and transportation of boulders by icebergs held the ground longer. In 1864 Lyell still maintained the submergence of the plain of northern Europe, and of the glaciated region of North America; and it was not until 1875 that Torell, in a memorable meeting of the German Geological Society, convinced the German geologists that the drift of northern Germany was transported by an ice sheet whose center was in the mountains of Scandinavia, and the name Diluvium, though still used in German geological writings, was completely emptied of its original connotation.

Within the last few decades the labors of a large number of earnest and able investigators have developed the glacier theory more in detail, and have added vastly to our knowledge of Quaternary history. The imaginary polar ice cap has given place to ice sheets of more limited dimensions, though still vast, developed respectively about the Laurentide, Keewatin and Cordilleran centers. The series of terminal moraines, marking stages of readvance or halts in the retreat of the ice sheet, have been carefully mapped. From the experience gained in the study of terminal moraines in this country, Lewis was enabled, in 1886, to recognize and interpret the terminal moraines of the ice sheet in England; and Salisbury, in the following year, those of north Germany. The recognition of interlobate moraines and of driftless areas led to a clearer understanding of the nature of the movement of the great ice sheet. The driftless area of Wisconsin was noticed by J. D. Whitney as early as 1862, and has been more carefully studied by Chamberlin and Salisbury; and the latter investigator has called attention to a smaller driftless area in Illinois. The Quaternary period, instead of being brief and comparatively simple, has been shown to be of long duration and great complexity. It has been analyzed into a succession of glacial and interglacial epochs; and, from the vast amount of erosion in some of the interglacial epochs, it has been inferred that post-Glacial time is very short in comparison with inter-Glacial time. The history of the series of lakes held between the front of the receding ice sheet and the southern water shed of the Saint Lawrence basin, has been studied by Gilbert, Taylor, Fairchild and others. In Chamberlin's theory that the cause of the Glacial climate is primarily the diminution of the amount of carbon dioxide in the atmosphere, and that the location of the main centers of glaciation is due to the path of cyclonic storms, we have a theory which, if it can not be accepted with full confidence as the true one, is at least the only theory of the Glacial climate which has not yet been weighed in the balance and found wanting. There seems still to be some disagreement among physicists on the critical question, what effect small changes in the amount of carbon dioxide in the atmosphere would produce upon the climate. The views of Arrhenius, upon which the theory of Chamberlin is based, have been contested by Angström and Very, and are not accepted by Hann. If the physicists can be brought to an agreement on this fundamental point, we may feel that we have, at last, a theory of the Glacial climate. The alternative at present seems to be the acceptance of Chamberlin's theory, or the confession that we have as yet no explanation of the Glacial climate.

IV. SUBAERIAL DENUDATION AND THE EVOLUTION OF DRAINAGE SYSTEMS

In early years the study of geology in this country was substantially confined to the region east of the Mississippi; but, in due season, the weird and fascinating region of the Cordillera revealed itself to explorers and geologists. It is now more than half a century since American geologists began the study of that western wonderland. The earliest geological work was done in connection with expeditions undertaken for other purposes, as the Pacific Railroad explorations, which commenced in 1853, and Ives's Colorado expedition. It is, however, forty years since the national government established geological surveys in that western country. A period of a dozen years commencing with 1867 was marked by the achievements of four great organizations devoted specifically to geological work—the Survey of the Fortieth Parallel, the Survey West of the One Hundredth Meridian, the Survey of the Territories and the Survey of the Rocky Mountain Region. Since 1879 all these organizations have been superseded by the United States Geological Survey. A new world for geologists was that weird western land—that land of deserts, plateaus and cañons, with vast stretches of almost horizontal stratification broken by faults and monoclines, revealing its geological structure with wondrous clearness by reason of an arid climate whereby it has been left naked and destitute of any mantle of soil and vegetation. There is revealed, as nowhere else in the world, the power and the method of subaerial denudation. The Grand Cañon of the Colorado is a stupendous object lesson of erosion; and, if one is compelled to recognize river erosion in the cañon itself, scarcely less strenuous is the compulsion to recognize subaerial denudation in the vast platform of Carboniferous strata with little outlying buttes of Permian and later formations. The study of that country has been fruitful in its contributions to the knowledge of aqueous agencies in dynamical geology. Very early appeared those three monumental works: Powell's 'Exploration of the Colorado River,' Gilbert's 'Geology of the Henry Mountains' and Dutton's 'Tertiary History of the Grand Cañon District.' The value of those early studies in a new field is not greatly lessened if some conclusions must be modified by later study. It may be that the course of the Green River through the Uinta Mountains is not a typical case of antecedent drainage; and it seems certain that the esplanade in the Kanab section of the Grand Cañon is not due to a long pause in the movement of elevation, in which the river nearly attained base-level: but the conceptions introduced into dynamical geology as the result of those early studies are no less valuable and fruitful.

The first lesson that geologists learned in that western land was the efficiency of subaerial denudation—the power of atmosphere, rain and river to remove vast quantities of material, and shape the topography of wide areas. From Hitchcock's 'Elementary Geology' I learned in my boyhood, in regard to cracks and fissures in the strata, "If the fissure is open and of considerable width, it is called a gorge; if it be still wider, with the sides sloping or rounded at the bottom, a valley is produced." Hitchcock was convinced, indeed, that the Niagara gorge was due to erosion; but he declared that the gorges of the Connecticut between Mount Tom and Mount Holyoke, and below Middletown, could not be due to the action of the river, and that the gorges of the same river at Bellows Falls and Brattleboro were too wide to have been formed by the river alone. In his 'Illustrations of Surface Geology,' in discussing the criteria by which to distinguish fluviatile from oceanic work, he declared, "Rivers have little power to form wide valleys." Apparently geologists were confused with a vague idea that the ocean is bigger than the rivers, and that, therefore, it can do more work in erosion. As early as 1847, Ramsay had recognized that the summits of the mountains of Wales were remnants of a former plain of erosion, but he conceived of it as a 'plain of marine denudation,' and that phrase holds its place to-day in English writings. With genuine British loyalty and conservatism, Sir Archibald Geikie retains the phrase even in the latest edition of his 'Text-book,' in explanation of table-lands of erosion; though, even in his earliest edition, he declared that, in the production of plains of marine denudation, "the sea has really had less to do than the meteoric agents. A plain of marine denudation is that sea-level to which a mass of land has been reduced mainly by the subaerial forces." He attributes to ocean waves and currents only 'the last touches in the long process of sculpturing.' Elsewhere Geikie bears emphatic testimony to the influence of American geologists, in the words: "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." It is, indeed, marvelous with what prophetic vision Hutton and Playfair conceived some of those ideas of river action which the geologic world in general learned only from the work of American geologists in the Cordilleran region. On this subject, those two Scotchmen were a half-century in advance of their time.

That western land has taught us not only to recognize the fact of subaerial denudation, but also to formulate its methods. In Powell's 'Exploration of the Colorado River,' he distinguished rivers as consequent, antecedent, and superimposed. Davis has carried the analysis somewhat further, giving us subsequent and obsequent rivers. Powell formulated the doctrine of base-levels; Davis has given the conception greater accuracy and consistency by distinguishing base-level from profile of equilibrium. The base-level of a district is a portion of the ideal spheroidal surface of the earth. Disregarding the curvature of the earth's surface, base-level may be represented in profile as a straight line; profile of equilibrium as a curve, concave upward, tangent to base-level at the mouth of the river, gradually approaching base-level with the progressive denudation of the country, but never quite reaching it except at the point of tangency. To Davis also we owe the full development of the conceptions of youth and age in river valleys and in drainage systems, and of cycles of erosion ending in the formation of peneplains. We have learned to search in every rugged mountain region for remnants of ancient peneplains. We have learned, in general, that geological history is to be read, not only in deposits, but also in erosion forms.

V. TERTIARY MAMMALS AND THE DOCTRINE OF EVOLUTION

Half a century ago the exploring expeditions connected with the Smithsonian Institution began to collect fossils from the Tertiary deposits of the western plains. Later the work was followed up by the Geological Surveys under the auspices of the national government, and by numerous private expeditions under the auspices of universities, scientific associations and individuals. Over those western plains were found to stretch vast continental deposits, certainly not all of lacustrine origin, as at first reported, but in part piedmont alluvial formations, in part eolian deposits, and, in limited areas, deposits of volcanic dust. These continental deposits of the western plains yielded in unparalleled richness mammalian fossils, which have been studied by Leidy, Marsh, Cope, Osborn, Scott, Wortman, and others. No other single series of discoveries has been so potent in changing the bearings of paleontology upon the doctrine of evolution.

In Darwin's two chapters on geology in the 'Origin of Species,' he marshaled with great skill the geological facts then known which appeared favorable to evolution. Yet he recognized in the facts of paleontology 'perhaps the most obvious and serious objection which can be urged against my theory.' He cited a long list of recognized authorities in geology and paleontology, still living or recently dead at the time of the publication of his first edition, who were believers in the immutability of species—Cuvier, Agassiz, Barrande, Pictet, Falconer, Forbes, Lyell, Murchison, Sedgwick. Of these Lyell alone lived to become a convert to evolution.

Of course the objection which Darwin felt so strongly himself, and which seemed conclusive to so many paleontologists at the time, was the absence of gradation between different forms. The theory of evolution, and especially the strictly Darwinian form of that theory, requires fine gradation between species—not indeed between different species now existing, but between existing species and species now extinct, and between fossil species of successive periods. In general, such gradations do not appear. Fossil species are about as sharply defined as recent ones; and whole groups of species—orders, classes, sub-kingdoms—have appeared without recognizable ancestry. Darwin's answer to this objection was given in the phrase now become classical, 'the imperfection of the geological record.'

In the half-century since the publication of Darwin's first edition, the attitude of paleontologists has completely changed. Not only is it true at present that paleontologists are substantially unanimous in accepting the doctrine of evolution; but it has come to be generally believed that the very science which afforded a half-century ago the strongest objection to evolution now affords its strongest support. This change is in large part due to the discoveries which have so shattered the objection that once appeared so strong. Innumerable links then missing have been brought to light. Intermediate forms between orders and classes formerly supposed to be widely separated from each other have been discovered in great abundance. Numerous series of genera may be traced through successive geological periods, exhibiting a gradually progressive change which almost irresistibly suggests to the mind the belief that the series are truly genetic. The fossils of our western plains have afforded a goodly share of the most important of these new evidences of evolution.

When the first edition of the 'Origin of Species' was published, the classes of birds and reptiles seemed to stand widely asunder. But in the very next year (1860) an odd feather of Archæopteryx was discovered, and a year later the skeleton now preserved in the British Museum. But Archæopteryx was a solitary representative of the birds of markedly reptilian character until the discovery of Ichthyornis and Hesperornis in the Cretaceous of Kansas, of which preliminary descriptions were published by Marsh in 1872. Both these remarkable types show reptilian affinities, in the possession of teeth, in the structure of the skull (though unhappily the palatal region is but imperfectly known), and in the pelvis; and Ichthyornis very notably in its slightly biconcave vertebræ, contrasting strongly with the saddle-shaped articulating surfaces of the vertebræ of modern birds. However strongly these genera suggest the idea of an evolutionary connection between reptiles and birds, their own place in the evolutionary series is not easy to determine. Ichthyornis may be in or near the direct line of descent from Archæopteryx to some such generalized dromæognathous type as is represented by those curious living fossils, the Crypturi, from which divergent lines of evolution may have led, on the one hand, to the ostriches and other flightless Dromæognathæ, and, on the other hand, to the Carinatæ. Hesperornis, a degenerate and in some ways highly specialized form, stands certainly at the end of a side branch, and has left no descendants.

But the discoveries of most evolutionary significance, as already intimated, have been among the Tertiary mammals. A number of series have been traced, leading from generalized types in the Eocene, through forms of gradually increasing specialization, to genera which still survive. The first of these genetic series to be brought to notice was the genealogy of the horse, as traced by Marsh in 1874. Marsh's views were adopted by Huxley in his brilliant 'American Lectures,' and thereby gained a larger share of public attention than they would otherwise have received. Probably no single fact or group of facts brought to light since the appearance of the 'Origin of Species' has been so influential in bringing the theory of evolution into general acceptance. The genealogy of the horse has been corrected in detail and completed by later investigations. The line of descent may now be traced through Hyracotherium and Eohippus of lower Eocene, Protorohippus and Orohippus of middle Eocene, Epihippus of upper Eocene, Mesohippus of Oligocene, Anchitherium of lower Miocene, Parahippus, Protohippus, and Pliohippus of middle and upper Miocene, to Equus of Pliocene and Quaternary; while side branches lead to Hipparion, Hippidium, and other forms which have died without issue.

A similar series, though with not quite so fine gradations, reveals the genealogy of the camel. From the Eocene Protylopus this line is traced, through Oligocene Poebrotherium, to Pliocene Procamelus, whence one branch leads to Camelus, and the other to the South American Auchenia. It is indeed remarkable that the characteristically old-world types, Equus and Camelus, should have been evolved in North America and have become extinct in this their original home. Another series, beginning in the lower Eocene Systemodon, ends in the modern tapirs.

In like manner, among the very primitive carnivores which have been classified as the order Creodonta, the ancestors respectively of the dog and the cat have been recognized in the Eocene genera Vulpavus and Palæonictis.

Of extraordinary interest in an evolutionary point of view is the most primitive Tertiary fauna from the Puerco beds discovered by Cope in 1880. In that fauna is found the culmination of the Multituberculata, which made their first appearance in Triassic time, and whose teeth reveal their close relation to the Monotremata. But, with those survivals from Mesozoic time, appear generalized and primitive forms of placental mammals, wherein may be traced the ancestry of mammalian groups of later Tertiary and recent time. Hemiganus and Psittacotherium may be recognized as the ancestors of the Edentata. And, among the most primitive and generalized ungulates, the Phenacodontidæ, Protogonodon has been recognized as possibly the ancestor of the Artiodactyla, and Euprotogonia with more probability as the ancestor of the Perissodactyla.

In the American Museum, where some of our sessions are to be held, the evidence of these genetic series running through the Tertiary is placed before our eyes with incomparable fullness. As we behold the unparalleled richness of those collections, we do not wonder that not even a call to be the official head of the army of science in this country could induce Professor Osborn to leave the museum which is at once the monument of his work in the past and the material for his work in the future.

But, however numerous are the gradational forms which have been brought to light, Darwin's principle of the imperfection of the geological record is in no wise superseded. It still remains true that the theory of evolution must stand or fall according to our judgment of the adequacy of that principle of Darwin. If the fossils accessible to observation and collected in our museums afford an approximately complete representation of the life that has existed in past ages, there is certainly no standing ground for any theory of evolution. But, while we have seen numerous chasms bridged by series of gradational forms, we have also come to a fuller appreciation of the significance of Darwin's principle of the imperfection of the geological record.

In the conclusion of Darwin's chapter on the subject, he used a striking illustration: "I look at the natural geological record as a history of the world imperfectly kept and written in a changing dialect. Of this history we possess the last volume alone, relating only to two or three countries. Of this volume, only here and there a short chapter has been preserved; and of each page, only here and there a few lines." In the light of our present knowledge of geological history, we are able to see that even this striking illustration fails to do full justice to the subject. The imperfection of the record consists not merely in the fact that some of the chapters are missing. It appears most strongly when we inquire just what chapters are missing. In the conception of continental history to which I have already referred, we have come to recognize a truth which, in somewhat distorted form, found expression in the old catastrophism. We have come to recognize that comparatively short periods of rapid geographical change alternate with long periods of relative stability or slowly progressive change. This is, in substance, the doctrine of critical periods as formulated by Le Conte. It is precisely in those critical periods that the record fails, and the gap is indicated by unconformability. Darwinians and Lamarckians alike must recognize that the periods of rapid geographical change must be the periods of most rapid change in fauna and flora. Evolutionary change must be directly or indirectly the result of a failure of adjustment between organism and environment.

The doctrine of critical periods has taken somewhat more definite form in Chamberlin's discussion of the effects of intermittent subsidence of the ocean bottom. The critical periods in geological history are the times when the rigidity of the crust yields to accumulating strain, when the ocean bottom subsides, when the continents emerge to larger area and higher altitude, when more or less of mountain-making takes place, and when the geographical changes bring in their train the diminution of the atmospheric supply of carbon dioxide and a tendency to cold and arid climates. Then come the long periods in which the continents are slowly denuded, the continental shelves are extended landward by encroachment of the sea and seaward by sedimentation, the quota of carbon dioxide is slowly replenished, and the fauna and flora which had been impoverished gradually expand to their former luxuriance. The chapters which are lost from the record are precisely the chapters which would contain the story of those critical periods, marked by extinction of manifold species, and by rapid change in adjustment to new and more rigorous conditions. The geological record of the progress of life is like a history of the United States, in which, among other less important chapters, the chapters on the Revolution and the Civil War are lost.

CONCLUSION

I have not attempted to give a history of geological investigation in this country. Of the great number of earnest and able investigators whose names illustrate the scientific history of this country—of those who have finished their work, but whose memory and influence can never die—of those still living whose achievements in the past are only the promise of greater work in the future—I have named but few, though many others are equally worthy. Of the men whose names I have mentioned, I have doubtless not in all cases mentioned the work which has been most meritorious or important. I have mentioned only those investigations which have a bearing on a few special subjects. Nor have I referred, except occasionally and incidentally, to the work of European students which has gone on parallel with that of students in our own country. American geologists have had no patent rights giving them a monopoly of any particular department of investigation. The limited time of such an address as the present renders impossible a critical discussion of the precise share in the study of the various subjects which belongs to American geologists. But I believe it may be fairly claimed that on the five subjects which I have discussed—the permanence of continents, the theory of mountain-making, the history of the Glacial period, the laws of subaerial denudation, the evolution of mammalian life—the work of American geologists has been relatively so important that the results deserve recognition as, *par excellence*, THE CONTRIBUTIONS OF AMERICA TO GEOLOGY.

WILLIAM NORTH RICE
WESLEYAN UNIVERSITY

SCIENTIFIC BOOKS

The Development of Symbolic Logic: A Critical-Historical Study of the Logical Calculus. By A. T. SHEARMAN, M.A. London, Williams and Norgate. 1906.

As the subtitle indicates, the author has attempted a history of symbolic logic accompanied by a critical examination and estimate of the various systems as they may have contributed severally to the discipline from its earliest stages to the present time. He claims that in spite of the great variety of systems and methods there is clearly to be recognized but one logical calculus, and that the unity among the various symbolists abundantly compensates for the obvious differences. While this is true it should not be overlooked, however, that the progress of symbolic method has been retarded owing to the lack of a common symbolism such as we find in mathematics. The variety and the multiplicity of symbolical representation is, in my opinion, a serious defect. It is not merely that different writers are using different methods of symbolism—that in itself is sufficiently confusing—but also that any new operation is apt to give rise to some entirely new form of symbolism which might be represented equally as well by some new combination or new manipulation of the existing symbols already at hand. Within the scope of a few elementary symbols an indefinite range of differing processes and devices is possible, just as in mathematics the symbols used are exceedingly few—but they lend themselves easily and adequately to the exact expression of an innumerable array of operations and processes. The desideratum in a symbolic logic is, therefore, twofold: a common and a simplified symbolism. The simplicity of the symbolism of Leibniz, the founder of symbolic logic, is most striking; but the drift has been from this characteristic simplicity towards increasing difference and complexity. The author, by the way, does not give Leibniz his full due as the founder of the symbolic logic. Mr. Shearman insists that Boole is to have the complete credit of this on the ground that Boole worked independently and without any knowledge of the early work of Leibniz. The latter assumption seems